A Test of Mechanical Ambiguity*

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Abstract

We implement an experiment to elicit subjects' ambiguity attitudes in the spirit of Ellsberg's three-urn problem. The procedure includes three design elements that (together) have not been featured in similar experiments: Strict ambiguity preferences, a single decision, and a mechanical randomization device with an unknown distribution (to both subjects and experimenters). We use this device in order to eliminate possible 'strategic' ambiguity related to subjects' beliefs about the experimenters' motivations. In addition, we survey 39 experimental studies on Ellsberg's two- and three-urn problems, and find that, on average, slightly more than half of subjects are classified as ambiguity averse. Our results, with our new design, fall on the low end of the range of results in the surveyed studies.

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1 Introduction

There are many situations where decision makers must contend with natural phenomena which are not well understood. Take for example the first man to ever eat a mushroom. We might now know if the mushroom in question is toxic, and, if so, the probability of various reactions. This man, on the other hand, would have known nothing of the sort. Similarly, consider the first men to plant non-native crops in various parts of the world, chemists experimenting with new compounds, or early explorers on uncharted oceans. Each deals with what economists generally refer to as ambiguity. Further, the ambiguity in these instances stems primarily from acts of nature, with no discernible strategic interactions with other people.

In order to better understand the decision-making process in these situations we propose and conduct a lab experiment designed to approximate these types of situations. While other experiments have looked at ambiguity, the source nearly always stems from some conscious decision maker. We instead take steps to ensure that the ambiguity stems from a natural process, which not even we as experimenters fully understand. In order to maintain comparability to the existing literature in terms of results, we utilize a three-color Ellsberg urn with one risky and two ambiguous colors.

In order to gain clean evidence on the effects of ambiguity from natural phenomena, we incorporate three design features, whose combination has not before been implemented. The first is to differentiate between natural and strategic sources of ambiguity by constructing a mechanical randomization device with an unknown distribution (to both subjects and experimenters). The second is to eliminate the possibility of ambiguity "hedging" by sophisticated subjects. The last is to avoid mischaracterization due to indifference.

Upon implementing our design, we find that subjects are averse, in roughly equal proportions, to ambiguity from both sources. With our design this proportion seems to be around 40%. We also find that roughly 25% of subjects make ambiguity seeking choices, though further investigation of their reasoning allows for a different interpretation of this statistic. We additionally find high correlation between subjects' reasoning and their choices. In particular, subjects who emphasized the safety of the risky option (as compared to an ambiguous option) were 50% more likely to choose the risky option. Meanwhile, subjects whose reasoning was congruent with the principle of insufficient reason were 35% more likely to choose the option suggested by that principle.

To put our results into perspective, in the next section we survey 39 experimental studies on the Ellsberg urn problem and find that on average slightly more than half of subjects were classified as ambiguity averse. With our new design features, we can roughly confirm this result but find that with our new design probably slightly less than half should be classified as ambiguity averse. Section 3 discusses in detail the choices we made with respect to the experimental design and procedures. Results are analyzed and discussed in Section 4. Finally, we close with a brief discussion of the implications of our findings in Section 5.

2 A survey of the experimental literature

Since Ellsberg's (1961) famous thought experiment there have been numerous laboratory experiments that implemented either the 2-color or the 3-color Ellsberg urn. Camerer and Weber (1992) provide a survey of the early literature. Since in recent years the experimental literature seems to have exploded, an updated survey may be valuable. Trautmann and van de Kuilen (2013) provide an excellent survey focussing on the premium that subjects are willing to pay in order to avoid ambiguity. In Table 1 we provide a survey of 39 experimental studies focussing on the percentage of subjects that can be classified as ambiguity averse.

We have collected all studies we could find that implemented a classical Ellsberg urn experiment and where we were able to infer the percentage of ambiguity averse subjects from data presented in the paper (column five in Table 1). Whenever possible, we chose the treatment that was closest to the classical Ellsberg experiment.

The second column of Table 1 notes whether it was the 2-color or the 3-color version. Column three indicates whether the experiment was designed to elicit a strict preference regarding ambiguity (e.g. by eliciting an ambiguity premium or by paying a higher prize for the ambiguous lottery). The fourth column indicates whether the experiment involved multiple decision for the same subject (which often allows for some form of ambiguity hedging).

Table 1 shows that the range of the percentage of ambiguity averse subjects is large (from 8% to 93%). If we naively take a simple average of (the midpoints of the ranges) of all studies, we obtain a mean of 57.16 and a median of 59.5. If we discard the 5 most extreme studies on either side, the mean is almost unchanged at 57.86. If we only include studies that check for a strict preference towards ambiguity, we obtain an average of 51%. Thus, overall, a quite consistent picture emerges: Slightly more than half of subjects seem to be ambiguity averse.

Table 1: **Previous Ellsberg experiments**

Study	# of colors	strict pref.	mult. dec.	% averse	other comments
Akay et al. (2011)	2	yes	yes	57	
Becker, Brownson (1964)	2	yes	yes*	47	
Binmore et al. (2012)	3	yes	yes	45-64	
Butler et al. (2013a)	2	yes	no*	52	
Butler et al. (2013b)	2	no	yes	35	
Cettolin, Riedl (2010)	2	no	yes	50	
Charness et al. (2013)	3	yes	yes	8-24	Versions 1&2
Chew et al. (2012)	2	yes	yes	49.4	
Chew, Miao, Zhong (2013)	2	yes	yes	62	50 vs. [0,100]
Cohen et al. (1985)	2	yes	yes	59	
Cohen et al. (1987)	2	yes	yes	58	
Curley et al. (1986)	2	yes	yes	65	Experiment I
Dimmock et al. (2012)	2	no	yes	68	
Dimmock et al. (2013)	2	no	yes	51	
Dominiak et al. (2012)	3	yes	yes	54	
Dominiak, Duersch (2013)	3	yes	yes	23-48	
Dominiak, Schnedler (2011)	2	yes	yes	55	all subjects
Eichberger et al. (2013)	2	no	no	63	situation O
Einhorn, Hogarth (1986)	2	no	yes	47	MBA students
Eliaz, Ortoleva (2012)	3	no	yes	76	
Fox, Tversky (1995)	3	no	yes	62	Study 3
Gazzale et al. (2013)	2	no	yes	93	
Halevy (2007)	2	yes	yes	61	
Keck et al. (2012)	2	no	yes	60	$p = 0.50, \delta = 0.50$
Keren, Gerritsen (1999)	3	no	yes	68-79	
Kuehberger, Perner (2003)	2	no	yes	75	Graphical estimate
Kuzmics (2013)	2	yes	yes	55	$AA + AC$
Lauriola, Levin (2001)	2	no	yes	62-72	$p = .50$
MacCrimmon, Larsson (1979)	3	yes	yes	79	70% reported in text?
Mafioletti, Santoni (2005)	2	yes	yes	36	
Ross et al. (2012)	2	yes	yes	27	strong aversion
Rubinstein (2012)	3	no	yes*	49	
Slovic, Tversky (1974)	3	no	yes	51-78	averse reasoning
Stahl (2012)	3	yes	yes	35-39	12$ or 6$, resp.
Trautmann et al. (2008)	2	yes	yes	50-79	Experiment I
Trautmann et al. (2009)	2	no	yes	63	choice
Trautmann et al. (2011)	2	no	yes	63	Experiment I
Vinogradov, Shadrina (2013)	2	no	yes*	59-74	
Yates, Zukovsky (1976)	2	no	yes	78	G vs. G''

Notes: *non-monetary incentives.

3 Experimental design

In the following three subsections we will discuss the three crucial features of our experimental design.

3.1 Strategic vs. Mechanical Ambiguity

Ambiguity can have two different types depending on its source. One type, which we shall call Mechanical Ambiguity, is the ambiguity about the distribution of a variable determined by some natural process. The second type, which we shall call Strategic Ambiguity, is the ambiguity about the distribution of a random variable determined by the conscious decision of some other person.[1]

In a standard Ellsberg-urn experiment, subjects may perceive their decision problem as a game between whoever fills the urn and themselves. For example, subjects may believe that the experimenter is trying to trick them in order to protect his research budget.[2] Therefore, it can be said that these experiments are technically a case of strategic ambiguity. While strategic ambiguity is certainly important to study, as much ambiguity in life comes from conscious decision, it is not clear that results in these experiments can be applied to cases where ambiguity comes from unknown natural processes.

On the one hand, mathematically, the source of ambiguity makes no difference. On the other, the psychological source of ambiguity aversion is not well understood, and there are reasons to believe that the source of ambiguity may affect someone's aversion to it. In particular, failure in the face of strategic ambiguity may induce a feeling of inferiority that would not be present in the face of mechanical ambiguity. Similarly, success in the face of strategic ambiguity might induce analogous feelings of superiority.

We set out to answer these questions with an experiment using a new procedure for generating an ambiguous distribution that is mechanical in nature. The procedure involves creating an irregular Galton box. A Galton box (also called a bean machine) is a box with interleaved pins arranged in such a way that when a ball is dropped in the top of the box, it ricochets off the various pins and into a number of collection bins. In a regular Galton box, the pins are arranged in such a way that the balls fall into the various bins according to an approximately normal distribution. In an

[1] Thus, the two types of ambiguity differ in a manner similar to the difference between nodes of game trees where players move, and nodes where nature moves.

[2] This concern has been raised in a number of experimental papers (e.g. Keren and Gerritsen, 1999; Charness et al. 2013, Dominiak and Duersch, 2013).

irregular Galton box, the distribution is irregular. Our goal is for this distribution to be ambiguous.

In order to test mechanical ambiguity, it is not enough to create an ambiguous distribution for subjects. We need the distribution to be ambiguous to the experimenters as well. To this end, we construct the frame of a Galton box before the experiment, but arrange the pins and complete construction in the lab, with the assistance of the subjects. Specifically, we have subjects hammer nails into a plywood board before revealing anything else about the experiment. We then attach the plywood board to the frame and reveal its purpose.

We reveal that we will drop a number of bouncy rubber super-balls through the Galton box, where they will make their way into black and white bins. For each ball that falls into a white (black) bin we add a yellow (blue) marble to a three-color Ellsberg urn. Using this procedure, we create a randomizing device with a distribution that is ambiguous to both experimenters and subjects.

Others have attempted to test some form of natural ambiguity before (Butler, 2013b; Mafioletti and Santoni, 2005; Keller et al. 2007; Kilka and Weber, 2001; and Fox and Tversky, 1995, to name a few). These studies use natural phenomena such as the weather or stock markets to generate bets with ambiguous winning probabilities. Although when encountered in nature these phenomena would exhibit mechanical ambiguity, in the lab it is not as clear. If we expect a subject to be suspicious of an experimenter filling an urn, it is possible that similar suspicions may arise relating to the formulation of these bets. With respect to weather, for example, a plethora of data has been gathered and is easily searchable. Thus, the winning probability of a bet on the temperature in some city on a given day could be estimated fairly accurately beforehand. This allows the strategic motivations of the experimenter to come into play. A second reason, why we prefer to use the Galton box, is that making payments dependent on future events would necessarily delay payments thereby confounding issues of ambiguity with issues of discounting and time preferences.

3.2 Single vs. multiple decisions

We ask each participant to make a single decision. Although this increases our cost for data, standard multiple-decision designs would not meet our needs. In particular, even a mechanism in which one decision is randomly selected for payment[3] can change

[3] This mechanism has been called pay-one decison-randomly (POR), random decision mechanism, or random lottery incentive mechanism. See Cox et al. (2011) and Azriely et al. (2012) for

the nature of the ambiguity faced by subjects. Specifically, if subjects view the decisions as related, then they would view the experiment as a grand lottery. The different decisions could be interpreted as revealing subjects' preferences of grand lotteries with different ambiguity properties. This would greatly complicate the analysis.

Table 2: Ellsberg 3-color experiment

	ball drawn		
bet (act)	green	blue	yellow
G	\$1	0	0
B	0	\$1	0
$\neg G$	0	\$1	\$1
$\neg B$	\$1	0	\$1

To see this, consider the example of a typical three-color Ellsberg urn experiment with 30 balls. Subjects know that there are 10 green balls and that the remaining 20 balls can be either blue or yellow. Typically, subjects are now asked to make a decision between betting on green (G) and betting on, say, blue (B), where the "act" G means that a subject receives 1\$ if a green ball is drawn and 0 otherwise (see Table 2). Then subjects are asked to make a second decision, namely betting on "not green" ($\neg G$) or on "not blue" ($\neg B$). The standard choice of ambiguity averse subjects would be $(G, \neg G)$, i.e. betting on "green" in the first decision and on "not green" in the second.

Indeed, viewed separately, betting on green is the only unambiguous option for the first decision, and betting on "not green" is the only unambiguous option for the second decision. However, viewed together, betting for and against the same color, no matter which of the colors, produces an unambiguous expected value. As an extreme case suppose first that subjects believe that only one ball is drawn, the color of this ball is relevant for both bets, and both bets are being paid out. Then, the combination of bets $(G, \neg G)$ yields a guaranteed fixed payment of 1\$. But so does the bet $(B, \neg B)$.

A well designed ambiguity experiment would of course avoid this mistake. So let us suppose that two independent draws are made from the urn and that one of the two decisions is selected randomly (e.g. by a fair coin) for payment. However, this procedure still does not solve the problem.[4] In the appendix, we show formally

discussions of the incentives properties of this mechanism.

[4]This is also shown by Azriely et al. (2012) who show that POR is not incentive compatible

that a decision maker, who follows the MaxMin Expected Utility (MEU) approach by Gilboa and Schmeidler (1989) and who considers the two draws from the urn as independent, should be indifferent between the combination of bets $(G, \neg G)$ and $(B, \neg B)$, or $(Y, \neg Y)$. To avoid these problems, we ask each subject to make only one decision.

Further, this one decision is between a bet on green and *one* of the two ambiguous colors, where we randomly assign subjects to choosing either between green and blue or between green and yellow. Some experiments (see e.g. Halevy, 2007) have allowed subjects to choose the color they would like to bet on. This is a very reasonable design choice if one wants to alleviate subjects concern about the experimenter trying to trick them.[5] However, as argued already by Raiffa (1961) and more recently by Kuzmics (2013), it allows subject to create their own grand lottery by "flipping a coin in their head" to choose between the two ambiguous colors to create an unambiguous expected value.

3.3 Dealing with indifference

Anyone who employs the principle of insufficient reason in a typical Ellsberg experiment as described above would be indifferent between betting on any color because such a person would believe that there is an equal number of blue and yellow balls and hence 10 of each color. The phenomenon of ambiguity would not be very relevant if it were restricted to such cases as it may be quite forgivable, even for a Bayesian, to use the fact that the number of green balls is known as a tie-breaker. For this reason, a number of papers have explored whether subjects actually have a strict aversion to ambiguity (see e.g. Cohen et al., 1985; Curley et al., 1986; Einhorn and Hogarth, 1986; Curley and Yates, 1989, Charness et al., 2013).

We chose to address this problem by having 9 of 30 (or 11 of 30, depending on the treatment) green balls in an urn. This way, a decision maker who employs the principle of insufficient reason will never be indifferent.

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in the context of ambiguity if the order reversal axiom (see e.g. Seo, 2009) holds. In our thought experiment, this axiom essentially requires that decision makers do not care whether the balls are drawn first or the coin is tossed first.

[5] We chose instead to address this issue of strategic ambiguity with our machine.

3.4 Experimental procedures

We used a three-color Ellsberg urn with 30 marbles,[6] which were either green, yellow, or blue. The number of green marbles was shown to be 9 (or 11 depending on the treatment).[7] The number of yellow and blue marbles was determined by a newly designed machine. To isolate mechanical ambiguity, we needed to make sure that no one, not even the experimenter could know beforehand the distribution of marbles in the urn. We achieved this by constructing the aforementioned irregular Galton box for bouncy balls, which consists of a vertical board with interleaved rows of nails. To make it transparent to subjects that the experimenters could not have tested the machine before the experiment, the nails were hammered into the board by the subjects themselves at the beginning of the experiment. The timing was as follows:

1. Without knowing anything about the experiment, subjects were asked to volunteer for hammering nails into a plywood board (see Figure 1, left panel).[8] Black rectangles on the board indicated the areas where a nail had to go but left enough choice for subjects to find a location for their nails. No subject hammered more than two nails.

2. The rest of the machine was brought into the room and the board with the nails was affixed as the back wall of the machine (see Figure 1, right panel).

3. Subjects were then told that at the end of the experiment one of them would drop 21 (or 19, depending on the treatment) bouncy balls into an opening at the top of the machine. Balls would bounce left and right as they hit the nails. Eventually, they would be collected into different bags at the bottom. There were an equal number of white and black bags with alternating order. The total number of balls in the white bags would then determine the number of yellow marbles that would be put into the urn, while balls in black bags would determine the number of blue marbles.[9]

4. Before the balls were dropped into the machine, subjects had to make one and only one decision. Some were asked to bet either on green (with a known

[6] From here on we use "marbles" to refer to objects in the Ellsburg urn, while "balls" will denote bouncy rubber super-balls used in our Galton box.

[7] Subjects were allowed to verify the bags' contents after the experiment and some did.

[8] For each of our three sessions a new but identical board was used.

[9] For a short video of the machine in action see
`http://www.uni-heidelberg.de/md/awi/professuren/with2/video.mp4`

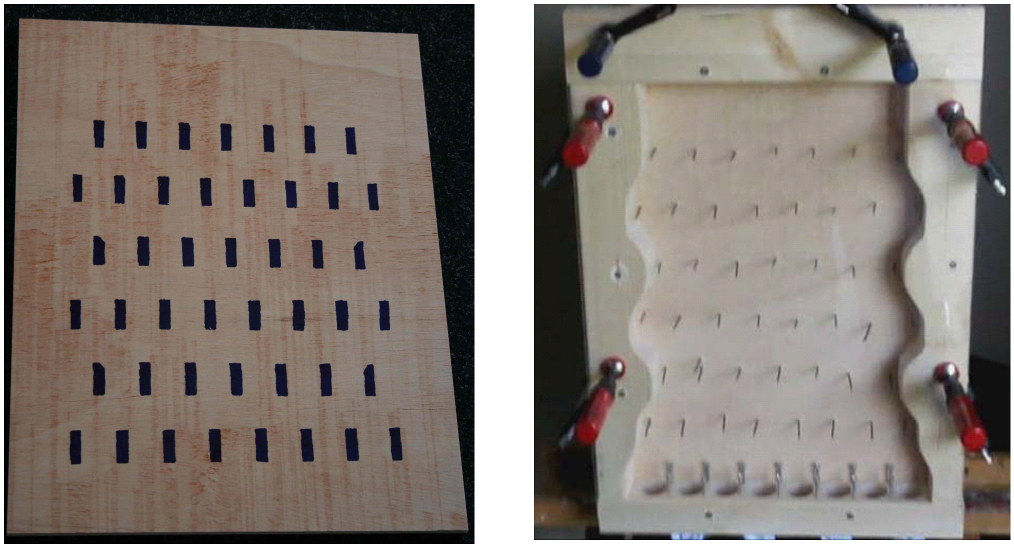

Figure 1: Left panel: The back of the plywood board with areas for nails marked in black. Right panel: The Galton machine assembled.

number of marbles) or on yellow, the others were asked to bet either on green or blue. If their color was drawn, a subject would win 6 euro.

5. After the decision sheets were collected, subjects were asked to fill in a questionnaire, which asked for their gender and for their expectation with respect to the number of green (as a check whether they understood the instructions), yellow, and blue marbles. They were also asked to explain in a few words how they came to their decision. Finally, there was a question whether they believed that the experimenters could have manipulated the distribution of balls.[10] The questionnaire was unpaid.

6. Finally, one subject would draw a marble from the urn containing 30 marbles.

The experiment was run in 2012 as a classroom experiment with paper and pencil. We chose a classroom experiment since, for reasons explained above, we wanted to run a one-shot experiment where subjects would just make a single decision. To keep opportunity costs of subjects low, we chose to run the experiment at the end of first- or second-year undergraduate economics courses at the Universities of Heidelberg and Mannheim.[11] Since subjects were already present, their opportunity costs of

[10] In the control treatment C9 (explained below) we asked whether the distribution of marbles could have been manipulated.

[11] Students in Heidelberg and Mannheim (located about 25 km from each other) are likely to be similar in many respects. We chose to add subjects from Mannheim for the control treatment since

participating were lower than usual. All subjects received a show-up fee of 4 euro. The instructions (see appendix) were distributed on paper. The experiments lasted about 30 min. By design, the earnings were either 10 or 4 euro. The average payment was 6.47 euro.

The experimental treatments were as follows ("M" stands for "mechanical", "C" for "control"):

- Treatment M9: The known number of green marbles was 9. The machine was used in the classroom to determine the remaining marbles (121 subjects participated).
- Treatment M11: The known number of green marbles was 11. The machine was used in the classroom to determine the remaining marbles (63 subjects).
- Treatment C9: The known number of green marbles was 9. The machine was used privately beforehand without subjects seeing it. Subjects were simply told that "it is unknown how many of the marbles are yellow and how many are blue." (53 subjects).

The control treatment C9 is the classical 3-color Ellsberg urn experiment as it has been performed many time with the exception that the percentage of known (green) marbles was not 1/3 but rather 9/30. We chose this design to make sure that subjective expected utility maximizers who have a belief (e.g. because of the principle of insufficient reason) that the two unknown colors blue and yellow are equally likely would have a strict incentive to bet on the unknown color. This way we can (conservatively) classify all those subjects who choose the known color green as ambiguity averse. Treatment M9 is the same as C9 except that a possible strategic aspect of ambiguity, namely that subjects believe that they play a game against the experimenter, should be eliminated through the use of the machine. Finally, treatment M11 was designed to classify subjects as ambiguity loving. All expected utility maximizers who have a belief that the two unknown colors blue and yellow are equally likely would have a strict incentive to bet on the known color green.[12]

we had already approached all first- and second-year economics students in Heidelberg. Students from higher years or non-economics students would likely have been more different from our original sample.

[12] We hope to identify subjective expected utility maximizers with an asymmetric prior (e.g. those that believe that there are many more blue marbles in the urn than yellow ones) with the help of our questionnaire. In fact, there were few cases like this in the data.

4 Experimental results

Before coming to our main results, we check whether the colors of the marbles the subjects could choose from had any effects. We can test for this by comparing groups of subjects who had the choice between green and blue to those who had the choice between green and yellow. Table 7 in the appendix shows the distribution of subjects choice depending on their options (green vs. blue or green vs. yellow).[13] Fisher's exact tests show that there is no significant difference according to subjects' options (p-value $= 0.47$ for M9 and $p = 1$ for B9). Thus, from now on we pool these data.

Table 3 presents our main results, the percentage of subjects choosing to bet on color "green". Recall that in treatments B9 and M9 we interpret this percentage as a lower bound on the percentage of ambiguity averse subjects.[14] In both of these treatments we find that the percentage of ambiguity averse subjects is around 40%. The difference between the treatments is not significant according to a χ^2-test (two-sided $p = 0.58$). Thus, interestingly, the strategic aspect of ambiguity aversion does not seem to be important. About the same share of subjects is ambiguity averse in both treatments.

Result 1 *The frequency of ambiguity averse subjects is about 40%. There does not seem to be an increase in ambiguity aversion due to strategic interaction with the experimenter.*

Table 3: Percentage of subjects choosing the different bets

	treatment		
	B9	M9	M11
bet on green	37.7%	45.5%	76.2%
# of observations	53	121	63

It is tempting to interpret the percentage of subjects in treatment M11 who did *not* choose to bet on green as the percentage of ambiguity loving subjects, which would be quite high at 23.8%. However, the questionnaire data may help to put this number into perspective (see Table 4). Of the 12 subjects in M11 who chose to bet on blue, 4 explicitly say in the questionnaire that they thought that a blue

[13] Due to an error by the experimenters this comparison can only be done for treatments M9 and B9. In M11 always the same option (blue vs. green) was used.

[14] It is a lower bound since there may be subjects who are mildly ambiguity averse but not so much that this compensates for the fact that the probability of green is less than 1/3.

ball was more likely to be drawn. Thus, their behavior would also be compatible with subjective expected utility maximization if their prior of blue is high. All of the remaining 8 subjects report that they have made their decision either randomly or based on “intuition”. Not a single subject expressed a preference for ambiguity (although this may be asking for too much).

Table 4: The reasons subjects gave for their decisons

treatment	reasoning	bets	
		green	not green
M11	principle of insufficient reason	24	0
	green is safer	8	0
	blue more likely	0	4
	other	12	8
M9	principle of insufficient reason	5	35
	green is safer	26	1
	blue more likely	0	4
	yellow more likely	0	4
	other	13	21
B9	principle of insufficient reason	0	25
	green is safer	16	0
	yellow more likely	0	1
	other	3	7

Note: The table is based on classifying answers to the question “Why have you decided the way you have?”. Category “other” consists mainly of subjects stating that they decided randomly or ”by intuition”.

In general, Table 4 shows a remarkable consistency of subjects decision and their reasonings. A large majority of ambiguity averse subjects (those choosing green in M9 and B9) cite the belief that “green is safer”. In all treatments, between 30% and 50% of subjects cited the belief that the unknown colors are equally frequent or likely, which is compatible with the principle of insufficient reason, although few if any mentioned the principle. Almost all of these subject chose their bets accordingly (i.e. green in M11, and not green in M9 and B9).

To study this relationship further, we present in Table 5 in column (1) a logit regression for the probability of choosing to bet on green. The explanatory variables relate to the subject’s reasoning. The variable “green is safer” is a dummy that equals one if a subject’s reasoning focussed on the safety of the green bet and 0 otherwise. The variable “principle of insufficient reason” was the product of a corresponding reasoning dummy multiplied by either 1 or -1 depending on the treatment (1 for

M9 and B9, and -1 for M11). Furthermore, we include a gender dummy and a dummy for subjects' belief (as indicated on the questionnaire) that the experiment could not be manipulated.

Both reasoning variables are highly significant and have the expected sign. Subjects who have beliefs consistent with the principle of insufficient reason are 35% more likely to choose the option predicted by that principle. Subjects who have the belief that green is safer are 50% more likely to choose green. Neither gender nor the belief with respect to the possibility of manipulation seems to matter.

Table 5: Logit regressions: probability of choosing green

	(1)	(2)
principle of insufficient reason	$-.35$ (.04)***	-
green is safer	.50 (.12)***	-
male	$-.05$ (.05)	$-.10$ (.07)
no manipulation	$-.03$ (.04)	$-.07$ (.07)
option blue	-	.05 (.07)
M11	-	.29 (.08)***
B9	-	$-.06$ (.08)
pseudo R^2	.45	.08
$\log L$	-90.71	-151.37

Note: Reported are marginal effects with robust standard errors in parentheses. A constant term is included. $n = 237$, *** significant at the 1% level.

Regression (2) in Table 5 reports results from a similar regression where the reasoning variables are replaced by treatment dummies and a dummy for the option a subject had (blue vs. green or yellow vs. green).[15] Here only the treatment dummy for M11 is significant confirming the above non-parametric test.

5 Conclusion

Our results suggest that a significant portion of society has an aversion to mechanical ambiguity. Further, we do not find a significant difference between the proportion who are averse to mechanical ambiguity, and the proportion averse to strategic ambiguity. These results suggest that the psychological underpinnings of ambiguity aversion may be primarily non-strategic in nature. This conclusion is bolstered by

[15] The option variables and treatment dummies are not included in regression (1) because they are closely correlated with the reasoning variables.

our survey data that shows that similar choices in strategic and mechanical treatments were supported by similar reasoning processes. In particular, ambiguity neutral choices were often backed by the principle of insufficient reason, while ambiguity averse decisions seemed to be supported by the relative "safety" of the risky choice.

Further, our three design features (the machine, a single decision, and strict preferences) ensure that what we observe is as close to natural ambiguity as has been seen in the lab. This is important not only for the applicability of our results, but, to an extent, the broader ambiguity literature as well. Our observed levels of ambiguity aversion are in line with much of the existing literature but slightly lower than what has been found on average previously. Thus, the possible confounds that we control for seem to have biased previous results slightly upwards. Overall, however, the differences are not too large: even when isolating mechanical ambiguity and controlling for ambiguity hedging and indifference, we find that around 40% of subjects seem to be ambiguity averse.

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Appendix A: Vanishing Ambiguity

Consider the bets described in Table 2 and suppose that two independent draws are made from the urn and that one of the two decisions is selected randomly (e.g. by a fair coin) for payment. In this appendix, we show formally that a decision maker, who follows the MaxMin Expected Utility (MEU) approach by Gilboa and Schmeidler (1989) and who considers the two draws from the urn and the coin toss as independent, should be indifferent between the combination of bets $(G, \neg G)$ and $(B, \neg B)$, or $(Y, \neg Y)$.

A state in the experiment is described by a triplet listing the colors (g, b, or y) of the two balls drawn from the urn and the result of the coin toss determining whether the first or the second decisions are being paid out (1 or 2). Thus, in total there are the 18 states, $S = \{s_1, ..., s_{18}\}$, listed in Table 6. For example, we denote state s_2 by $by1$ because in this state, ball b was drawn for the first decision, ball y was drawn for the second decision, and the coin decided that the first decision was paid out.

Table 6: States, bets, and probabilities

S		$(B, \neg B)$	$(G, \neg G)$	probabilities
s_1	$bb1$	1	0	$\pi_1(p,q) = p^2\frac{1}{2}$
s_2	$by1$	1	0	$\pi_2(p,q) = pq\frac{1}{2}$
s_3	$bg1$	1	0	$\pi_3(p,q) = p\frac{1}{3}\frac{1}{2}$
s_4	$gg1$	0	1	$\pi_4(p,q) = \frac{1}{3}\frac{1}{3}\frac{1}{2}$
s_5	$gb1$	0	1	$\pi_5(p,q) = \frac{1}{3}p\frac{1}{2}$
s_6	$gy1$	0	1	$\pi_6(p,q) = \frac{1}{3}q\frac{1}{2}$
s_7	$yb1$	0	0	$\pi_7(p,q) = qp\frac{1}{2}$
s_8	$yy1$	0	0	$\pi_8(p,q) = q^2\frac{1}{2}$
s_9	$yg1$	0	0	$\pi_9(p,q) = q\frac{1}{3}\frac{1}{2}$
s_{10}	$bb2$	0	1	$\pi_{10}(p,q) = p^2\frac{1}{2}$
s_{11}	$by2$	1	1	$\pi_{11}(p,q) = pq\frac{1}{2}$
s_{12}	$bg2$	1	0	$\pi_{12}(p,q) = p\frac{1}{3}\frac{1}{2}$
s_{13}	$gg2$	1	0	$\pi_{13}(p,q) = \frac{1}{3}\frac{1}{3}\frac{1}{2}$
s_{14}	$gb2$	0	1	$\pi_{14}(p,q) = \frac{1}{3}p\frac{1}{2}$
s_{15}	$gy2$	1	1	$\pi_{15}(p,q) = pq\frac{1}{2}$
s_{16}	$yb2$	0	1	$\pi_{16}(p,q) = q\frac{1}{3}\frac{1}{2}$
s_{17}	$yg2$	1	0	$\pi_{17}(p,q) = q\frac{1}{3}\frac{1}{2}$
s_{18}	$yy2$	1	1	$\pi_{18}(p,q) = q^2\frac{1}{2}$

We only compare two bets (or acts) here although, of course, there are 9 different acts f. The two acts are $(G, \neg G)$ and $(B, \neg B)$. The consequences (or payoffs) associated with these bets are also shown in Table 6. We assume that there is a utility function $u(\cdot)$ over consequences. Without loss of generality, we set $u(1) = 1$ and $u(0) = 0$.

The last column of Table 1 shows the probabilities of the states as they would be derived for a subjective expected utility maximizer who considers

- the draws from the two urns and the coin flip as independent events, and who assumes that
- the probability of a green ball drawn from urn is $r = \frac{1}{3}$, because this composition was announced,
- the probability of a blue ball drawn from the urn is $p \in [0, \frac{2}{3}]$,
- the probability of a yellow ball drawn from the urn is $q \in [0, \frac{2}{3}]$, and
- that the order in which the balls are drawn and the coin is tossed does not matter (i.e. the Reversal of Order axiom holds, see e.g. Seo, 2009).

The probability of state s, $\pi_s(p, q)$, then follows by the usual product rule for independent events.

It seems pretty intuitive to assume that the two balls and the coin toss are independent from each other.[16] However, in the presence of ambiguity, the notion of independencies no longer clear. Depending on how ambiguity is modeled, different concepts of independence arise. Here, we adopt the notion of independence suggested in Gilboa and Schmeidler (1989, p. 150).[17]

Let P be the set of priors for the probabilities that a ball drawn from the urn is blue and Q for the probability that a ball drawn from the urn is yellow . Let $\pi(p, q) := (\pi_1(p, q), \ldots, \pi_{18}(p, q))$, denote the probability distribution over states, where $\pi_s(p, q)$ is the product measure for state s as defined in the last column of Table 6.

Assumption 1 *The set of priors $\mathcal{P}$ is the set of Gilboa-Schmeidler-independent product measures,*

$$\mathcal{P} := \text{co}\,\{\pi(p, q) \mid p \in P,\ q \in Q\} \subseteq \Delta(S).$$

[16] Whether subjects in an experiment actually consider these events to be independent is another matter.

[17] Bade (2008) provides a discussion of alternative ways for defining independence of sets of priors.

The set of priors is thus the convex hull of all product measures that can be constructed in the familiar way.[18]

We model ambiguity–averse subjects using the MEU approach by Gilboa Schmeidler (1989). A decision maker whose preferences are described by MEU evaluates a bet f by

$$MEU(f) = \min_{\pi \in \mathcal{P}} \sum_{s \in S} \pi_s u(f(s)). \quad (1)$$

Proposition 1 *MEU–maximizers are indifferent between the combinations of bets* $(G, \neg G)$ *and* $(B, \neg B)$ *(or* $(Y, \neg Y)$ *).*

Proof 1 *Using equation 1 and the probabilities in Table 6 we can obtain*

$$MEU(G, \neg G) = MEU(B, \neg B) = MEU(Y, \neg Y) = \frac{1}{2}.$$

[18] Since payoffs in (1) are linear in probabilities, minimal payoffs are, of course, unaffected by taking the convex hull.

Appendix B: Instructions

Treatment M9[19]

Welcome to our experiment. Please read this instruction carefully. Turn off your mobile phone, don't talk to your neighbors, and remain quiet throughout the experiment. If you have any questions, please raise your hand, and someone will come over.

All participants who observe the rules will definitely receive a show-up fee of 3 €.

You can make more money if you make a decision. At the end of the experiment you will receive you total payment in cash.

On the table we have a bag containing 30 marbles which are either blue, green, or yellow.

- Exactly 9 of the 30 marbles are green.
- The other 21 marbles are either blue or yellow.

The machine we have built with your help is designed to determine the number of blue and the number of yellow marbles. Our objective is to make the content of the bag unpredictable, both for the participants and the experimenters.

The number of blue and yellow marbles are determined in the following way:

- A participant chosen by the other participants will throw 21 bouncy balls, one after another, into the machine.
- The number of bouncy balls ending up in the black socks will determine the number of blue marbles to be put in the bag.
- The number of bouncy balls ending up in the white socks will determine the number of yellow marbles to be put in the bag.

At the end of the experiment, one marble will be drawn by a participant chosen by the participants.

Now you can bet on one of the two colors:

[19] The instructions for treatment M11 are modified in an obvious way.

☐ I bet on blue	If a blue marble is drawn at the end of the experiment, you will be paid an additional 6€.
☐ I bet on green	If a green marble is drawn at the end oft he experiment, you will be paid an additional 6€.

By the way, half of the participants of this experiment (and in this room) are asked to choose between blue and green, the other half are asked to choose between yellow and green.

Treatment B9

Welcome to our experiment. Please read this instruction carefully. Turn off your mobile phone, don't talk to your neighbors, and remain quiet throughout the experiment. If you have any questions, please raise your hand, and someone will come over.

All participants who observe the rules will definitely receive a show-up fee of 3 €.

You can make more money if you make a decision. At the end of the experiment you will receive you total payment in cash.

On the table we have a bag containing 30 marbles which are either blue, green, or yellow.

- Exactly 9 of the 30 marbles are green.
- The other 21 marbles are either blue or yellow where it is unknown how many are blue and how many are yellow. Thus, all combinations of 0 blue marbles (and therefore 21 yellow marbles) and 21 blue marbles (and therefore 0 yellow marbles) are possible. After the experiment you may check the content of the bag.

At the end of the experiment, one marble will be drawn by a participant chosen by the participants.

Now you can bet on one of the two colors:

☐ I bet on blue	If a blue marble is drawn at the end of the experiment, you will be paid an additional 6€.
☐ I bet on green	If a green marble is drawn at the end oft he experiment, you will be paid an additional 6€.

By the way, half of the participants of this experiment (and in this room) are asked to choose between blue and green, the other half are asked to choose between yellow and green.

Appendix C: Additional tables

Table 7: Frequency of bets depending on option

			Bet			
treatment	option		Green	Blue	Yellow	total
M9	blue	count	30	31	0	61
		%	49.2%	50.8%	0.0%	100.0%
	yellow	count	25	0	35	60
		%	41.7%	0.0%	58.3%	100.0%
B9	blue	count	10	17	0	27
		%	37.0%	63.0%	0.0%	100.0%
	yellow	count	10	0	16	26
		%	38.5%	0.0%	61.5%	100.0%

Note: Distribution of subjects' choices depending on their options (green vs. blue or green vs. yellow). Due to an error by the experimenters this comparison can only be done for treatments M9 and B9. In M11 always the same option (blue vs. green) was used.