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The Rich Domain of Ambiguity Explored

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The Rich Domain of Ambiguity Explored

Anonymized

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ABSTRACT. Ellsberg and others suggested that decision under ambiguity is a rich empirical domain with many phenomena to be investigated beyond the Ellsberg urns. We provide a systematic empirical investigation of this richness by varying both the uncertain events, the outcomes, and combinations of these. Although ambiguity aversion is prevailing, we also find systematic ambiguity seeking, confirming insensitivity. We find that ambiguity attitudes depend on the source of uncertainty (the kind of uncertain event) but not on the outcomes. Ambiguity attitudes are closer to rationality (ambiguity neutrality) for natural uncertainties than for the Ellsberg urns, as appearing from the reductions of monotonicity violations and of insensitivity. Our rich domain serves well to test families of weighting functions for fitting ambiguity attitudes. We find that two-parameter families, capturing not only aversion but also insensitivity, are desirable for ambiguity even more than for risk. The Goldstein-Einhorn family performs best for ambiguity.

KEYWORDS: ambiguity aversion, likelihood insensitivity, pessimism, rationality, four-fold pattern

1 Introduction

The first studies of ambiguity focused on the aversion found in the classical Ellsberg (1961) urns. Later studies revealed a richer picture. First of all, Trautmann & van de Kuilen's (2016) empirical review reports a four-fold pattern: for moderate to high likelihoods of gains and for unlikely losses, ambiguity aversion is prevailing; but for unlikely gains and most losses, ambiguity seeking is prevailing. Additionally, several authors have argued for the importance to study natural sources of uncertainty as occurring in real life, rather than the artificial sources almost exclusively studied in laboratory experiments. In the latter sources, ambiguity is created artificially by concealing information from subjects, such as about compositions of urns with colored balls (Ellsberg urns), or with only upper and lower bounds of probabilities given to subjects. Camerer & Weber (1992 p. 361) wrote: "There are diminishing returns to studying urns." Ellsberg (2011) himself also emphasized the richness of ambiguity and the importance of considering other phenomena, both regarding events and outcomes:

> "… doesn't fully explain to me why nearly all later research has focused only on 'ambiguity aversion,' nor why most expositions have wrongly attributed the same *preoccupation* to me. … I happen to believe that this latter pattern [ambiguity seeking] will be much more frequent than the reverse in certain circumstances of payoffs and events other than the ones that were addressed explicitly in the QJE article and *almost exclusively investigated later*. Because these other circumstances … certainly deserving of much more experimental and theoretical investigation than it has received." [Italics added]

Other authors emphasizing the desirability to study natural events include Abdellaoui, Vossmann, & Weber (2005) and Heath & Tversky (1991 p. 6); footnote 2 gives further references.

The domain of nonprobabilized uncertainties is rich similarly to the domain of nonmonetary commodities, with many kinds of informational and emotional configurations. One ambiguity attitude per subject for all nonprobabilized uncertainties seems to be implausible similarly to one utility curve per subject for all nonmonetary commodities being implausible. To illustrate this point, Tversky & Fox (1995) showed that basketball fans are ambiguity seeking when the ambiguity concerns basketball, whereas they will continue to be ambiguity averse for most other sources. Although this finding is empirically unsurprising, it is useful as a first demonstration of the richness of ambiguity. Our paper follows up on the aforementioned findings and recommendations. We examine ambiguity attitudes toward different uncertain events, different outcome domains, and combinations of both. Thus we can compare source dependence with outcome dependence, and we can detect how artificial ambiguity with information concealed from subjects differs from natural ambiguity.

In addition to behavioral phenomena under ambiguity, this paper also examines parametric fittings of ambiguity attitudes. There have been numerous detailed studies of the performance of different parametric models for decision under risk. Focusing on nonexpected utility for risk, the Web Appendix cites 48 studies. Erev et al. (2010) reported a prediction competition between such models. This paper shows how such comparisons can be done for ambiguity. Because ambiguity is a richer domain than risk, we expect many future studies of parametric models for ambiguity to come.

Closest to our data fitting of ambiguity are Ahn et al. (2014), Hey, Lotito, & Maffioletti (2010), and Maafi (2011). The first two studies compared different general ambiguity theories regarding their overall fitting and predictive power. Our study differs from those two in the following two respects. First, our approach is less general in the sense that we consider only one ambiguity theory, biseparable utility, which however does comprise several other theories as special cases and which was found to best fit and predict data in the preceding studies.[1] Second, our approach is more general in the sense that we compare different parametric models and distinguish between several components of ambiguity attitudes and their corresponding parameters. Mainly, we consider the ability of parametric models to accommodate variations in ambiguity in a rich domain. Maafi (2011), like us, used biseparable utility, but only considered probability-interval events and monetary outcomes.

2 Related literature

It is well known that probability weighting for risk depends on the sign of outcomes (Tversky & Kahneman 1992) and, to some extent, on the size of outcomes (Etchart 2004; Fehr-Duda et al. 2010). For ambiguity, some theories model ambiguity attitudes through the utility of outcomes (Klibanoff, Marinacci, & Mukerji 2005; Nau 2006; Neilson 2010). Then, by definition, ambiguity attitudes depend on the outcomes considered. These theories are primarily normatively motivated. For normative applications in decision analysis, see Borgonovo & Marinacci (2015). Our purpose is, however, purely empirical.

We investigate outcome dependence by changing the nature of outcomes (from money to waiting time or life duration). This dependence has so far been investigated only for risk and not yet for ambiguity. Rottenstreich & Hsee (2001) found that extreme outcomes can induce emotions that affect probability weighting. Abdellaoui & Kemel (2014) also found such

[1] Kothiyal, Spinu, & Wakker (2014) showed that this also holds for Hey, Lotito, & Maffioletti (2010).

dependence for monetary outcomes versus temporal outcomes, where time referred to waiting time with nothing to do, i.e., time lost, as relevant in transportation economics. Kemel & Travers (2016) considered decisions from experience, which can be considered to be intermediate between risk and ambiguity. Their results are similar to those of Abdellaoui & Kemel (2014). Armantier & Treich (2015) found that the probability weighting function can depend on the source that generates the probabilities even if all probabilities concerned are objective. Chew, Ebstein, & Zhong (2012) similarly found a difference when the probabilities are generated by a digit of temperature in a known city versus an unknown city. Thus there is some evidence of outcome- and source-dependent probability weighting under risk. Yet, it mostly occurs for very emotional outcomes and sources and it may not be very strong in general. In many applications, outcome independent probability weighting will serve well as an approximation tractable enough to allow predictions (Berns et al. 2007).

For ambiguity, some studies considered natural sources of uncertainty.[2] They demonstrated that ambiguity attitudes depend on the source. We are not aware of studies that investigated the dependence of ambiguity attitudes on kinds of outcomes or on combinations of outcomes and events, or that tested parametric families for ambiguity. We consider three of the most important outcomes: (a) Money, which is the most studied outcome in economics; (b) delayed time of getting an outcome, widely investigated in the literature on discounting; (c) life duration, the most important outcome in the health domain. McFadden (2010) suggested that studying ambiguity with time as outcome, as in (b), is important. Several studies considered this topic (see Kemel & Paraschiv 2013 and their references). The only study that, like ours, considers both variations in outcomes and events under ambiguity is Eliaz & Ortoleva (2015). They investigated effects of correlations on ambiguity attitudes. Calibrating ambiguity attitudes or their dependence on outcomes or events was not the purpose of their study.[3]

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[2] See Abdellaoui et al. (2011), Abdellaoui et al. (2014), Baillon et al. (2016), Baillon & Bleichrodt (2015), Chew, Ebstein, & Zhong (2012), Chew et al. (2008), Fox, Rogers, & Tversky (1996), Fox & Tversky (1998), Li (2016), and Tversky & Fox (1995).

[3] Because their outcomes differed regarding correlations, the underlying uncertainty also differed. Ambiguity neutrality or ambiguity aversion therefore could not be calibrated.

3 Theory on ambiguity attitudes: the source method and α-maxmin expected utility

Gilboa & Marinacci (2013) reviewed the theoretical and normative literature on ambiguity aversion. Our descriptive analysis of ambiguity is based on biseparable utility, which is a convenient point of departure for many popular ambiguity models (Ghirardato & Marinacci 2001). Biseparable utility comprises multiple priors, α-maxmin, prospect theory for gains (and for losses), and Choquet expected utility (Wakker 2010 §10.6). Thus our results pertain to all these theories. We use the source method (Abdellaoui et al. 2011), a tractable specification of biseparable utility based on Chew & Sagi's (2008) axioms. In Kothiyal, Spinu, & Wakker (2014), the source method predicted ambiguous choices better than a number of popular alternative models. We will also discuss our results from the perspective of the popular α-maxmin model (Ghirardato et al. 2004).

In the source method, for each source of uncertainty, ("a(mbiguity)-neutral") subjective probabilities are specified, which are next transformed into ambiguity decision weights. Although it was long believed, based on Ellsberg's paradoxes, that probabilities cannot be used to model ambiguity, Chew & Sagi (2008) showed that they can still be used, by allowing decision attitudes to depend on the source of uncertainty. Thus an a-neutral probability 0.5 for an ambiguous Ellsberg urn is transformed more pessimistically than an objective probability 0.5, implying ambiguity aversion as in the Ellsberg paradox. *Sources* of uncertainty are groups of events generated by the same uncertainty mechanism. This concept was proposed by Heath & Tversky (1991) and formalized by Tversky & Fox (1995). The three sources of uncertainty that we will consider in our experiment are: (1) which of 10 possible colors a ball drawn from an Ellsberg urn has; (2) which of 10 possible districts a child from India came from; (3) which of 10 possible viruses caused a disease.

We use Dimmock, Kouwenberg, & Wakkers's (2016) simplified implementation of the source method. Those authors deliberately minimized the number of measurements and the experimental time per subject so as to demonstrate the tractability of their method. We will use more detailed and thorough measurements and more time per subject so as to obtain better reliability and validity.

This section explains how we measured the ambiguity indexes for the Ellsberg urn. For the other two sources of uncertainty it was done the same way. The basic setting is that 100 colored balls are contained in an urn. Each ball has been painted in one out of ten colors. Suppose there is one winning color; say it is red. One ball is drawn randomly from the urn. If its color is red, a good outcome is received, say €500. Otherwise the bad outcome of receiving nothing occurs.

Subjects thus consider *gambles* $\gamma_E\beta$ on events E, yielding a good outcome γ if event E happens and a bad outcome β otherwise. We considered events E_j of j winning colors for j = 1, 3, 5, 7, and 9, where higher j's give more favorable events because their likelihoods are higher. We call $\frac{j}{10}$ the *ambiguity-neutral (a-neutral) probability* of event E_j, because an ambiguity neutral (Bayesian) decision maker would assign this subjective probability $\frac{j}{10}$ to event E_j. For each event E_j we elicited the *matching probability* $m(\frac{j}{10})$, being such that a subject considered gaining γ with objective probability $m(\frac{j}{10})$ to be equivalent to gaining γ under event E_j. The function $m(\cdot)$ depends on the source of uncertainty, which can be expressed by adding a subscript: $m_{So}(\cdot)$.

Within each source, subjects had the same information about all events E_j of j winning colors, and they had no reason to consider any preferable to any other. Hence we made the common assumption that subjects have no color preference, and satisfy Chew & Sagi's (2008) exchangeability axiom, justifying the use of a-neutral probabilities and their transformations.

An ambiguity neutral decision maker has $m\left(\frac{j}{10}\right) = \frac{j}{10}$ for all j. For general decision makers and each event E_j,

$$AA_j = \frac{j}{10} - m(\frac{j}{10}) \tag{3.1}$$

serves as *event-dependent ambiguity aversion index*. Ambiguity averse subjects dislike the ambiguity comprised in E_j and a smaller objective probability $m\left(\frac{j}{10}\right) < \frac{j}{10}$ will then be equivalent to E_j, implying Eq. 3.2. We have:

$$\frac{j}{10} - m\left(\frac{j}{10}\right) > 0\text{: ambiguity aversion for } E_j; \tag{3.2}$$

$$\frac{j}{10} - m\left(\frac{j}{10}\right) = 0\text{: ambiguity neutrality for } E_j; \tag{3.3}$$

$$\frac{j}{10} - m\left(\frac{j}{10}\right) < 0\text{: ambiguity seeking for } E_j. \tag{3.4}$$

Thus the matching probabilities $m\left(\frac{j}{10}\right)$ provide an easy tool to measure ambiguity attitudes. Dimmock, Kouwenberg, & Wakker (2016 Theorem 3.1) gave a theoretical justification, showing that matching probabilities easily and completely capture ambiguity attitudes for biseparable utility. Knowledge of the risk attitude and of matching probabilities indeed fully captures preferences over binary gambles.

Dimmock, Kouwenberg, & Wakker (2016) derived global indexes of ambiguity attitudes as follows. As an intermediate step of recoding data, for the five data points $\left(\frac{j}{10}, m\left(\frac{j}{10}\right)\right)$ in which $j = 1, 3, 5, 7, and$ 9, the best-fitting (by quadratic distance) line

$$p_{a-neutral} \rightarrow c + s * p_{a-neutral} \tag{3.5}$$

($s \geq 0$ and truncated at values 0 and 1; i.e., it should not be negative or exceed 1) is determined. This line only serves as an intermediate step in a mathematical calculation of the indexes, and not as a statistical estimation. It is natural that ambiguity aversion is higher as the values $m\left(\frac{j}{10}\right)$ are lower, analogously to Schmeidler's (1989 pp. 572, 574) index of ambiguity aversion (see Eq. 5.1). We thus define the following index (equivalent to the area above the line in Eq. 3.5):

$b = 1 - s - 2c$ is the index of *ambiguity aversion*. (3.6)

This component is motivational, reflecting an overall liking or disliking of ambiguity. Under expected utility (ambiguity neutrality) c = 0 and s = 1, and the index has value 0. Positive values indicate ambiguity aversion, with 1 being the maximum value, while negative values indicate ambiguity seeking, with –1 being the minimum value.

Further

$a = 1 - s$ is the index of *a(mbiguity-generated likelihood)-insensitivity*. (3.7)

This index reflects the shallowness of $m(\cdot)$ in the middle region and, hence, insensitivity towards changes in likelihood of the E_j events. Insensitivity is most naturally interpreted as a cognitive component of ambiguity, reflecting general (lack of) understanding of probability. Under expected utility we have $a = 0$, reflecting optimal sensitivity. The index a usually is positive, reflecting lack of sensitivity. Dimmock, Kouwenberg, & Wakker (2016) gave further explanations and theoretical background. The indexes reflect distances from ambiguity neutrality and satisfy the desirable property of utility independence (Baucells & Borgonovo 2014; Qiu & Steiger 2011).

Baillon et al. (2015) showed that the above analysis can be reinterpreted using the α-maxmin model. Then their level of perceived ambiguity is identical to our a, and their α ambiguity aversion index is our aversion index b per unit of perceived ambiguity (α ≈ b/a). Hence the indexes contain the same information. Dimmock et al. (2015) used this alternative interpretation.

We also use parametric families to fit the data. We estimate how the matching probabilities are a function of the a-neutral probabilities. The parametric families that we use have commonly been used for probability weighting functions for decision under risk, capturing risk attitudes. For risk, aversion ("pessimism") and insensitivity are central, as they are for ambiguity, which is why they can be expected to also be suited for analyzing ambiguity. We use them for the matching probabilities $m\left(\frac{j}{10}\right)$, and consider the following five families.

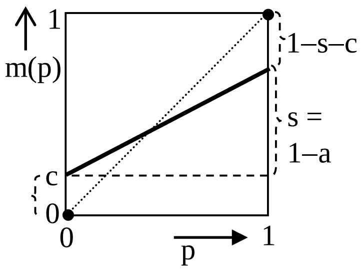
Neo-additive (Figure 3.1):

$m(0) = 0$; $m(1) = 1$; $m(p) = c + sp$ for $0 < p < 1$; $s \geq 0$;

$m(\cdot)$ is truncated at values 0,1. (3.8)

Indexes of a-insensitivity and ambiguity aversion were defined in Eqs. 3.6 and 3.7.

FIGURE 3.1. The neo-additive family



Goldstein & Einhorn (1987):

$$m(p) \;=\frac{bp^{a}}{bp^{a}+(1-p)^{a}}\;;\,a\geq0,\,b\geq0\,. \tag{3.9}$$

Here a is an (anti-)index of a-insensitivity and b is an (anti-)index of ambiguity aversion.

Prelec (1998) 2-parameter:

$$m(p)=\Big(exp\Big(-\big(-ln(p)\big)^{a}\Big)\Big)^{b};\,a\geq0,\,b\geq0\,. \tag{3.10}$$

Here a is an (anti-)index of a-insensitivity and b is an index of ambiguity aversion.

Prelec (1998) 1-parameter:

Eq. 3.10 with $b\,=\,1$.

Tversky & Kahneman (1992):

$$m(p) \;=\frac{p^{c}}{(p^{c}+(1-p)^{c})^{1/c}}\;\text{for}\geq0.28\,. \tag{3.11}$$

Here c is an (anti-)index of both a-insensitivity and ambiguity aversion.

4 Experimental design

4.1 The basic treatment

We considered five treatments, that is, five combinations of sources and outcomes, displayed in Table 4.1. Exact wordings of the instructions for subjects are in the appendix. We partially randomized the order of presentation of the treatments by using two different orderings: week, basic, year, health, kid; and the partly reversed ordering: health, year, basic, week, kid. We kept the kid treatment at the end because it was meant to arouse specific emotions. Those could distort the other decisions.

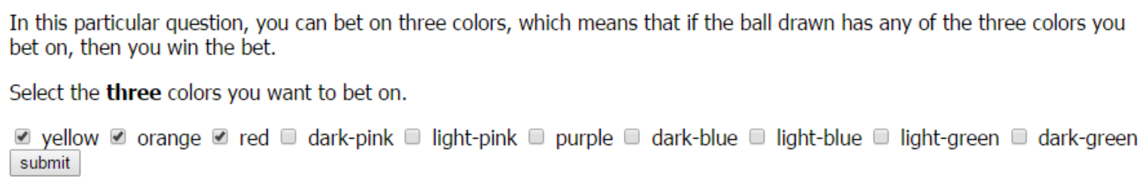
TABLE 4.1. The five treatments

treatment	source of uncertainty	outcome
basic	Ellsberg urn	money
week	Ellsberg urn	waiting time (weeks)
year	Ellsberg urn	waiting time (years)
kid	districts	money
health	viruses	life duration

This subsection presents the first treatment, i.e., the basic treatment, which concerns a standard Ellsberg experiment. Two urns both contained 100 balls with possibly up to ten different colors: yellow, orange, red, dark-pink, light-pink, purple, dark-blue, light-blue, light-green and dark-green. For urn K the composition of balls was known, while for urn U the composition was unknown. The unknown urn U was prepared beforehand by an outside party (a secretary). Therefore the experimenters themselves did not know its composition during the experiment. Subjects were informed about the preparation of the unknown urn so that they knew that the experimenters could not influence the composition of the unknown urn.

For each $j = 1, 3$, or 5, subjects first chose which j out of ten colors were the winning colors, which determined the winning event E_j for urn U (Figure 4.1). Subjects next chose from which urn, U or K, a ball was randomly drawn. A choice list was used to determine the urn K yielding indifference. Figure 4.2 gives an example. The three winning colors were yellow, orange, and red. Urn U is at the right side, and the 11 urns K are on the left, one in each row, with the number of winning balls specified. This number was different in different choice situations (rows). Subjects chose between K and U in each row, marking their preference in the middle columns. If the color of the ball drawn was a winning color, then the subject would receive a good outcome (€500); otherwise a bad outcome (€0) would result. If the implementation of the real choice situation at the end involved urn K, i.e., if the subject had chosen urn K, then this urn was prepared with the proper composition by the experimenters in front of the participants of that session.[4]

FIGURE 4.1. Screenshot of choosing the winning colors



For each E_j, we elicited choices for all 101 compositions of winning balls in urn K using the incentive compatible implementation of refined choice lists introduced by Abdellaoui et al. (2011) which will be explained next. For low numbers of winning balls in K, subjects should prefer urn U, and for high numbers urn K. Somewhere in between these 101 choices, preferences switched. We measured this switching point in two steps, as follows. A first choice list (Figure 4.2) included 11 choices between urn K and urn U, with 0, 10, …, 100 balls of the winning color(s) in urn K. After subjects made a decision as in Figure 4.2, another more refined choice list was shown to them. The second choice list (Figure 4.3) was refined between the two values in the first choice list where the switching had happened. In Figure 4.2, switching happened between 30 and 40, so that the next choice list in Figure 4.3 included the choices for 31, 32, … to 39 balls of winning color(s) in urn K. Here the switch happened between 35 and 36. This way we were able to infer the choices of the subject for all 101 compositions of urn K.

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[4] For swiftly implementing the composition of urn K , for every color, groups of twenty balls were stringed (the balls had holes), thus enabling us to quickly and reliably prepare any amount of balls between 0 and 100 in front of the participants, verifiable for all.

FIGURE 4.2. Screenshot of choice list: first step

You have chosen the colors **yellow, orange, and red**, which means that if the ball drawn is in **yellow, orange, or red**, then you win the bet.

Look at the choice list below. Now you can decide which bag (Bag K or Bag U) to draw the ball from.

Number of Balls in Bag K		K	U	Number of Balls in Bag U	
€500: yellow, orange, and red	€0: the other colors			€500: yellow, orange, and red	€0: the other colors
0	100	Bag K	Bag U		
10	90	Bag K	Bag U		
20	80	Bag K	Bag U		
30	70	Bag K	Bag U		
40	60	Bag K	Bag U		
50	50	Bag K	Bag U	**unknown**	100 - unknown
60	40	Bag K	Bag U		
70	30	Bag K	Bag U		
80	20	Bag K	Bag U		
90	10	Bag K	Bag U		
100	0	Bag K	Bag U		

submit

FIGURE 4.3. Screenshot of choice list: second step

In the previous choice list, you have chosen Bag U when there are **30 or fewer** yellow, orange, and red balls in Bag K. You chose Bag K when there are **40 or more** yellow, orange, and red balls in Bag K. Please indicate your preference in the refined choice list below, between row **31 and 39**.

Row	Number of Balls in Bag K		K	U	Number of Balls in Bag U	
	€500: yellow, orange, and red	€0: the other colors			€500: yellow, orange, and red	€0: the other colors
0	**0**	100	Bag K	Bag U	**unknown**	100 - unknown
...	...	...	Bag K	Bag U		
30	**30**	70	Bag K	Bag U		
31	**31**	69	Bag K	Bag U		
32	**32**	68	Bag K	Bag U		
33	**33**	67	Bag K	Bag U		
34	**34**	66	Bag K	Bag U		
35	**35**	65	Bag K	Bag U		
36	**36**	64	Bag K	Bag U		
37	**37**	63	Bag K	Bag U		
38	**38**	62	Bag K	Bag U		
39	**39**	61	Bag K	Bag U		
40	**40**	60	Bag K	Bag U		
...	...	...	Bag K	Bag U		
100	**100**	0	Bag K	Bag U		

submit

If for j winning balls in U, preferences switched between i and $i + 1$ winning balls in K, then we estimated the matching probability $m(\frac{j}{10})$ to be $\frac{(i+\frac{1}{2})}{100}$. If there were no switches, then $m(\frac{j}{10})$ was 0 or 1 as the case may be. The program enforced monotonicity and did not allow for multiple switches. Following Abdellaoui et al. (2011), we randomly selected one from the 101 compositions of urn K implemented, and not only from the choices actually asked in the two choice lists. In this manner we ensured incentive compatibility.

For each determination of $m(\frac{j}{10})$ as just described and $j = 1, 3, 5$, we would immediately consider the same set of j colors, with the difference that these were now the losing colors, while the other $10 - j$ colors were the winning colors. Hence, six questions (with different winning colors) were asked in each treatment. This way we would determine six values, $m(\frac{j}{10})$ and $m(1 - \frac{j}{10})$, $j = 1, 3, 5$. [5]

4.2 Alternative treatments

In the second treatment week, the outcome was changed into waiting time (for receiving an outcome) instead of money. Subjects still received money, €250, with certainty. But now the uncertainty concerned the time when subjects would receive the €250. The good outcome was receiving the money immediately, and the bad outcome was receiving it eight weeks later. Interactions between money, time, and uncertainty, as in the magnitude effect (Baucells & Heukamp 2012), play no role in our design because, first, money is kept constant and, second, all that matters is that there is a good and a bad outcome.

The third treatment year was like the treatment week, the only difference being that the money to be won with certainty amounted to €5000 and that the time of receipt was either immediately or in 10 years. Choices in this treatment year were hypothetical (serving to test the hypothetical bias), and subjects received an immediate flat payment of €250 if this treatment was selected for implementation. In every other respect the two treatments week and year were the same as the basic treatment, concerning the same Ellsberg urns and the same way to measure matching probabilities $(\frac{j}{10})$ and $m(1 - \frac{j}{10})$, $j = 1, 3, 5$.

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[5] For $j = 5$, we thus obtained two measurements of $m(\frac{5}{10})$. These were never statistically different for any treatment (Wilcoxon signed rank tests), suggesting that there were no framing effects or color preferences. In most of the following analyses we therefore used the average of the two observations of $m(\frac{5}{10})$. In parametric fittings it is appropriate to take the two observations as separate, and so we did.

Treatment kid

In the fourth treatment, the kid treatment, we did not change the outcomes (€500 or €0) relative to the basic treatment, but instead the source of uncertainty. The source involved a charitable program in rural India, paying for school education of children. We showed our subjects a photo (Appendix) of one of the children whose lives have been transformed by this charitable program.

The child came from one of 100 villages that were distributed over 10 possible districts: Ludhiana, Sangrur, Amritsar, Kaithal, Sonipat, Jodhpur, Pali, Udham Singh Nagar, Bulandshahar and Shahjehanpur. Subjects could now gamble on the district that the child's village belonged to. They could choose which winning districts (instead of colors) to gamble on. The ambiguous option in this treatment is called Option C (Charity) and the risky option is called Bag K (Known).

Our subjects could not be expected to have any geographic knowledge of the concerned villages or districts, or their sizes. Thus the 10 districts represented equally likely events to our subjects in the same way as the 10 colors in the Ellsberg urn were equally likely events. The 100 villages are analogous to the 100 balls in the Ellsberg urn; neither of them is outcome-relevant beyond district/color. Both the photo and the charitable context (related to school education) can be expected to arouse positive emotions[6], which may offset the negative emotions generated by us concealing information about the districts from our subjects. Hence, this treatment could have been called the feel-good treatment. Matching probabilities were measured using the same procedure using a known urn K and an unknown urn U as before. Now each district was coupled with a color, so that gambling on three districts corresponded with gambling on three colors in the known urn; and so on.

The uncertainty in this treatment is less artificial than in the preceding treatments in the sense that the uncertainty refers to real, natural events rather than to drawings of balls from urns only done for the purpose of the experiment. Yet they are still artificial in the sense that information is deliberately kept secret from the subjects. Hence the ambiguity here is intermediate between artificial and natural.

Treatment health

The fifth and final treatment was a health treatment, which deviated more from the basic treatment than the other treatments did. We now changed both the outcomes and the source of uncertainty. This treatment was again hypothetical, and subjects received an immediate flat payment of €250 if this treatment was selected for implementation. For the source of uncertainty,

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[6] We do not have the direct rating of the picture we used in the experiment, but two similar pictures, given in the Web Appendix, are highly rated in International Affective Picture Scale (IAPS).

we employed a virus story. The subjects were asked to imagine that they were diagnosed with a particular disease and that they would have to receive a treatment against it. It would furthermore be known that there were ten possible mutually exclusive viruses (numbered from 1 to 10) causing the exact same disease. There would be no way to diagnose which virus was causing the disease, but the disease would only be cured if the real virus was treated. In the case of recovery (disease cured), the subjects would live 50 years longer in good health, and otherwise one year longer in good health. That is, the outcome now was life duration. Specifying a particular life duration may seem to be unrealistic, but is still widely used in the health domain for various reasons (Gold et al. 1996). Therefore, its study is important.

Subjects were asked to choose between Treatment K and Treatment U. Treatment K would use a broad-spectrum antiviral supplement with a known success rate (given in %). Treatment U was said to be new and would use specific supplements (numbered from 1 to 10) which would only be effective against the virus with the corresponding number. Only if the right supplement for the real virus would be chosen, the disease would be cured. Because the subjects were told that there is no way to diagnose which virus is causing the disease, the 10 viruses were equally likely to them in the same way as the Ellsberg colors or the districts were. Event E_j now meant that only j supplements could be provided. To measure matching probabilities $m(\frac{j}{10})$, we now did not use a known urn, but the treatment K with success rates specified for 0%, 1%, …, 100%.

Because this fifth health treatment was hypothetical, subjects did not choose the j supplements provided as they chose the j colors in the basic experimental treatment. Instead, the first j supplements were offered in treatment U. This avoided both suspicion and illusion of control, the common confounds in Ellsberg experiments.

The uncertainty in this source is not artificial in the sense that it does not result from an experimenter deliberately concealing information from subjects, but is caused by extraneous lack of information, as is common in applications. In this sense this treatment is the most natural one in this study.

4.3 Further experimental details

Subjects N=66 subjects (73% male, 27% female), bachelor and master students from various fields were recruited online from the ESE-EconLab website of Erasmus School of Economics.

Procedure The experiment was conducted at the experimental laboratory of *anonymized.*

Incentives Subjects received a show-up fee of €5. One randomly selected subject in each of the three sessions received an additional payment. We first randomly selected which of the five treatments would be implemented. Two treatments were hypothetical, for which a fixed payment of €250 was given. For the other three, one randomly selected choice was implemented. Total average earnings were €16.36. All implementations of random selections were non-computerized and verifiable to the subjects, by drawings from bags.

5 Results

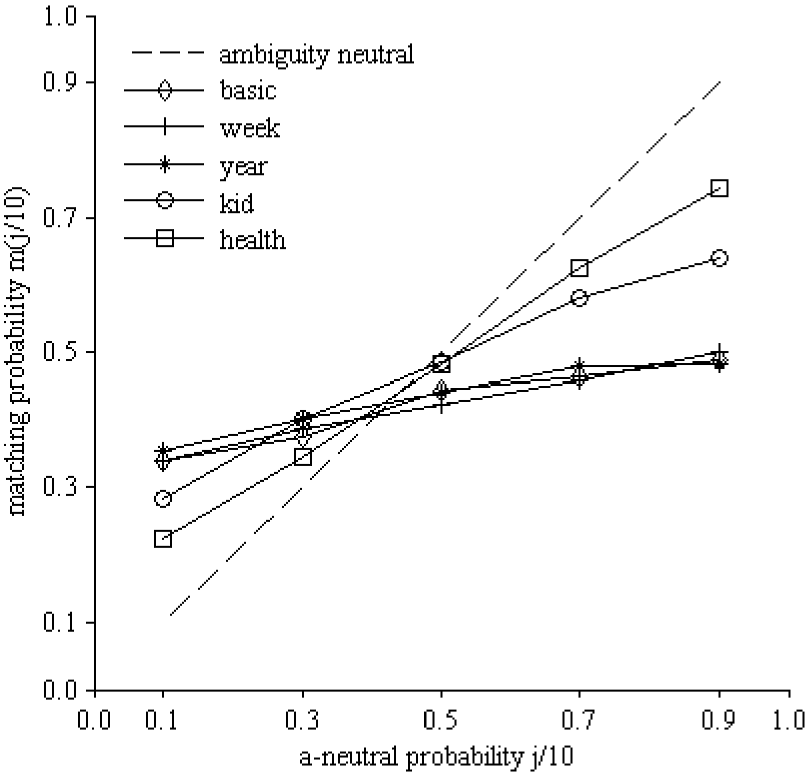
Different orders of treatments mostly gave no differences (Web Appendix). Hence, we pooled the data. In short, our findings regarding the indexes are as follows. A principal component analysis shows that our two indexes capture most of the variance of the ambiguity attitudes. Changing the outcomes does not affect the ambiguity attitudes but changing the sources of uncertainty does. In the kid and health treatments we find lower aversion and better sensitivity. Analyses using the parametric families of weighting functions confirm the aforementioned findings. The Goldstein & Einhorn family fits the data best. The indexes of all families are strongly correlated across different treatments, showing predictability across sources of uncertainty and person-specific components.

5.1 Indexes *b* and *a*, and outcome- versus event-dependence

Figure 5.1 plots the mean matching probabilities $m(\frac{j}{10})$, $j = 1, 3, 5, 7, 9$, and displays the main phenomena, which will later be confirmed by statistical tests. The curves are somewhat below 0.5 on average, meaning that there is more ambiguity aversion than ambiguity seeking. For low likelihoods there is prevailing ambiguity seeking, in agreement with a-insensitivity. The curves are almost linear in the interior, suggesting that neo-additive functions fit the data well, in agreement with common findings (Baucells & Villasís 2015; Trautmann & van de Kuilen 2016).

[7] All sessions were scheduled the same day to avoid that participants could learn beforehand that a charitable program in rural India was involved, and could have gathered information about it.

FIGURE 5.1. Mean matching probability



As for comparisons between treatments, the curves of the three Ellsberg treatments (basic, week, year) are very similar. Hence, outcomes do not affect ambiguity attitudes. Changes in the source of uncertainty, in the kid and health treatments, do affect ambiguity attitudes. In particular, sensitivity becomes better as the ambiguity becomes more natural.

Table 5.1 analyses ambiguity attitudes per event (Eqs. 3.2-3.4), presenting the median event-dependent ambiguity aversion index (Eq. 3.1) per event and treatment. For treatments basic, week, year, and kid, it shows ambiguity seeking for the unlikely events E_1 and E_3 and ambiguity aversion for all other events, except for ambiguity neutrality[8] for E_5 in the kid treatment. For the health treatment, the index is negative and close to 0 for all the events, with E_1, E_3, and E_9 displaying significant ambiguity seeking.

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[8] By our middle-point approximation of matching probabilities, ambiguity neutrality with $m\left(\frac{j}{10}\right) = \frac{j}{10}$ can never happen exactly (except for the average of the two measurements for a-neutral probability 0.5). Hence, we also considered the modification of Eq. 3.3 into $\left|\frac{j}{10} - m\left(\frac{j}{10}\right)\right| \leq 0.005$, and did statistical tests using two-sided Wilcoxon signed rank tests and comparing individual *AAj*'s with 0.005 and -0.005, taking as *p*-values the larger of the two tests. This modification did not seriously affect our results. Only the significance levels of (0.5, year), (0.1, health), and (0.9, health) were downgraded by one *.

TABLE 5.1. Ambiguity attitudes per event

a-neutral probability $\frac{j}{10}$	median event-dependent ambiguity aversion index AA_j (percentage of subjects with the majority ambiguity attitude)				
	basic	week	year	kid	health
0.1	-0.245^{***} (87.88%)	-0.245^{***} (89.39%)	-0.280^{***} (84.85%)	-0.085^{***} (84.85%)	-0.005^{***} (81.82%)
0.3	-0.065^{***} (80.30%)	-0.100^{***} (74.24%)	-0.105^{***} (71.21%)	-0.050^{***} (80.30%)	-0.005^{***} (74.24%)
0.5	0.035^{***} (77.27%)	0.010^{***} (75.76%)	0.005^{***} (66.67%)	0.005 (53.03%)	–0.005 (66.67%)
0.7	0.230^{***} (95.45%)	0.205^{***} (96.97%)	0.205^{***} (89.39%)	0.100^{***} (71.21%)	–0.005 (56.06%)
0.9	0.430^{***} (96.97%)	0.405^{***} (93.94%)	0.410^{***} (98.48%)	0.205^{***} (75.76%)	-0.005^{**} (51.52%)

$^{***}\ p \leq 0.01$; $^{**}\ p \leq 0.05$; $^{*}\ p \leq 0.10$

Table 5.2 presents estimations of the indexes assuming that subjects are homogeneous and then minimizing overall linear least squares.[9] Standard errors are corrected for clustering at the subject level. There is some ambiguity aversion, but it is close to neutral (0), and for the kid and health treatment it is not significant. A-insensitivity is strong. Changes of outcomes do not affect the indexes, which are the same for the treatments basic, week, and year ($p > 0.52$ and $p > 0.56$ for b and a). Changing the source of uncertainty from basic to the kid treatment gives lower ambiguity aversion (because of prior expectation, one-sided test: $p < 0.01$) and much better sensitivity ($p < 0.001$). The health treatment has yet more sensitivity than the kid treatment ($p < 0.01$), but aversion is not significantly different ($p = 0.84$).

TABLE 5.2. Overall ambiguity attitudes across treatments

	basic	week	year	kid	health
ambiguity aversion index b	0.15^{***}	0.16^{***}	0.13^{***}	0.04	0.03
a-insensitivity index a	0.81^{***}	0.80^{***}	0.83^{***}	0.55^{***}	0.34^{***}

$^{***}\ p \leq 0.01$; $^{**}\ p \leq 0.05$; $^{*}\ p \leq 0.10$

[9] We also extracted the two indexes b and a for every subject per treatment using linear least squares estimations. See the Web Appendix for medians and comparison among treatments. They confirm all results reported here.

5.2 Individual consistency

Table 5.3 reports Spearman's rank correlation coefficients of each of the indexes between all five treatments, where the upper-right triangle is for ambiguity aversion index b and the lower-left triangle is for a-insensitivity index a. Correlations among the treatments basic, week, and year are highly significant. The treatment kid is dissimilar, and the health treatment even more so. For the a-insensitivity index a, the health treatment even has no significant correlation with the treatments basic, week, and year.

TABLE 5.3. Correlations of individual ambiguity attitudes across treatments

		ambiguity aversion index b				
		basic	week	year	kid	health
	basic		0.50***	0.64***	0.24*	0.21*
	week	0.56***		0.53***	0.27**	0.18
a-insensitivity index a	year	0.57***	0.40***		0.47***	0.32***
	kid	0.32***	0.24*	0.11		0.16
	health	0.06	-0.04	-0.03	0.36***	

$^{***}\ p \leq 0.01$; $^{**}\ p \leq 0.05$; $^{*}\ p \leq 0.10$

5.3 Principal component analysis of the ambiguity attitudes

Table 5.4 shows the results of a principal component analysis of the event-dependent ambiguity aversion indexes AA_j, j = 1, 3, 5, 7, 9, for each treatment.[10] For all the treatments, the first two components together account for more than 83% of the variance in the decisions of the subjects. In the treatments basic, week, year, and health, the first component is highly correlated with ambiguity aversion index b and the second component with a-insensitivity index a. Treatment kid, however, reverses the explanatory power of the indexes: a-insensitivity is more dominant than ambiguity aversion. This may be because there is less variation in ambiguity aversion in the kid treatment but less variation in a-insensitivity in the other treatments. These results confirm that indexes a and b are primary components in ambiguity attitudes, capturing most of the variance. This finding confirms early psychological theories (Hogarth & Einhorn 1990).

[10] We also perform a non-parametric version by conducting a principal component analysis on the tied ranks of these indexes AA_j. The results are fully consistent with the ones reported here and are in the Web Appendix.

TABLE 5.4. Principal component analysis of event-dependent ambiguity aversion indexes

Variable		loadings on first two components									
		basic		week		year		kid		health	
		1^{st}	2^{nd}	1^{st}	2^{nd}	1^{st}	2^{nd}	1^{st}	2^{nd}	1^{st}	2^{nd}
AA_1		0.14	0.83	0.18	0.86	0.14	0.86	-0.53	0.65	0.19	0.77
AA_3		0.18	0.49	0.31	0.37	0.22	0.42	-0.25	0.43	0.19	0.46
AA_5		0.23	0.05	0.33	0.10	0.32	0.11	0.05	0.28	0.34	0.22
AA_7		0.54	-0.03	0.52	-0.14	0.55	-0.11	0.29	0.34	0.52	-0.04
AA_9		0.78	-0.25	0.71	-0.32	0.72	-0.26	0.76	0.44	0.73	-0.39
eigenvalue of the component		0.10	0.05	0.13	0.05	0.11	0.05	0.09	0.06	0.13	0.06
proportion of variance explained (%)		60.32	28.84	61.96	23.49	56.23	27.76	54.12	34.61	62.63	27.71
correlation coefficient	a	0.66^{***}	-0.63^{***}	0.59^{***}	-0.69^{***}	0.62^{***}	-0.70^{***}	0.98^{***}	-0.03	0.40^{***}	-0.80^{***}
	b	0.92^{***}	0.22^{*}	0.94^{***}	0.18	0.92^{***}	0.27^{**}	0.36^{***}	0.88^{***}	0.89^{***}	0.37^{***}

*** $p \leq 0.01$; ** $p \leq 0.05$; * $p \leq 0.10$

5.4 Discussion of rationality

Due to monotonicity, $m(\frac{j}{10})$ should be increasing in j. We find much insensitivity, with $m(\cdot)$ only weakly increasing with a shallow slope. Because of this, and because of the randomness that is common in decision experiments, there are many violations of monotonicity at the individual level. We test monotonicity in all cases possible. The second row in Table 5.5 gives the percentages of violations for the five treatments. These relatively high percentages—higher than commonly found for decision under risk—confirm that there is more insensitivity (lack of understanding) under ambiguity. They also show that choices are most rational in the health treatment, second-most in the kid treatment, and they are least so, and about equal, in the remaining three treatments.

TABLE 5.5. Violations of monotonicity and correlations between indexes

	basic	week	year	kid	health
violations of monotonicity	25%	28%	27%	19%	14%
correlations between indexes b and a	0.45^{***}	0.39^{***}	0.35^{***}	0.29^{**}	0.15

$^{***}\ p \leq 0.01$; $^{**}\ p \leq 0.05$; $^{*}\ p \leq 0.10$

Although ambiguity aversion and a-insensitivity are conceptually distinct, they may well be empirically correlated. A positive correlation is natural because both indexes concern deviations from Bayesianism and, according to many, deviations from rationality. The third row in Table 5.5 gives the Spearman's rank correlations of the two indexes for each of the five treatments. The correlations all are significantly positive except for the health treatment (where there is not as much irrationality).

5.5 Parametric fittings

We use least squares data fitting which equals the maximum log-likelihood method when assuming Fechnor error. For a detailed analysis, see Johnstone (2012). We also did fitting at the individual level, reported in the Web Appendix. Those results confirm all results reported here. Table 5.6 shows that the ordering of goodness of fit by adjusted R^2 is: (1) neo-additive; (2) Goldstein & Einhorn; (3) Prelec 2-parameter; (4) Prelec 1-parameter; (5) Tversky & Kahneman. The adjusted R^2 criterion corrects for the number of parameters used, but still the two-parameter families are superior. It is clearly important to consider both the aversion and the insensitivity component to study ambiguity, and focusing on one (Prelec 1-parameter considers only insensitivity) or combining the two (Tversky & Kahneman) loses too much explanatory power.

Other than this, the ordering of parametric fit found is different than for risk (Balcombe & Fraser 2015). The reason is that insensitivity plays a more central role for ambiguity than for risk. Hence the neo-additive family, which can readily handle extreme degrees of insensitivity, fares best, and Prelec's 1-parameter family fares better in this case than Tversky & Kahneman's. That Goldstein & Einhorn perform to some extent better than Prelec's two-parameter family may be because the former better separates the two parameters. In Prelec's family, the insensitivity parameter a overlaps partly with the aversion parameter b, also capturing some aversion.

TABLE 5.6. Fit of parametric families: adjusted R^2 (%)

parametric family	basic	week	year	kid	health
neo-additive	86.58	81.89	84.44	88.74	87.37
Goldstein & Einhorn	86.34	81.61	84.18	88.52	87.09
Prelec 2-parameter	86.27	81.59	84.12	88.38	87.07
Prelec 1-parameter	85.84	81.30	83.43	87.52	86.83
Tversky & Kahneman	82.09	77.90	79.35	86.67	86.77

TABLE 5.7. Fitted parameters (significance level given by comparison with basic)

parametric family	parameters	basic	week	year	kid	health
neo-additive	c	0.33	0.32	0.35	0.26^{**}	0.15^{***}
	s	0.19	0.20	0.17	0.45^{***}	0.66^{***}
Goldstein & Einhorn	$b^{\downarrow}$	0.74	0.73	0.76	0.92^{***}	0.93^{***}
	$a^{\downarrow}$	0.15	0.15	0.13	0.35^{***}	0.55^{***}
Prelec 2-parameter	b	0.91	0.93	0.89	0.86	0.92
	$a^{\downarrow}$	0.14	0.14	0.12	0.35^{***}	0.56^{***}
Prelec 1-parameter	$a^{\downarrow}$	0.18	0.18	0.17	0.42^{***}	0.60^{***}
Tversky & Kahneman	$c^{\downarrow}$	0.52	0.51	0.52	0.62^{***}	0.70^{***}

$^{***}\ p \leq 0.01$; $^{**}\ p \leq 0.05$; $^{*}\ p \leq 0.10$

$^{\downarrow}$: anti-index

Table 5.7 reports the fitted parameters of these parametric families (all significant at the 1% level).[11] Comparing across treatments, changes of outcomes do not affect the parameters, where treatments week and year are the same as basic. Changing the source of uncertainty in the kid

[11] We also fit these parametric families individually. For medians of those individual parameters, correlations of parameters across treatments, and correlations among parametric families per treatment, see the Web Appendix. They confirm all results reported here.

and health treatments gives better sensitivity and lower ambiguity aversion judging by the parameters, except in the Prelec 2-parameter family where the ambiguity aversion parameter *b* is constant across all treatments. Comparing between the health and kid treatments, the neo-additive family further gives lower *c* and *s* for the health treatment than the kid treatment ($p <$ 0.01 both), Goldstein & Einhorn and the Prelec 2-parameter family both give the same *b* ($p =$ 0.90 and $p = 0.29$ respectively) and higher *a* (one-sided test: $p < 0.001$ and $p < 0.01$ respectively) for health. The Prelec 1-parameter family also gives a higher *a* ($p < 0.01$), but Tversky and Kahneman's family gives the same *c* for health and kid ($p = 0.13$).

5.6 Discussion of experimental details

To control for suspicion (the experimenters rigging urns/districts), subjects could choose the colors/districts to gamble on for the basic, week, and year treatment with the unknown Ellsberg urn and also for the kid treatment with unknown regions. That, immediately after having gambled on an event, the subjects gambled on its complement further made clear to subjects that we had no interest in rigging urns or districts.

We grouped events and their complements together to make likelihoods clearer to subjects and thus obtain replies of higher quality. This way we can also directly measure Schmeidler's (1989) indexes of ambiguity aversion, as follows. In Schmeidler (1989), as a consequence of the expected utility assumed for risk, the matching probabilities $m\left(\frac{j}{10}\right)$ are the weights of the events E_j. Schmeidler (1989 pp. 572, 574) proposed

$$1 - m\left(\frac{j}{10}\right) - m\left(1 - \frac{j}{10}\right) \tag{5.1}$$

as indexes of ambiguity aversion in terms of his weighting function. The indexes are the sum of the event-dependent ambiguity aversion indexes (Eq. 3.1) of event E_j and its complement E_{10-j}, and they have been widely used since. Our index *b* of ambiguity aversion is an aggregation of these indexes for the pairs (E_1, E_9), (E_3, E_7), and (E_5, E_5^c). By using matching probabilities instead of Schmeidler's weighting function, we make the indexes directly observable.

In one respect the health treatment is not realistic, which forced us to resort to hypothetical choice: we should have exchangeable symmetric uncertainties. This is needed for direct comparability with the Ellsberg urn where such symmetry is central. Such symmetries are virtually absent from practice, and therefore it is virtually impossible to come up with a realistic example of this kind with real incentives. This difficulty can be held against the representativeness of the Ellsberg urns for applications. It is more interesting to study natural sources of uncertainty. For an emotion-neutral source of ambiguity, the ambiguity-generator of Stecher, Shields, & Dickhaut (2011) may serve better than Ellsberg urns or probability intervals where information is concealed for subjects, but we here focus on traditional stimuli. Baillon et al. (2016) introduce new indexes of ambiguity attitudes for natural events without symmetries. Here again, we use traditional symmetries to stay close to traditional measurements and to directly compare with Ellsberg-type measurements, so as to test their external validity in most direct comparisons.

The year treatment in our experiment was also hypothetical, even though there are many reasons to prefer real incentives to hypothetical choice. One of the aims of this treatment was to test for the hypothetical bias. That we found no differences between the year treatment and the incentivized week treatment suggests that there is no hypothetical bias in our design. The good quality of the results in the health treatment, better than in the other treatments, and our apparently well motivated subjects, further suggest that we have no hypothetical bias. Because of the high insensitivity that we found, together with many violations of monotonicity, the behavior of our subjects in the first three treatments comes close to models of complete ignorance, where all ambiguous events are treated alike (fifty-fifty), leading to a maximal insensitivity index of 1. Such behavior was axiomatized by Cohen & Jaffray (1980). It is used for the diffuse events of Gul & Pesendorfer (2015), who put source dependence, examined empirically in this paper, central in their theoretical analysis.

6 General discussion

We first discuss the basic treatment with the classical Ellsberg urns (10 colors) and monetary outcomes. Here we find the usual prevailing ambiguity aversion as appearing from our *b*-index. In particular, for the ambiguous fifty-fifty event of five colors, which is similar to the two-color Ellsberg paradox, 77% of the subjects exhibited ambiguity aversion. For unlikely events (one and three colors), a-insensitivity has an effect contrary to ambiguity aversion, resulting in ambiguity seeking. This is what we find, with over 80% of the subjects exhibiting ambiguity seeking. This finding of ambiguity seeking confirms Ellsberg's prediction made in the 1960s (see Ellsberg 2001 p. 203 ll. 12-14; pp. 205-206) and agrees with common empirical findings (Trautmann & van de Kuilen 2016). Chew, Bin, and Zhong (2015) found it (“preference for skewed”) in a study on source preference for different kinds of probability intervals and their unions. Combined with ambiguity aversion for likely events it gives an estimated a-insensitivity index of 0.81, showing that this component is also present in the traditional Ellsberg setting. Our study adds to several other recent studies questioning the universality of ambiguity aversion (Trautmann & van de Kuilen 2016).

We incorporated the second (week) and third (year) treatment to see if changes of outcomes, keeping the source fixed, impacted ambiguity attitudes. They do not, as confirmed by all our analyses: their matching probability curves (Figure 5.1), the comparisons of ambiguity attitude per event (Table 5.1), the strong correlations between these treatments (Table 5.3), similar fits of parametric functions (Table 5.6), and same best-fitting parameters (Table 5.7). This is further confirmed by the same violations of monotonicity and correlations between indexes (Table 5.5). Because these, anticipated, results are based on accepted null hypotheses, we used two treatments so as to have high statistical power.

We incorporated the fourth (kid) treatment to acquire more natural, although not yet entirely natural uncertainty. We made a special effort to increase source preference by letting this treatment be what can be called a feel-good treatment. We added the fifth (health) treatment as the most deviating one, with the most natural source of uncertainty and again different outcomes. It could be called an understand-good treatment. The fourth and fifth treatments exhibit increased source preference and sensitivity through their matching-probability curves (Figure 5.1) and event-wise ambiguity attitudes (Table 5.1). Their aversion and insensitivity indexes are less related to the other three treatments, with: (a) the cognitive sensitivity index of the health treatment even being unrelated to those of the first three treatments (Table 5.3); (b) their monotonicities being increasingly better than for the first three treatments; (c) their overlap being lower because there was less irrationality to be shared (Table 5.5); (d) better fits of parametric families suggesting less noise (Table 5.6); and (e) parameters from the fitted families (Table 5.7) deviating from the first three treatments.

Although many more studies are needed before general conclusions can be drawn, our results suggest that ambiguity attitudes depend on the sources of uncertainty (the kinds of events) more than on outcomes. This finding supports empirical theories that model ambiguity attitudes through event functions. Such theories include Choquet expected utility, multiple priors and α-maxmin models, new prospect theory, and their many recent generalizations, and also biseparable utility as used in our analyses. Our findings are consistent with Maafi (2011), Abdellaoui et al. (2013), and Abdellaoui et al. (2014). They did not investigate dependence of ambiguity attitudes on outcomes as we did, but instead dependence of utility of money on the source of uncertainty. They did not find such dependence.

We have demonstrated how the full richness of ambiguity can be investigated in principle. Of course, completing this large task is impossible for one paper. Even a complete design of all combinations of the sources and outcomes that we considered in this study would be too large for one paper. We chose the combinations that we expected to give the most interesting results at this stage.

One reason for us to consider waiting time as outcome is that there is much interest in the effect of ambiguity on optimal stopping times. See Della Seta, Gryglewicz, & Kort (2014), Nishimura & Ozaki (2007), and Riedel (2009). The results of these studies could have been distorted if ambiguity attitudes towards waiting-time outcomes were different from other outcomes. It therefore is reassuring that we find no such difference.

Our deviating findings for the kid treatment are unsurprising given that we deliberately induced positive emotions for the events involved, which is comparable to the emotion-arousing outcomes of Rottenstreich & Hsee (2001). In this sense our finding is similar to Tversky & Fox (1995), whose finding of ambiguity seeking for ambiguous basketball events under basketball fans is similarly unsurprising.

The high sensitivity in the health treatment, and absence of ambiguity aversion, are remarkable. Subjects discriminated between different levels of likelihood considerably better than in the other treatments. It suggests greater interest and better motivation on the part of subjects, even though this treatment could not satisfy the real incentive principle of experimental economics. It has been observed before that subjects are well motivated to answer questions about health, even if hypothetical (Bleichrodt & Pinto 2009 end of §2). In the same spirit, many people voluntarily donate money to support medical investigations. One reason for the reduced ambiguity aversion could be that outcomes in this treatment were perceived as losses. It is well known that there is less ambiguity aversion for losses (Trautmann & van de Kuilen 2016; Attema, Brouwer, & l'Haridon 2013).

7 Conclusion

Following the recommendation of Ellsberg (2011) and many other authors, we have investigated ambiguity and its richness empirically, with varying outcomes, varying uncertain events, and combinations of those. The richness considered reinforces the external validity of our general findings. These findings are:

- For natural uncertainties, ambiguity aversion is less pronounced and rationality (sensitivity and monotonicity) is higher than for artificial Ellsberg uncertainties.
- Ambiguity attitudes are more driven by the kind of uncertainty than by the kind of outcome.
- Our two indexes of ambiguity attitudes capture most of the variance in the data.
- For ambiguity, insensitivity (inverse-s)[12] is even more important (besides aversion) than for risk, implying that ambiguity seeking is prevailing for some stimuli.

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[12] Or, equivalently, perceived level of ambiguity in the α-maxmin model.

- Individual ambiguity attitudes have predictive power across different sources.
- We recommend using the Goldstein-Einhorn (1987) family for analyzing ambiguity attitudes.

Several specific findings in this paper depend on the particular sources and outcomes considered. Future studies will further investigate the relevant phenomena of ambiguity attitudes in different contexts, bringing new insights into this important but new domain of human decisions.

Appendix. Experimental instructions (screenshots)

FIGURE A.1. Screenshot of treatment kid

In this part, you can bet not only on bags but also on an option of **C**hildren's welfare (Option C).

Option C concerns school-building work in rural India.

The Satya Bharti School Program aims to make available free, high quality primary and secondary education to poor children in the rural areas of India. The kid in the photo is one of the kids whose lives have been transformed because of this program. The program helped 100 villages in 10 districts in India and the kid is from one of the 100 villages. You can bet on the district where the kid's village belongs.

At the end of the experiment, we will inform you about the correct district (irrespective of your decisions).

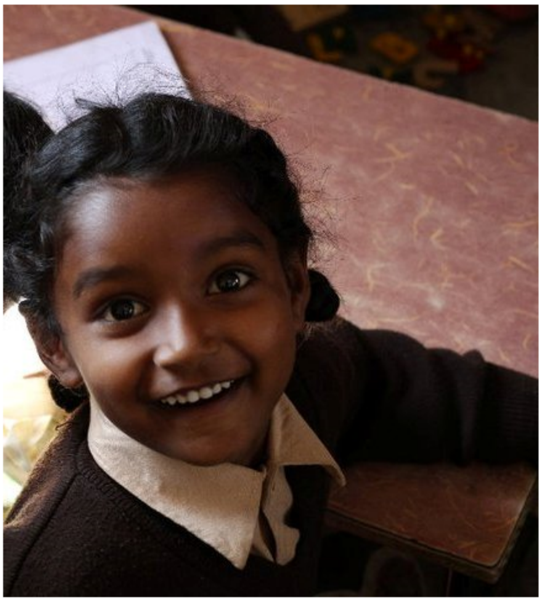

FIGURE A.2. Screenshot of treatment health

Imagine that you are diagnosed with a certain disease. You have to receive a treatment against the disease; there is no possibility to abstain from treatment. The only choice you have is which treatment you will receive.
Research on the disease all over the world has revealed the following facts: There are ten possible viruses causing the disease (i.e. virus 1, virus 2, virus 3, virus 4, virus 5, virus 6, virus 7, virus 8, virus 9, and virus 10). The prevalence (the rate of occurrence) of the viruses causing the disease is unknown. And there is no way to diagnose which virus you have; they all lead to the same disease. (The viruses are mutually exclusive; you will always have just one virus). Only if the real virus is treated will the disease be totally cured. Assume that then you will live **50 years** longer from now on in good health and die. If the real virus is not treated, you will live only **1 year** longer from now on in good health and die.

For all the decision scenarios in this part, there are two possible treatments. Both treatments have the **same treatment duration** and the **same costs**. Neither treatment has **adverse side effects**.
Treatment K:
Treatment K treats the disease with a **K**nown success rate. The success rate is known from experiences with previous patients. It uses a broad-spectrum antiviral supplement, which is not specific to any one of the viruses, but is generally effective for all viruses alike. For example, for treatment K, the success rate can be 10%. (As an explanation: if 10 out of 100 patients are cured, the success rate is 10%.) We will also consider other possible success rates. If you are cured by treatment K, you will live 50 years longer from now on in good health; otherwise you will live only 1 year longer from now on in good health.
Treatment U:
Treatment U is new. It uses ten different supplements. We name the ten supplements S1, S2, S3, S4, S5, S6, S7, S8, S9, and S10 respectively. Each supplement is effective for the corresponding virus. (For example, supplement S7 is only effective for virus 7.) However, different supplements are not always available. You will therefore be treated only with the available supplements. Remember that there is no way to tell which virus causes your disease. If the right supplement for the real virus is chosen, then you will be cured and live 50 years longer from now on in good health; otherwise you will live only 1 year longer from now on in good health.

Web Appendix

See *anonymized*

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