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DISCRIMINATING BETWEEN MODELS OF AMBIGUITY ATTITUDE: A QUALITATIVE TEST

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Discriminating between Models of Ambiguity Attitude: A Qualitative Test

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Abstract:

The exchange between Epstein (2010) and Klibanoff *et al*. (2012) identified a behavioral issue that sharply distinguishes between two classes of models of ambiguity sensitivity, exemplified by the α-MEU model and the smooth ambiguity model, respectively. The issue in question is whether a subject's preference for a randomized act (compared to its pure constituents) is influenced by a desire to hedge independently resolving ambiguities. Building on this insight, we implement an experiment whose design provides a qualitative test that discriminates between these importantly distinct classes of models. Among subjects identified as ambiguity sensitive, we find greater support for the class exemplified by the smooth ambiguity model; the relative support is stronger among subjects identified as ambiguity averse.

KEYWORDS: Ambiguity sensitivity; ambiguity attitude; testing models of ambiguity sensitive preference.

JEL-CLASSIFICATION: C91, D01, D03, D81, G02.

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1. Introduction

Decision makers choosing between acts are said to face ambiguity if they are uncertain about the probability distribution over contingent states of the world. Numerous experimental studies, largely built on Ellsberg (1961)'s classic examples, show that subjects commonly adjust their behavior in response to such uncertainty (see, e.g., Camerer and Weber, 1992; Wakker, 2010). For instance, many decision makers (henceforth, DMs) demonstrate an ambiguity averse attitude: intuitively put, being inclined to choose actions whose consequences are more robust to the perceived ambiguity.

Over the course of the past two and half decades a large decision theory literature has developed, inspired in part by these experimental findings and in part by the introspective view that in the real world it is often implausible that DMs can confidently conceive of a single probability distribution to summarize her uncertainty about the contingent states (Gilboa and Marinacci, 2013). The pioneering models in this literature, and arguably still the most popular, were the Choquet expected utility (CEU) model of uncertainty aversion introduced in Schmeidler (1989) and the maxmin expected utility (MEU) model of Gilboa and Schmeidler (1989). These models have preference representations which show the DM behaving as if she has a *set* of probability distributions that she considers possible or relevant. Furthermore, an ambiguity averse attitude is modeled by having the DM evaluate an act by its minimum expected utility, where the minimum is taken over the set of probability measures considered possible. In a more general version of this style of model (α-MEU; Hurwicz, 1951; Jaffray, 1989; Ghirardato, Maccheroni and Marinacci, 2004), the DM evaluates acts by considering a weighted average of minimum and maximum expected utility. More recently developed theories have brought in more complicated preference representations which would allow finer nuances of ambiguity attitude. An important feature of the preference representations that distinguishes the newer vintage models from the earlier ones, is that the new models use aggregation rules that do not restrict attention to extreme expected utilities. An example is the smooth ambiguity model of Klibanoff *et al.* (2005) (henceforth, KMM).

Given this theoretical development a natural question is: Are the features that these more complicated theories build in empirically compelling? Or, if we were to stick to the classic models of ambiguity averse behavior, would we be missing any important aspect of such behavior? Experiments based on Ellsberg examples do not discriminate between the old and new vintage models. This paper reports an experimental study which uses a test that does so discriminate: the two classes of models predict qualitatively different behavior in our design.

As mentioned above, a key conceptual divide between the newer vintage models and the older models is the parameterization of ambiguity attitude. In the newer models, it is a more elaborate function. At a foundational level, this has roots in the fact that the newer models drop the requirement of Certainty Independence[1] (or the related Comonotonic Independence) assumed in the pioneering models. An associated behavioral consequence is that indifference curves (in the contingent payoff space) of the newer preference models are not necessarily kinked, unlike those of the older models. Hence, a possible testing strategy to distinguish between and older and newer vintage models would be to exploit the kink property directly. This strategy has been used in the literature, e.g., in Ahn *et al.* (2011) and in Bossaerts *et al.* (2009) (see Section 2.3). However, the kink property is one which the older vintage ambiguity models share with models of first order risk aversion, where preferences are fully probabilistically sophisticated, in the sense of Machina and Schmeidler (1992). (For example, the rank dependence model of Quiggin (1982) and prospect theory of Tversky and Kahneman (1992) bring in kinks in different ways.) Hence, a potential problem of a test that discriminates directly on the basis of a preference kink is that it might attribute behavior to the first generation models of ambiguity attitude when in fact the behavior is due to first order risk aversion.

Our testing strategy does not focus on the presence or absence of a preference kink. It is inspired by the second thought experiment of Epstein (2010) and its generalization in Klibanoff *et al.* (2012). The strategy is to test whether a subject's preference for a randomized act (compared to its pure constituents) is influenced by a desire to hedge across ambiguities in a way that is similar to how diversifying across bets on independent risks hedges those risks. Models of preferences whose representations focus exclusively on minimum and/or maximum expected utilities in the set considered relevant are uninfluenced by such a desire, in sharp contrast to models whose representations also consider non-extreme expected utilities. Intuitively, a DM focusing only on minimum expected utility is analogous to an *infinitely* risk averse agent caring exclusively about the worst possible outcome and so not about diversifying across independently resolving risks, since such diversification does not affect the possibility of the worst outcome.

For concreteness and to allow the reader to relate easily to the discussions in Epstein (2010) and Klibanoff *et al.* (2012), we explain our design and results in the next three sections mainly in terms of α-MEU and smooth ambiguity models, the divide between which is particularly clear. However, as just indicated, the predictions of our test mark a divide across broader classes of models, conditional on particular assumptions. We explain this divide in detail in Appendix C.

The rest of the paper is organized as follows. Section 2 elaborates the background: Section 2.1 describes the α-MEU and smooth ambiguity preference representations more precisely; Section 2.2 presents a modified version of Epstein's example and uses it to explain our testing strategy; Section 2.3 contrasts this strategy with others taken in the literature. Section 3 presents our experimental design and Section 4 our results. Section 5 identifies some issues of robustness of our analysis that are

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[1] Preferences over acts with state-contingent outcomes are said to satisfy Certainty Independence, if whenever an act is preferred to another act, a (stochastic) mixture of the first act with a lottery is also preferred to the act obtained by mixing, using the same weights, the second act with the same lottery.

examined further in Appendices A, B and C. Section 6 concludes the main part of the paper. Appendix D contains further details of the experimental procedures, instructions and results.

2. Background

2.1 Preference representations

Formally, the DM's choices are acts, maps from contingent states $s \in S$ to consequences, which include simple lotteries with real outcomes. We focus on two models of preferences over acts: the α-MEU model and the smooth ambiguity model.

In the α-MEU model, an act f is evaluated by:

$$V_M(f) = \alpha \min_{p \in \mathcal{P}} \left(E_p\big(u(f)\big) \right) + (1-\alpha) \max_{p \in \mathcal{P}} \left(E_p\big(u(f)\big) \right),$$

where u is a vN-M utility function, α is a fixed weight, and $\mathcal{P}$ is a set of probability measures p over the states. The operator E_p takes expectations with respect to the measure p. Attitudes towards pure risk are characterized by the shape of u, as usual, while attitude towards ambiguity is characterized by the weight α; in particular, the greater the value of α, the more ambiguity averse the preference. With $\alpha = 1$, we get the MEU representation.

In the smooth ambiguity model, an act f is evaluated by:

$$V_S(f) = E_\mu \phi \left(E_{p \in \mathcal{P}}\big(u(f)\big) \right),$$

where ϕ is an increasing function mapping utilities to reals, and μ is a subjective probability over elements in $\mathcal{P}$. The operators E_μ and E_p take expectations with respect to the measures μ and p, respectively. Thus, μ represents the DM's subjective uncertainty about the different probabilities deemed possible and, in this sense, is a second-order belief. Attitudes towards ambiguity are characterized by the shape of ϕ, given u. In particular, a concave ϕ characterizes ambiguity aversion, equivalently an aversion to mean preserving spreads in the distribution over expected utilities induced jointly by μ and u. When ϕ is linear or μ is degenerate, the smooth ambiguity model collapses to a subjective expected utility (SEU) model.

2.2 Conceptual background

Consider the following variant of the second thought experiment proposed in Epstein (2010).[2] The DM is told that a ball will be drawn from an urn containing a fixed number of balls, of 4 different types: B_1, B_2, R_1, R_2. She is also told that, in terms of numbers of balls, $B_1 + B_2 = R_1 + R_2$ will hold and, finally, that the relative proportions within the *B-component* (B_1, B_2) and within the *R-component*

[2] Readers familiar with Epstein (2010) and Klibanoff *et al.* (2012) should note that our variant differs in some minor respects from Epstein's thought experiment. Such readers should also note that we adopt a notation that is suited to our experimental design (in ways that will emerge below) but does not match the earlier papers.

(R_1, R_2) will be determined separately. The DM considers acts with contingent outcomes c, c^* and the 50-50 lottery between them. Let $c^* > c$ and normalize the utility index u, so that $u(c^*) = 1$ and $u(c) = 0$. The acts to be considered have state-contingent (expected) utility payoffs as described in Table 1.

To clarify, f_1 yields c^* when a ball of type B_1 is drawn and c otherwise; whereas f_2 yields c^* when type R_1 is drawn and c otherwise. The outcome of the act *mix* is in part decided by the toss of a fair coin: specifically, for any contingency, there is a 0.5 probability that the outcome is determined by applying f_1 and a 0.5 probability that it is determined by applying f_2. Below, "mixed act" always refers to this mixed act and "constituent acts" to f_1 and f_2 (or, in each case, later to their experimental counterparts). The acts g_1 and g_2 each yield, in the contingencies for which a cell-entry of ½ is shown, either c^* or c, depending on the toss of a fair coin (and c otherwise).

Table 1: Five Acts: (Expected) utilities

	B_1	B_2	R_1	R_2
f_1	1	0	0	0
f_2	0	0	1	0
mix	$\frac{1}{2}$	0	$\frac{1}{2}$	0
g_1	$\frac{1}{2}$	$\frac{1}{2}$	0	0
g_2	0	$\frac{1}{2}$	$\frac{1}{2}$	0

How might we expect the DM to choose between some of these acts? The probability of the event $\{B_1, B_2\}$ is objectively known to her (equal to 0.5), whereas that of the event $\{B_2, R_1\}$ is unknown. The information that the DM has about balls of type B_1 exactly matches her information about type R_1. Thus, the symmetry in the situation suggests $f_1 \sim f_2$; and, it is natural to expect that, if the DM is ambiguity averse, she will have the strict preference $g_1 \succ g_2$.[3]

While there may be little to disagree about in these claims, it is much more controversial whether an ambiguity averse DM would see mixing f_1 and f_2 as desirable, compared with either of the latter two acts alone. This issue illustrates one of the main points of contention between the two models considered in the previous subsection. The issue is whether, for an ambiguity averse DM who is indifferent between f_1 and f_2,

$$f_1 \sim f_2 \sim mix \quad \text{or} \quad f_1 \sim f_2 \prec mix$$

would obtain. In the smooth ambiguity model, the latter condition will hold. The averaging that the mixed act offers will be preferred by an ambiguity averse DM to either constituent act, because the

[3] Correspondingly, if $f_1 \sim f_2$ and the DM is ambiguity seeking, one would expect $g_2 \succ g_1$. Ambiguity neutrality, specifically as represented in the model of Anscombe and Aumann (1963), implies that the direction of preference between f_1 and f_2 should match that between g_1 and g_2.

relative proportions within the B-component and the R-component are determined separately. However, in the α-MEU model, an ambiguity averse DM is not swayed by this consideration.

To illustrate the point of contention, it is useful to write down a concrete set of probability measures $\{p_1, \ldots, p_4\}$ that we suppose to be those considered by the DM. In the context of the smooth ambiguity model, think of these as probabilities that are given positive weight by the measure μ and, importantly, with the weights for p_2 and p_3 equal.

Table 2: Example Probabilities

	B_1	B_2	R_1	R_2
p_1	$\frac{1}{10}$	$\frac{4}{10}$	$\frac{1}{10}$	$\frac{4}{10}$
p_2	$\frac{1}{10}$	$\frac{4}{10}$	$\frac{4}{10}$	$\frac{1}{10}$
p_3	$\frac{4}{10}$	$\frac{1}{10}$	$\frac{1}{10}$	$\frac{4}{10}$
p_4	$\frac{4}{10}$	$\frac{1}{10}$	$\frac{4}{10}$	$\frac{1}{10}$

These measures respect the given information, in that, for each $i = 1, 2, 3, 4$, $p_i(B_1 \cup B_2) = p_i(R_1 \cup R_2) = ½$; and, as p_2 and p_3 have equal weight, there is complete symmetry between the B-component and the R-component. The measures respect the independence of the two components in the sense that fixing a "marginal" over (B_1, B_2) does not restrict the "marginal" over (R_1, R_2), or vice versa. The expected utilities generated by applying each of the measures p_i from Table 2 to the acts from Table 1 are as follows:

Table 3: Resulting Expected Utilities

	p_1	p_2	p_3	p_4
f_1	$\frac{1}{10}$	$\frac{1}{10}$	$\frac{4}{10}$	$\frac{4}{10}$
f_2	$\frac{1}{10}$	$\frac{4}{10}$	$\frac{1}{10}$	$\frac{4}{10}$
mix	$\frac{1}{10}$	$\frac{2.5}{10}$	$\frac{2.5}{10}$	$\frac{4}{10}$
g_1	$\frac{2.5}{10}$	$\frac{2.5}{10}$	$\frac{2.5}{10}$	$\frac{2.5}{10}$
g_2	$\frac{2.5}{10}$	$\frac{4}{10}$	$\frac{1}{10}$	$\frac{2.5}{10}$

First, consider acts f_1 and f_2. Their expected utilities coincide under p_1 and p_4, but differ from each other across p_2 and p_3. To see why, note from Table 1 that the evaluation of f_1 depends on the ratio $B_1{:}B_2$ but not on $R_1{:}R_2$; whereas the evaluation of f_2 depends on the ratio $R_1{:}R_2$ but not on $B_1{:}B_2$. In contrast, the evaluation of *mix* depends on both these ratios, but has half the exposure to the uncertainty about each, compared to each of the constituent acts. The point at issue turns on the significance of these facts.

From the perspective of the α-MEU model, what matters for the evaluation of an act are the extremes of the possible expected utilities. Thus, the diversification aspect of the comparison between f_1, f_2 and *mix* is irrelevant, as the minimum and maximum possible expected utilities are the same under each of these acts, as Table 3 shows. So, the DM will be indifferent between them, regardless of her preference over g_1 and g_2.

From the perspective of the smooth ambiguity model, the mixed act provides a hedging of two separate ambiguities, one involving each of the two components, just as diversifying across bets on independent risks provides a hedging of risks. The benefit of such diversification to an ambiguity averse DM is captured through a concave ϕ, in that mean-preserving spreads in the subjective distribution of expected utilities generated by an act are disliked. Since p_2 and p_3 have equal weight, each of f_1 and f_2 yields a mean-preserving spread in expected utilities compared with *mix*, as Table 3 shows. Thus, according to the smooth ambiguity model, the mixed act is preferred to its constituents by any ambiguity averse DM. Indeed, from this perspective, the smoothing of expected utility that the mixed act offers relative to either of its constituents is essentially *the same* as the smoothing which causes an ambiguity averse DM to prefer g_1 to g_2. Thus, the distinctive prediction of the smooth ambiguity model for the case where p_2 and p_3 have equal weight: an ambiguity averse DM will prefer not just g_1 to g_2 but also *mix* to each of f_1 and f_2; and, correspondingly, an ambiguity seeking DM (convex ϕ) would have the reverse preference in each case; and an ambiguity neutral DM (linear ϕ) would be indifferent between g_1 and g_2, and between *mix* and its constituents.

It is important to note a key feature of the perspective of the smooth ambiguity model. Each of p_2 and p_3, the measures across which the mixed act smoothes expected utility relative to its constituents, corresponds to a situation where there is one "marginal" over component (B_1, B_2) and another "marginal" over (R_1, R_2). Thus, it is precisely *because* it is uncertain whether the two components are identical to one another (so leading the DM to consider p_2 and p_3) that the diversification provided by the mixed act is seen by the smooth model as valuable to an ambiguity averse DM. If, instead, the two components were known to be identical (and so only p_1 and p_4 considered), smooth ambiguity preferences would display indifference between the mixed act and its constituents. And, of course, in such a case, α-MEU preferences would display that indifference too. Thus, the key difference between smooth ambiguity preferences and α-MEU preferences that we have highlighted is whether the DM values hedging across ambiguities that are separate, in the sense that the uncertainty about the probability governing one component resolves separately from the analogous uncertainty for the other component. This insight is at the heart of our experimental design, explained in Section 3.

2.3 Related literature

Our experimental design identifies subjects whose behavior is sensitive to ambiguity, categorizing them as ambiguity averse or seeking, and determines whether they behave according to the α-MEU model or the smooth ambiguity model in a set-up of the kind described in Section 2.2. The tests rely on qualitative features of the data, i.e., binary preferences as revealed, in our case as explained below, by ordinal comparisons of certainty equivalents. They do not require estimates of model specific parameters. It is useful to bear these points in mind as we discuss how this experiment fits in with other recent literature. We concentrate on papers whose main objective is to distinguish between models similar to those we consider.

The experimental approach of Halevy (2007) is to determine, on one hand, whether a subject may be classified as ambiguity neutral/averse/seeking (an Ellsberg style determination), while also checking how the subject evaluates an *objective* two-stage lottery, in particular whether the evaluation is consistent with reduction of objective compound lotteries (ROCL). The main finding is that ambiguity aversion is strongly associated with violation of ROCL. Using a strategy based on this finding, the study sifts evidence for or against various models of ambiguity sensitivity. For instance, while the α-MEU model predicts a zero association with ROCL, in several models in (what Halevy terms) the "recursive expected utility" class, ambiguity sensitivity logically implies violation of ROCL. However, under the assumptions of KMM, there is no logical connection between ambiguity aversion (or, seeking) and reduction of compound *objective* lotteries in the smooth ambiguity model.[4] Hence, the strategy based on ROCL is not as useful in distinguishing α-MEU from the smooth model as it is in making other distinctions.

Ahn *et al.*'s (2011) experiment studies a simulation of a standard economic choice problem: each subject allocates a given budget between three assets, each of which pays depending on the color of the ball drawn from a single Ellsberg style three color urn, while the prices of assets are exogenously varied.[5] Different parametric preference models of choice under uncertainty imply different demand functions - the one that best fits a subject's choices identifies his preference model. This strategy distinguishes quite effectively between two classes of models: those that have kinked indifference curves (e.g., α-MEU and the rank dependent utility model) and those with smooth indifference curves (e.g., SEU and smooth ambiguity, even if ambiguity averse), since kinked and smooth indifference curves imply demand functions with distinctively different qualitative characteristics. However, the identification is more problematic *within* each class of model. Indeed, if a subject's preferences are ambiguity averse and conform to the smooth ambiguity model, qualitative properties of choice data in this experiment do not distinguish her from an SEU subject; and similarly an α-MEU preference is difficult to distinguish qualitatively from first order risk aversion.

Conte and Hey (2013) observe subjects' choices between various prospects and study how well the data fits various models of decision making. The identification strategy is not based primarily on qualitative features of the data. The paper estimates various parametric preference models, in

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[4] See the discussion following KMM's Remark 1 and the discussion immediately preceding and in footnote 8 in Klibanoff *et al.* (2012).

[5] Bossaerts *et al.*'s (2009) experiment employs a choice problem very similar to Ahn *et al.*'s (2011), but in the context of a market where prices are determined by trading among subjects.

particular, the SEU, α-MEU and smooth ambiguity models. One part of the study fits the various models subject-by-subject, while another part estimates a mixture model. However, the uncertain prospects the subjects are given to choose between are objective two stage lotteries of the kind used in Halevy (2007). As we remarked earlier, it is not logically implied that subjects who are strictly ambiguity averse/seeking, and whose preferences conform to the smooth model, would evaluate such lotteries any differently from those whose preferences satisfy expected utility theory.

Hayashi and Wada's (2009) experiments investigate choice between lotteries where the subject has imprecise (objective) information about probability distributions defining the lotteries. While they do not specifically test the α-MEU model against a smooth ambiguity model, a finding relevant to our discussion is that their subjects appear to care about more than the best-case and worst-case probability distributions. However, their strategy for detecting this influence of non-extreme points (in the set of possible probabilities) does not exploit the hedging motive that we stress.

3. Experimental Design

3.1 *Core of design*

Our design implements the theoretical set-up of Section 2.1. In place of an ambiguous urn containing balls of four different types, we used specially constructed decks of cards, divisible into the four standard suits. We implemented the component (B_1, B_2) as the composition by suit of the black-suit (henceforth black) cards and the component (R_1, R_2) as the composition by suit of the red-suit (henceforth red) cards, specifically B_1 = spade, B_2 = club, R_1 = heart and R_2 = diamond. Subjects were told that there would be equal numbers of black and red cards in each deck, but not exactly how the black cards would subdivide into clubs and spades, nor how the red cards would sub-divide into hearts and diamonds.

A key feature of our design is that we manipulated whether the compositions of black cards and red cards were mutually dependent (in that the composition by suit of cards one color implied the composition by suit of cards of the other color) or were mutually independent (in that the composition by suit of cards of one color does not give any information about the composition by suit of cards of the other color). In each case, the compositions were determined by drawing from a bag containing two types of balls with their relative proportions unknown. In one case (our '1-ball' condition), a single ball was drawn and its type determined the compositions of both the black cards and the red cards. In the other case (our '2-ball' condition), two balls were drawn *with replacement*: the first to determine the composition of the black cards, the second to determine the composition of the red cards. In the 1-ball condition, the information implied that the set of possible compositions of the whole deck corresponded to $\{p_1, p_4\}$ from Table 2. In the 2-ball condition, the information implied that the set of possible compositions of the whole deck corresponded to $\{p_1, p_2, p_3, p_4\}$, with those corresponding to p_2 and p_3 having equal (but unknown) likelihood. Thus the 2-ball condition implements exactly our variant of the Epstein example, explained in Section 2.2. The 1-ball condition is a control that eliminates the scope for preference for the mixed act over its constituents to derive from the hedging motive postulated by the smooth ambiguity model.

3.2 Presentation of acts

Acts were presented to subjects as "gambles", the outcomes of which would depend, as just indicated, on the suits of cards drawn from decks. We used two protocols, one verbal and the other tabular, in different sessions, to describe the acts and the construction of the decks to subjects. In the event, the results proved insignificantly different and, in Section 4, we pool results from both types of session. Here, we report the tabular protocol in the main text and indicate how the verbal protocol differed from it in footnotes.

In the tabular protocol, acts were described by rows in tables of which the column-headings were suits and the cell entries indicated the results, under each given act, of a card of each suit being drawn. The cell entries indicated either that the act would yield €20 in the relevant contingency; or that it would yield €0 in that contingency; or that the outcome in the relevant contingency would depend on a roll of a (standard 6-sided) die in the following way: €20 if the roll was even and €0 if it was odd.

Table 4: Description of the Acts

	Spade	Club	Heart	Diamond
f_1	€20	€0	€0	€0
f_2	€0	€0	€20	€0
Mix	Roll die is EVEN: €20 Roll die is ODD: €0	€0	Roll die is EVEN: €20 Roll die is ODD: €0	€0
g_1	Roll die is EVEN: €20 Roll die is ODD: €0	Roll die is EVEN: €20 Roll die is ODD: €0	€0	€0
g_2	€0	Roll die is EVEN: €20 Roll die is ODD: €0	Roll die is EVEN: €20 Roll die is ODD: €0	€0

Table 4 contains the rows corresponding to the acts from Table 1.[6] Subjects never had to consider all these acts at once. Instead, they saw tables like Table 4, but with only those rows for the acts they were required to consider at a given point (see below).[7]

[6] We used the die-rolling procedure as it is easier to perform reliably in the lab than tossing a coin. With this amendment, suits as contingencies, c^* = €20 and c = €0, the experimental acts exactly match their theoretical counterparts in Table 1.

[7] In the verbal protocol, each act was described by a single line of text, indicating how the outcome would depend on the card drawn from a deck. Subjects were told that, depending on the card drawn, each gamble could have one of three outcomes: WIN, LOSE, or ROLL. WIN and LOSE would yield payments to the subject

3.3 Decks

Each act was resolved using one of three 10-card decks that subjects were informed would be constructed after they had completed the experimental tasks. Subjects were also told that, after each deck had been constructed, it would be shuffled and placed face down in a pile. A 10-sided die would then be rolled; and the card "drawn" from the deck would be the one whose position in the pile matched the number on the die. All of these processes were conducted publicly, so making it transparent that the compositions of the decks were determined by the procedures described; that any card could be drawn from a given deck; and that neither the experimenter nor subjects' choices could influence which one was.

At the start of the experiment, subjects completed tasks relating to two risky acts each of which would be resolved using Deck 1, which subjects were told would contain 7 spades and 3 hearts. These risky acts served as a simple introduction to the experiment for subjects and, as they would be resolved with a deck of known composition, made it more salient that the remaining acts would be resolved using decks about which subjects had only limited information. Those decks (Decks 2 and 3) are our main focus.

For each of Decks 2 and 3, subjects were told that the deck would consist of 5 black cards and 5 red cards; and, in addition, that the number of spades would be either 4 or 1, with clubs adjusting accordingly; and, similarly, that the number of hearts would be either 4 or 1, with diamonds adjusting accordingly. What subjects were told beyond this varied between Decks 2 and 3, with the different instructions employing in different ways an opaque bag containing balls, numbered either 1 or 4.

In the *1-ball condition*, tasks concerned acts to be resolved using Deck 2. Before completing these tasks, subjects were told that, at the end of the experiment, one ball (only) would be drawn from the opaque bag. The number on it would give both the number of spades and the number of hearts in Deck 2. Thus, in that deck, the number of spades and the number of hearts would be identical.

In the *2-ball condition*, tasks concerned acts to be resolved using Deck 3. Before completing these tasks, subjects were told that, at the end of the experiment, two balls would be drawn from the opaque bag (with replacement). The number on the first ball would give the number of spades in Deck 3 and the number on the second ball would give the number of hearts in Deck 3. Thus, in that deck, the number of spades and the number of hearts would be independent draws.

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of €20 and €0, respectively; whereas ROLL would yield €20 if a standard 6-sided die rolled at the end of the experiment came up even, and €0 if it came up odd. Each line of text had the following form: "If ______ you ______, otherwise you LOSE" where the first placeholder was filled either by a single suit (e.g. "Spade") or a disjunction over two suits (e.g. "Spade or Club") and the second placeholder by either WIN or ROLL.

The information about the possible compositions of Decks 2 and 3 was conveyed to subjects by projection of slides onto the wall of the lab, while the experimenter described the relevant procedures. The slides for the tabular protocol are as shown in Figures 1 and 2.[8]

Figure 1: Deck 2 (1-ball condition)

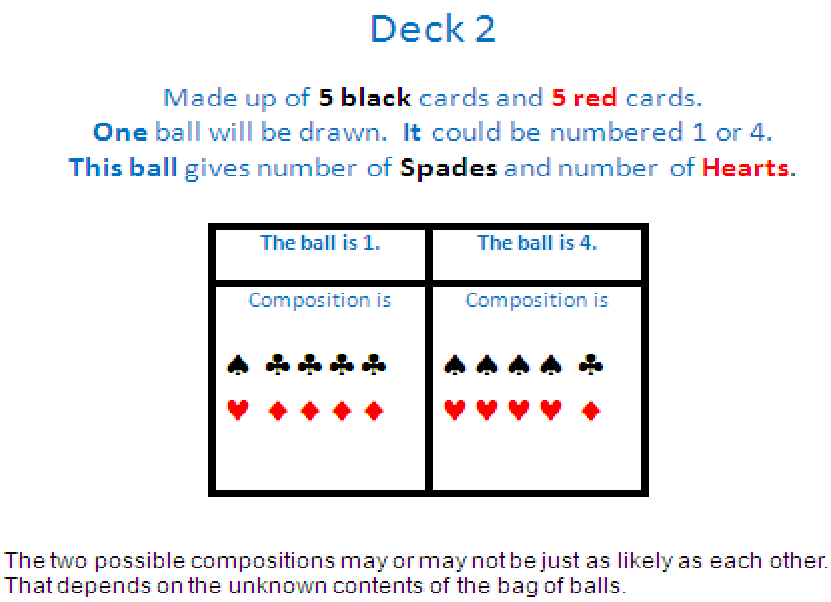
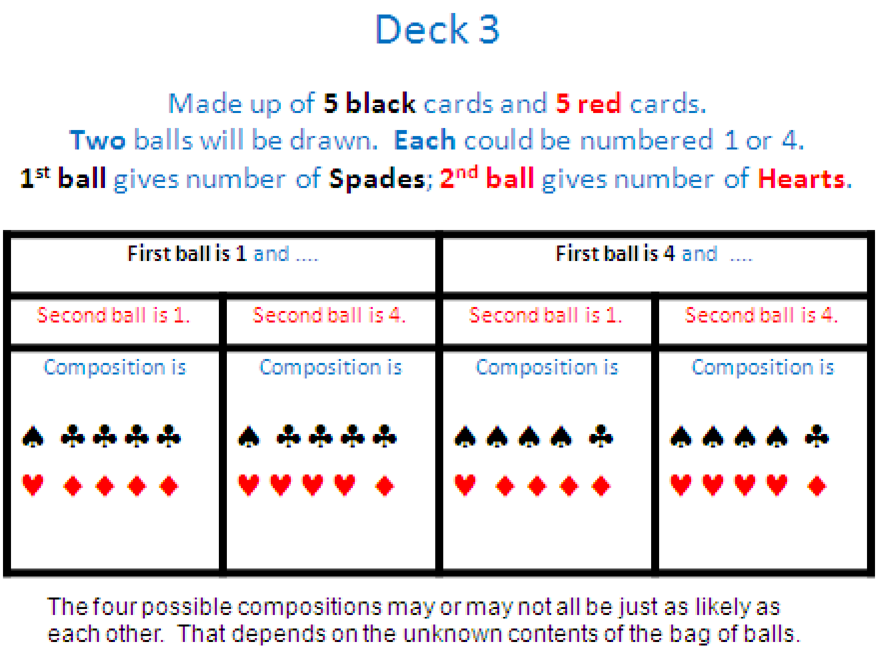


Figure 2: Deck 3 (2-ball condition)



Note that, in each condition, subjects faced uncertainty about the composition of the deck (and, hence, about the probability of the card drawn having any given suit). Importantly, subjects knew

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[8] Each suit (the symbol and corresponding word) appeared in its own color, as did the words black and red, with other text blue. In the verbal protocol, slides described the compositions in words rather than pictorially.

nothing of the relative frequency of types of ball in the opaque bag and hence had no objective probabilities for the possible compositions of either Deck 2 or Deck 3. Thus, for acts resolved with these decks, subjects faced ambiguity in the same sense as in classic Ellsberg experiments. In particular, they were not facing two-stage objective lotteries.

While they had no information about the relative likelihoods of the two possible compositions of Deck 2, nor about those of the first and fourth possible compositions of Deck 3 (relative either to each other or the other two compositions), the information given to subjects *implied* that the second and third possible compositions of Deck 3 were equally likely. (As the draws from the opaque bag determining Deck 3 were with replacement, 1 followed by 4 was precisely as likely as 4 followed by 1.)

3.4 Elicitation of preferences

Our procedure for inferring indifference or strict preference between two acts was to elicit a certainty-equivalent for each of them and to infer the binary preference from the relative magnitudes of the certainty-equivalents. This procedure allows incentivized elicitation of indifference between two acts while avoiding the problems of choice-tasks in which subjects are allowed to express indifference directly.[9] To infer a subject's certainty-equivalent of a given act, we used a form of *choice-list* procedure that yielded interval estimates with a bandwidth of €0.05. The procedure is similar to that of Tversky and Kahneman (1992), sharing with it the important feature that, because estimated certainty equivalents are obtained from choices, they should be unaffected by endowment effects.

In our case, the details of this procedure were as follows. Acts were displayed to subjects and choice-lists completed by them on computers. The experiment was programmed using z-Tree (Fischbacher, 2007). Each choice-list consisted of a table, each row of which described a choice between an act and a certain sum of money. Comparing successive rows of a given choice-list, the sums of money rose moving down the table, but the act remained the same.[10] In a *basic* list, the first row was a choice between the relevant act and €0; the certain sum of money then rose by €1 per row, till the final row was a choice between the act and €20. (See Appendix D for example basic lists.) As, for each act in our design, the two possible final outcomes were €20 and €0, we obviously expected subjects to choose the act in some early rows (at least the first one); to switch to the certainty

[9] If subjects are presented with the choice between two acts and allowed, as a third option, to say they are indifferent, the experimenter faces the problem of how to incentivize the task. If the third option yields randomization over the other two then, when it is taken, what has been elicited is arguably strict preference for the randomization, rather than indifference between the initial two acts (that is, unless the nature of the randomization is unknown to subjects – but, in the latter case, there would be a worse confound in the form of an illegitimate role for attitude to ambiguity).

[10] The "iterative multiple price list" design of Harrison *et al.* (2005) is similar. For further support for the use of choice lists rather than attempting to elicit an indifference point directly, see Cason and Plott (2013). For support for using choice lists where amounts of money, but not the uncertain option, vary from row to row, see Bosch-Domenech and Silvestre (2013).

in some subsequent row; and then to choose the certainty in all remaining rows. After completing all rows of a basic choice-list, subjects had to confirm their choices; the computer would only accept confirmed responses with the single-switch property just described. After confirmation of their responses to a basic choice-list, the subject proceeded to a *zoomed-in* list for the same act. This had the same structure as the basic one, except (i) that the first and last rows were, respectively, the two choices where the subject had switched from the act to the certainty in the basic list, with the responses to these rows filled in as the subject had already confirmed them; and (ii) across the intervening rows the certain sums of money rose in increments of €0.05. Again, the subject was required to choose between the act and each certain sum, observing the single switch requirement. A subject's certainty equivalent was coded as the average of the certain sums in last row of the zoomed-in list in which she chose the act and the first row in which she chose the certain sum.

3.5 Incentives

Each subject completed basic lists for ten acts, plus the corresponding zoomed-in lists. They were told at the start that, after they had completed all choices in all choice lists, one such choice would be selected at random to be for real: that is, if they had chosen the certain sum of money in it, they would receive that sum; and, if they had chosen the act, they would receive the outcome of its resolution.[11] This is a form of the *random lottery incentive system*, widely-used in individual choice experiments. It prevents confounding income effects between tasks which might arise if more than one task was paid (likewise, Thaler and Johnson (1990)'s "house money" effects). It is also theoretically incentive-compatible (though for different reasons in each case) under both expected utility theory and prospect theory (Kahneman and Tversky, 1979). It is easy for subjects to understand and, in the current context, allows us to elicit certainty-equivalents without using cognitively more demanding devices such as auctions or forms of the Becker-De Groot-Marschak mechanism (Becker *et al.*, 1964) in which buying or selling "prices" are declared and compared with randomly drawn ones.[12] If, notwithstanding these points, but as hypothesized by Holt (1986), there is some tendency for subjects to see their responses to tasks as constructing a portfolio, that could affect responses to individual

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[11] This worked as follows: The computer selected at random one row from one basic choice-list. If the task in this row was neither the last in which the subject chose the act nor the first in which she chose the certainty, the subject received his choice in the selected row. Otherwise, the computer selected at random a row from the zoomed-in list defined by the subject's choices in the selected basic list, and the subject received his choice in that row. This procedure has the desirable property that the subject's choices have no effect on which row of which basic choice-list is selected by the computer at the first stage; and, if the second stage is reached, no effect on which row of the relevant zoomed-in list is selected. Either way, the subject's choice in the row finally selected would be either a sure sum of money, in which case she received that sum, or the act, in which case she received the final outcome of the resolution of the act. A card was drawn from each deck at the end of the experiment and a 6-sided die was rolled. Together, these resolved all chosen acts. All sums due were paid in cash before subjects left the experiment.

[12] See Cubitt *et al.* (1998), Bardsley *et al.* (2010, Section 6.5), Baltussen *et al.* (2012), Harrison and Swarthout (2012), and Azriely *et al*. (2012) for detailed discussions.

tasks. However, as long as the attractiveness of the mixed act is reduced by background randomization, that would tend to militate against, rather than for, it. In any case, if a portfolio effect is common to the 1-ball and 2-ball conditions, it would not affect comparison of them.

3.6 Sequence of tasks

After the choice-lists for the risky acts to be resolved with Deck 1, subjects completed choice-lists for the ambiguous acts f_1, f_2 and *mix* in the 1-ball condition (Deck 2), followed by choice-lists for the ambiguous acts f_1, f_2, *mix*, g_1 and g_2 in the 2-ball condition (Deck 3). This progression from a risky environment to environments with progressively more complex ambiguity provided a natural sequence, conducive to subjects' understanding.

Our design was constructed to make it "easy" for subjects to express indifference between the acts f_1, f_2 and *mix*. In each condition, all basic choice lists for these three acts were shown and completed side-by-side on the same screen; and subjects then proceeded to the corresponding zoomed-in lists, again with the lists for the three acts side-by-side on the same screen.

After subjects had completed all choice-lists for the mixed act and its constituents in the 1-ball condition and then in the 2-ball condition, they proceeded to a further screen with the basic choice-lists for g_1 and g_2. They were completed side-by-side on the same screen, as were the corresponding zoomed-in lists. As the certainty-equivalents for these acts would be used to categorize subjects by ambiguity attitude (as we explain in the next sub-section), they were elicited last to rule out any possibility that subjects could construct their other choices deliberately to make them consistent with these ones.

3.7 Classification of subjects

As it was resolved with Deck 3, Table 4 and Figure 2 show that g_1 offered 5 chances (out of 10) of a 50-50 die-roll under every possible composition of the deck. In contrast, g_2 would yield the die-roll if a club or a heart was drawn; and the combined number of clubs and hearts was uncertain: specifically g_2 offered 5, 8, 2 and 5 chances (out of 10) respectively of the 50-50 die-roll under the four possible compositions of Deck 3. As the second and third possible compositions of that deck are equally likely, ambiguity aversion requires preference for g_1 over g_2 and ambiguity seeking the reverse preference.[13] We use this fact to classify subjects by ambiguity attitude. Subjects who were indifferent between g_1 and g_2 were classified as ambiguity neutral; and all of the remainder as ambiguity sensitive, with the

[13] Ex ante, we cannot be sure that a subject would appreciate that the second and third possible compositions of Deck 3 are equally likely, even though this is a simple implication of the information we gave them. But, even a subject who did not appreciate the implication would have no obvious reason to treat one of these two compositions as more likely than the other, as the information about them is entirely symmetric. Unless stated otherwise, we use as a maintained hypothesis, that subjects did treat the second and third possible compositions of Deck 3 alike. We discuss the robustness of our conclusions with respect to this point in Appendix A.

latter group divided into ambiguity seeking and ambiguity averse. Since the predictions in relation to preference over g_1 and g_2 are common to the smooth ambiguity and α-MEU models, this procedure is neutral between the two models.

3.8 Predictions and control

We now put the theoretical predictions in the context of the design. For the 2-ball condition, which matches the set-up of Section 2.2, the smooth ambiguity model predicts that those subjects who prefer g_1 to g_2 (ambiguity averse) should also prefer *mix* to each of f_1 and f_2; those who prefer g_2 to g_1 (ambiguity-seekers) should also prefer each of f_1 and f_2 to *mix*; and those indifferent between g_1 and g_2 (ambiguity neutral) should be indifferent between *mix*, f_1 and f_2. In contrast, the α-MEU model predicts that all subjects should be indifferent in the 2-ball condition between the mixed act and each of its constituents, regardless of their preference over g_1 and g_2.

We use the 1-ball condition as a control in several related ways. In the 1-ball condition, the smooth ambiguity model joins the α-MEU model in predicting indifference between f_1, f_2 and *mix* as, in each possible composition of Deck 2, the number of spades equals the number of hearts, making the overall chances of receiving €20 the same under those three acts. If we observe preference for *mix* over its constituents among ambiguity averse subjects in the 2-ball condition, and if the smooth ambiguity model correctly diagnoses the source of that preference, the preference should be absent in the 1-ball condition. However, it is possible that subjects will be attracted (or repelled) by the mixed act relative to its constituent acts for reasons other than the hedging argument of the smooth ambiguity model. For example, subjects might have an attitude, positive or negative, towards the presence in the resolution of the mixed act of a source of uncertainty, die-rolling, in addition to the drawing of cards from decks. But, if so, this should show up in both the 2-ball and the 1-ball conditions. Thus, the difference between the two conditions is of particular interest, regardless of whether we observe the predicted indifference in the 1-ball condition.

To build on these points, we now define variables used in our data analysis. We use $CE(f, C)$ to denote the certainty equivalent of act f in condition C (though we omit the condition where obvious from the context) and we use $AvCE(f, g, C)$ to denote the (arithmetic) mean of a subject's certainty equivalents for acts f and g in condition C. The following "premium" variables can then be defined for each subject:

Mixed act premium (2-ball)	=	$CE(mix, \text{2-ball}) - AvCE(f_1, f_2, \text{2-ball})$;
Mixed act premium (1-ball)	=	$CE(mix, \text{1-ball}) - AvCE(f_1, f_2, \text{1-ball})$;
2-ball premium	=	$CE(mix, \text{2-ball}) - CE(mix, \text{1-ball})$;
Difference between mixed act premia	=	*Mixed act premium (2-ball) – Mixed act premium (1-ball).*

Mixed act premium (2-ball) measures the excess attractiveness of *mix* over its constituents in the condition where the smooth ambiguity model makes its distinctive prediction that ambiguity averse subjects prefer the mixed act and ambiguity seeking subjects the constituent acts. Mixed act premium (1-ball) measures the corresponding excess attractiveness in the condition where both models predict that all three types are indifferent between *mix* and its constituents. The variable "difference between mixed act premia" measures how far "excess attractiveness" of *mix* over its constituents is greater in the 2-ball condition than it is in the 1-ball condition. Thus, it measures the influence of the hedging of independent ambiguities consideration, controlling for any other factors that (contrary to both models being considered) may make *mix* either more or less attractive than its constituent acts in the 1-ball condition. Finally, the 2-ball premium measures directly the extent to which *mix* is more attractive when it does offer a hedge across independent ambiguities than when it does not.

According to the smooth ambiguity model, all of these premium variables should be positive for the ambiguity averse, zero for the ambiguity neutral, and negative for the ambiguity seeking, except for mixed act premium (1-ball), which should be zero for all three types. The predictions of the α-MEU model are simpler to state: each of the four premium variables should be zero for all types. Finally, SEU theory implies ambiguity neutrality and zero values of all four premium variables.

4. Results

4.1 Preliminaries

The experiment was conducted at the University of Tilburg. 97 subjects took part, all of whom were students of the university.[14] They were paid a show-up fee of €5 on top of their earnings from the tasks, yielding a total average payment of €15.74. The main function of the risky acts resolved with Deck 1 was to enhance subjects' understanding of subsequent ones, but we report that the median certainty equivalents for 70% and 30% chances, respectively, of €20 were €11.73 and €5.58, suggesting levels of risk aversion not uncommon among experimental subjects. We now turn to ambiguous acts.

4.2 Results on classification of subjects

Certainty equivalents for g_1 and g_2 allow us to categorize subjects into three *types*: the ambiguity averse ($CE(g_1) > CE(g_2)$); the ambiguity neutral ($CE(g_1) = CE(g_2)$); and the ambiguity seeking ($CE(g_1) < CE(g_2)$). Out of a total of 97, the numbers of subjects of each type were 31, 50 and 16 respectively.[15]

[14] We exclude from these figures and our data analysis one subject who always chose the same option.

[15] We use this typology, simply noting at this point the qualification that, strictly, $CE(g_1) - CE(g_2)$ is only a sufficient statistic for identifying a subject's attitude to ambiguity when the shared prediction of the smooth

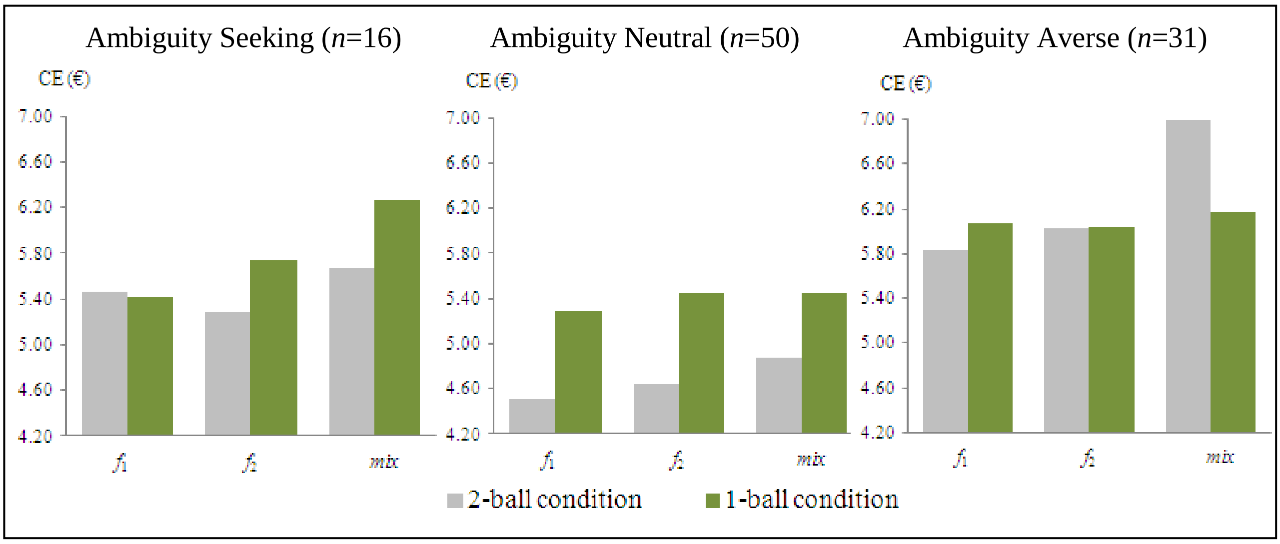
Although some studies have found a higher proportion of ambiguity sensitive subjects than we do, Ahn *et al*. (2011) found that 79% of their subjects were ambiguity neutral. Recall that our design was constructed to make it "easy" to reveal indifference between certain sets of acts the certainty equivalents of which were elicited side-by-side on the same screen. A subject who saw a relationship between two such acts which she regarded as making them equally attractive would have had no difficulty in giving certainty equivalents that reflected that judgment. From this perspective, the proportion of subjects coded as ambiguity neutral is actually quite encouraging, even though it lowers the proportion coded as ambiguity sensitive. As subjects clearly were able to express indifference between g_1 and g_2, there is no reason to think they would not have been able to do so between *mix* and its constituents (the certainty equivalents of which were also elicited side by side on the same screen) if they saw fit.[16]

Notwithstanding ambiguity neutral being the largest group, the mean difference $CE(g_1) - CE(g_2)$ was €0.45 across all subjects, reflecting some ambiguity-aversion on average. The corresponding figures for the two ambiguity sensitive types were €1.90 for the ambiguity- averse and –€0.93 for the ambiguity seeking.

4.3 *Comparing certainty equivalents: central tendencies*

As an initial display of our findings, Figure 3 reports the mean certainty equivalents, for each ambiguous act under each condition, separately by type of subject.

Figure 3: Mean CE's for Ambiguity Seeking, Neutral, and Averse Subjects



ambiguity, α-MEU and SEU models that $CE(f_1\text{,2-ball}) = CE(f_2\text{,2-ball})$ holds. We return to this issue in Appendix A.

[16] In further support of this: Out of 97 subjects, 71 gave *identical* certainty equivalents for f_1 and f_2 in the 2-ball condition, in line with theoretical predictions. (We return to the others in Appendix A.) For comparison, the only case we have where all theories would predict a difference between certainty equivalents is that of the two risky acts; here, just 7 subjects gave identical certainty equivalents.

As explained in Section 3.8, the most important features of our data are comparisons between certainty equivalents and the premium variables defined in terms of them, especially where they exploit the 1-ball condition as a control alongside the 2-ball condition. The mean, median and standard deviations of each of the four premium variables are reported Table 5.

Table 5: Premia (in €, rounded to nearest cent)

	Ambiguity Seeking (*n*=16)				Ambiguity Neutral (*n*=50)				Ambiguity Averse (*n*=31)		
	Mean	Median	St. Dev.		Mean	Median	St. Dev.		Mean	Median	St. Dev.
Premia											
Mixed act (2-ball)	0.30	0.46	2.68		0.29	0.00	1.58		1.06	0.73	2.63
Mixed act (1-ball)	0.69	0.00	2.53		0.09	0.00	2.02		0.12	0.00	2.39
2-ball	-0.60	-0.43	1.85		-0.58	0.00	2.13		0.83	0.55	2.53
Difference between mixed act premia	-0.39	-0.64[17]	2.01		0.21	0.00	2.00		0.95	0.30	2.39

Several points stand out from Figure 3 and Table 5. If, first, we confine attention to subjects coded as ambiguity averse, then the findings are, at eyeball level, very much in line with the predictions of the smooth ambiguity model. In particular, Figure 3 shows that, for these subjects, *mix* seems to have been judged on average notably more attractive than its constituents in the 2-ball condition, but not in the 1-ball condition. Table 5 indicates that, for the ambiguity averse, the mixed act premium (2-ball) and the 2-ball premium are both, on average and by median, positive and seemingly non-trivial; whereas the central tendencies of mixed act premium (1-ball) are close to zero.[18]

For ambiguity averse subjects, one-tailed Wilcoxon signed-rank tests reject the equality of *CE*(*mix*,2-ball) with each of *CE*(f_1,2-ball) and *CE*(f_2,2-ball), and also reject the equality of *CE*(*mix*,2-ball) with *CE*(*mix*,1-ball), in each case at conventional levels of significance.[19] In contrast, we cannot

[17] Among the ambiguity seeking, median difference between mixed act premia is negative, though median mixed act premium (2-ball) is positive and median mixed act premium (1-ball) zero. This is not a typo, but just a sharper reflection of the fact, also evident for the ambiguity averse, that median (*x*-*y*) may not equal (median *x*) – (median *y*).

[18] In interpreting the median values in Table 5, recall that our design makes it easy to reveal certain indifferences and that we only have interval estimates of certainty equivalents, so median values of precisely zero are less surprising than they might otherwise seem.

[19] p-values are 0.0059 for rejection of the null hypothesis *CE*(f_1,2-ball) ≥ *CE*(*mix*,2-ball), 0.0083 for rejection of the null hypothesis *CE*(f_2,2-ball) ≥ *CE*(*mix*,2-ball), and 0.0372 for rejection of the null hypothesis *CE*(*mix*,1-ball) ≥ *CE*(*mix*,2-ball). Only in the third case would the corresponding two-tailed test fail to reject a null of equality between the two variables, and then only marginally so (p = 0.0744).

reject equality of *CE*(*mix*, 1-ball) with either *CE*(f_1,1-ball) or *CE*(f_2,1-ball).[20] Thus, there is clear evidence, at the level of central tendencies, in favor of the hypothesis that ambiguity averse subjects value the hedge against independent ambiguities that *mix* offers over its constituents in the 2-ball condition but that, as also predicted by the smooth ambiguity model, this attraction to *mix* disappears in the 1-ball condition, where the ambiguities are not independent.

In the case of subjects coded as ambiguity neutral, all theories agree. The medians of each of the premium variables are exactly as predicted by the theories. However, it is surprising that ambiguity neutral subjects seem from Figure 3 to prefer each of the acts in the 1-ball condition over the same act in the 2-ball condition, as Wilcoxon signed-rank tests confirm.[21] The reason for this is unclear but, one possibility is that some subjects are averse to greater numbers of possible compositions of the deck. Whatever the reason, as the effect favors the 1-ball version, it does not seem to indicate any factor that would contribute to our earlier finding that ambiguity averse subjects prefer the 2-ball version of *mix* over its constituents. Indeed, if anything, it strengthens that finding.

Our findings for subjects coded as ambiguity seeking are more mixed than those for the ambiguity averse. For example, for ambiguity seeking subjects, the mean and median values of the mixed act premium (2-ball) both have the wrong sign from the perspective of the smooth ambiguity model, though statistically we cannot distinguish *CE*(*mix,*2-ball) from either *CE*(f_1, 2-ball) or *CE*(f_2, 2-ball). However, the mean and median of the 2-ball premium have the correct sign from the perspective of the smooth ambiguity model, but we cannot reject a null of equality of *CE*(*mix,*2-ball) and *CE*(*mix,*1-ball). Similarly, the mean and median of difference between mixed act premia have the correct sign from the perspective of the smooth ambiguity model, so there is some indication that the *relative* attractiveness of *mix,* compared to its constituents, differs across conditions in the direction predicted by the smooth ambiguity model, but the effect is not statistically significant. Indeed, given the small number of ambiguity seeking subjects, it is difficult to detect a reliable pattern in their behavior.

To summarize, at the level of central tendencies, the predictions of the smooth ambiguity model perform notably better than those of the α-MEU model when we consider the ambiguity averse, where the models conflict. Where the models agree (i.e., in the case of the ambiguity neutral and in the case of the mixed act premium (1-ball) for all types) the median premium values are in all cases zero, as predicted. For the ambiguity seeking, the two models again conflict, but our findings are less clear cut than in the case of the ambiguity averse. For the ambiguity seeking, in line with the α-MEU model, there are no statistically significant differences. However, there are relatively few ambiguity

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[20] p-values for Wilcoxon signed-rank tests of the null hypothesis of equality are 0.6571 and 0.6348 respectively.

[21] p-values for rejection of null hypotheses *CE*(*mix*,1-ball) = *CE*(*mix*,2-ball), *CE*(f_1,2-ball) = *CE*(f_1,1-ball) and *CE*(f_2,2-ball) = *CE*(f_2,1-ball) are, respectively, 0.0285, 0.0095, and 0.0008.

seeking subjects and what differences there are take the signs predicted by the smooth ambiguity model, *provided* one exploits the control offered by the 1-ball condition.

4.4 Categorical analysis

The analysis of the previous subsection is subject to two limitations. Firstly, it concentrates on *magnitudes* of certainty equivalents and premium variables, whereas the theoretical predictions are really about *ordinal* comparisons of certainty equivalents (and hence about signs of the premium variables). Secondly, as it focuses on the "typical" subject in each type, it does not fully capture the proportion of subjects in a given type conforming to a given prediction. In this sub-section, we present a brief categorical analysis that addresses these points.

We define (with slight abuse of terminology) the *sign* of a variable as taking one of three values: strictly positive, zero, and strictly negative. Table 6 presents contingency tables for the sign of $CE(g_1) - CE(g_2)$ (i.e. the subject's type) against the sign of each of mixed act premium (2-ball), 2-ball premium, and difference between mixed act premia.[22] Table 6 gives the frequencies of subjects with each combination of type and sign of premium variable, for each premium variable. The first number in each cell gives the absolute number, for each frequency.

As it may be "difficult" for a subject to achieve a value of precisely zero for a given premium variable, we also consider an alternative coding. We have already argued that subjects seemed to have no difficulty in achieving $CE(g_1) = CE(g_2)$, as these two certainty equivalents were elicited side-by-side on the same screen. (For this reason, we use the exact equality when classifying subjects as ambiguity neutral.) But, that argument is less compelling for the premia. To achieve a 2-ball premium of zero requires declaring identical certainty equivalents in choice-lists presented on different screens. To achieve a mixed act premium (2-ball) of zero requires setting one certainty equivalent equal to the average of two others, which is hard (and, given elicitation by intervals, perhaps impossible) when the two others are not equal to each other. Finally, achieving a difference between mixed act premia of zero requires having two premia, each of which is the difference between one certainty equivalent and the average of two others and each of which is elicited on a different screen, precisely equal to one another. In view of these points, Table 6 also indicates parenthetically the frequencies under a revised coding scheme in which a sign of zero is attributed to the premium variable if its absolute value is no more than €0.20 from zero. Unsurprisingly, this pulls more observations into the central rows of each panel of Table 6. It is quite a generous adjustment for theories predicting premium variables of zero, in that €0.20 corresponds to four rows of a zoomed-in choice-list.

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[22] We ignore mixed act premium (1-ball) here, as it does not distinguish between the theories under consideration.

Table 6: "Signs" of Premia and Ambiguity Attitude

		$CE(g_1) - CE(g_2)$ Ambiguity attitude		
		< 0 (Seeking)	0 (Neutral)	> 0 (Averse)
Mixed act premium (2-ball)	>0	9 (9)	13 (11)	21 (20)
	0	1 (2)	26 (31)	3 (5)
	<0	6 (5)	11 (8)	7 (6)
2-ball premium	>0	4 (4)	12 (7)	19 (17)
	0	0 (4)	15 (23)	2 (6)
	<0	12 (8)	23 (20)	10 (8)
Difference between mixed act premia	>0	5 (5)	16 (16)	19 (17)
	0	1 (1)	19 (21)	4 (7)
	<0	10 (10)	15 (13)	8 (7)

According to the smooth ambiguity model, each subject's type (revealed by sign of $CE(g_1) - CE(g_2)$) should match the sign of their premium variable, for each of the three premium variables presented in Table 6. To capture the extent of conformity with this prediction, we calculate, for each of the premium variables in Table 6, the *sign-matching rate*, defined as the percentage of subjects for whom type matches the sign of the premium variable. Correspondingly, for each premium variable, we also calculate the *sign-zero rate*, defined as the percentage of subjects for whom the sign of the premium variable is coded as 0, in accordance with the α-MEU model.

Given our coding and the classification of outcomes by sign, differences between the sign-matching and sign-zero rates, for a given premium variable, may be interpreted as differences in Selten's index of predictive success (Selten, 1991) for the smooth ambiguity and α-MEU models, as they both have the same "parsimony" – that is, each permits observations in three out of nine cells of the panel of Table 6 pertaining to the relevant premium variable.

Table 7 reports both rates, for each of the premium variables from Table 6, separately for all subjects; ambiguity sensitive subjects; ambiguity seeking subjects; and ambiguity averse subjects. Rates are given to the nearest percentage. As with Table 6, un-parenthesized entries correspond to the stricter coding rule for a zero sign on the premium variable and parenthesized entries to the looser coding.

By construction, the looser coding rule for a sign of zero on the premium variable cannot lower the sign-zero rate. In fact, as Table 7 shows, it raises that rate in all cases but one, in some cases substantially so. In contrast, the looser coding rule sometimes raises and sometimes lowers the sign-matching rate; and most (but not all) of these adjustments are quite small. In terms of the comparative performance of the smooth ambiguity and α-MEU models for a given premium variable and group of subjects, what matters is the difference between the sign-matching and the sign-zero rate. This is always reduced (or, in one case, left unchanged) by the looser coding rule for the latter rate.

Table 7: Sign-matching and Sign-zero Rates (%) by Premium Variable

		Subjects			
Premium variable	Rate: Sign-…	All (*n*=97)	Ambiguity sensitive (*n*=47)	Ambiguity seeking (*n*=16)	Ambiguity averse (*n*=31)
Mixed act (2-ball)	Matching	55 (58)	57 (53)	38 (31)	68 (65)
	Zero	31 (39)	9 (15)	6 (13)	10 (16)
2-ball	Matching	47 (49)	66 (53)	75 (50)	61 (55)
	Zero	18 (34)	4 (21)	0 (25)	6 (19)
Diff. between mixed act premia	Matching	49 (49)	62 (57)	63 (63)	61 (55)
	Zero	25 (30)	11 (17)	6 (6)	13 (23)

However, even using the looser of our codings, the sign-matching rate exceeds the sign-zero rate in every case reported in Table 7, by a margin never lower than 15 percentage points. If attention is restricted to ambiguity sensitive subjects then, even on the coding that favors the zero-sign rate, the sign-matching rate exceeds it by 38 percentage points, 32 percentage points and 40 percentage points for the mixed-act premium (2-ball), the 2-ball premium, and the difference between mixed act premia, respectively. In this respect, the smooth ambiguity model outperforms the α-MEU model.

That said, the performance of the smooth ambiguity model in Table 7 is far from perfect. The sign-matching rates reported in the All column are only around 50%. Restricting attention to ambiguity sensitive subjects raises these rates, for given coding and a given premium variable, with one exception[23] and some of these increases are quite marked. For example, on the un-parenthesized coding, moving from consideration of all subjects to consideration of ambiguity sensitive subjects raises the sign-matching rate from 47% to 66% for the 2-ball premium and from 49% to 62% for the difference between mixed-act premia.

The categorical analysis of this sub-section broadly coheres with the consideration of central tendencies in the previous one. Though neither model captures all aspects of our data, there are striking features of the data that conform more closely to the smooth ambiguity model than to the α-MEU model in respect of whether subjects react in the predicted ways to the opportunity to hedge independent ambiguities.

[23] The exception is mixed act premium (2-ball) on the looser coding.

5. Extensions

Before concluding, we comment on how our analyses are extended in the first three appendices that follow. As noted in Section 3.7, our categorization of subjects assumes that they see the second and third compositions of Deck 3 as equally likely, as implied by the information provided. Appendix A shows that the main conclusions of our analysis are robust to the relaxation of this assumption. A different possible concern about our analysis, in view of the findings of previous experiments in the literature, is its reliance on expected utility theory as the model of choice under objective risk. In this respect, our analysis is true to the smooth and α-MEU models, as usually formulated. Notwithstanding this, in Appendix B, we show that our testing strategy would be robust to a reformulation of these models in which expected utility theory is replaced in this role by a range of non-expected utility models. In Appendix C, we substantiate the point made in Section 1 that the α-MEU and smooth ambiguity models stand as examples of broader classes of models of ambiguity sensitive preference: that is, respectively, of models of preferences whose representations focus exclusively on minimum and/or maximum expected utilities and of models whose representations also give weight to non-extremal expected utilities.

6. Concluding Remarks

Interpreting our findings narrowly in terms of a comparison of the smooth ambiguity and α-MEU models, we find that, while neither model captures all aspects of our data, there are striking features of the data that conform more closely to the smooth ambiguity model than to the α-MEU model.

However, as noted in Sections 1 and 5, there is a broader theoretical context to our findings. According to the broader interpretation, the two classes of models we distinguish belong to different periods in the history of modeling attitude to ambiguity. The more recent models are more complex than their predecessors but also predict distinctive, nuanced responses to ambiguity. Our test for distinguishing between models focuses on one such response, namely dependence of a preference for hedging separate ambiguities on ambiguity attitude; and our findings provide evidence of precisely this phenomenon. Thus, reassuringly for theoretical development, our findings suggest some empirical support for a distinctive and economically meaningful qualitative prediction of the more recent generation of models, notwithstanding their complexity. As we explain in Appendix C, this support is not confined to the smooth ambiguity model.

Finally, our results may be seen to provide a welcome reassurance to the theories of ambiguity aversion quite generally. It has long been argued, at a theoretical level, that reliance on stochastic mixing as a way to hedge ambiguity is a defining part of a rational response to ambiguity. Indeed, such is the motivating basis of Schmeidler's (1989) *Uncertainty Aversion* axiom, which lies at the heart of ambiguity averse preference models, quite generally (Cerreia-Vioglio *et al*., 2011). However, the only other experimental study we are aware of which investigates the link between ambiguity aversion and preference for randomization, Dominiak and Schnedler (2011), finds little support for this central premise of theories of ambiguity aversion. In view of this, it is particularly notable that our main finding, specifically strict preference for *mix* over f_1 and f_2 on the part of the ambiguity averse in the 2-ball condition, supports the Uncertainty Aversion axiom.[24] Hence, a further overall "take-away" from our findings, is that they provide evidence for a link between ambiguity aversion and propensity to use randomization to hedge ambiguity, and thus for the central foundational principle of theories of ambiguity aversion, quite generally.

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[24] Consider ambiguity averse DMs. An α-MEU preference and a smooth ambiguity preference both satisfy Schmeidler's Uncertainty Aversion axiom. The axiom implies that there will be instances when, given two acts between which the DM is indifferent, the mixture between the acts is strictly preferred to either act, delineating the characteristic departure of ambiguity aversion from behavior satisfying the (Anscombe and Aumann) SEU theory. However, the axiom does not further stipulate what those instances will be. Our acts *mix*, f_1 and f_2 in the 2-ball condition provide an instance where a smooth ambiguity model implies a strict preference while an α-MEU model predicts indifference. (In contrast, both models predict strict preference for the mixture of two bets, one on each color of an Ellsberg 2-color ambiguous urn, over its constituents.) Hence, had we found that subjects coded as ambiguity averse were largely indifferent between our three acts in the 2-ball condition then that would be evidence neither in support nor against Schmeidler's axiom, whereas the strict preference that what we do find is positive support for the axiom.

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Appendix A: Robustness

Our classification of subjects as ambiguity averse, ambiguity neutral or ambiguity seeking used a procedure that, strictly, relies on subjects being indifferent, in the 2-ball condition, between f_1 and f_2. In fact, for 71 subjects, the condition $CE(f_1$,2-ball) = $CE(f_2$,2-ball) holds *exactly*. For the remaining 26 subjects, the statistic $CE(g_1) – CE(g_2)$ is well-defined, but no longer sufficient to identify a subject's ambiguity attitude. In this appendix we comment on how that affects our analysis, in particular, the robustness of our conclusions regarding the comparison of models. To deal with the possibility of non-indifference between f_1 and f_2, we use a set of predictions of the models that are not predicated on the indifference.

For purposes of this discussion, we fix the domain of preferences to include those described by:

1. an α-MEU model with the set of probabilities in the representation given by $\mathcal{P} = \{p_i \mid i = 1,\ldots,4\}$, or its convex hull, where p_i refers to the probabilities described in Table 2;
2. an SEU model (specifically, an Anscombe-Aumann model) which puts positive weight on each of the four probabilities, p_i, $i = 1,\ldots,4$;
3. a smooth ambiguity model with a non-linear ϕ and a μ which puts positive weight on each of the four probabilities, p_i, $i = 1,\ldots,4$;

Within this domain, the α-MEU model predicts indifference between f_1 and f_2 and *mix* regardless of preference over g_1 and g_2. In contrast, both the smooth ambiguity and SEU models allow non-indifference between f_1 and f_2 if p_2 and p_3 are weighted unequally. Importantly, they also require that the directions of a subject's preference over g_1 and g_2 and her preference over *mix* and f_2 match, as can seen from an inspection of the relevant rows of Table 3. However, SEU imposes an additional restriction that the smooth model does not, namely, that the direction of preference between f_1 and f_2 must also match that between g_1 and g_2. Under the smooth ambiguity model, non-linearity of ϕ could upset this correspondence, thereby distinguishing SEU from smooth ambiguity. In fact, of the 26 subjects who are not indifferent between f_1 and f_2 in the 2-ball condition, only 5 conform to the additional restriction imposed by SEU.

Thus, if we set SEU aside and focus exclusively on the two models of ambiguity sensitivity, we may use a comparison of the preferences between *mix* and f_2 and between g_1 and g_2 as a means of discriminating between the models, even when the subject is not indifferent between f_1 and f_2. Hence, our strategy for comparing the α-MEU and smooth ambiguity models is a simple modification of our analysis in the main part of the paper. We replace $AvCE(f_1, f_2$, 2-ball) with $CE(f_2$, 2-ball) in the definition of the mixed act premium (2-ball), noting that this also has a knock-on effect on the definition of difference between mixed act premia. For a given sign of $CE(g_1) – CE(g_2)$, the predictions of the smooth ambiguity model for the signs of the two re-defined premium variables are exactly as for the original definitions. Similarly, for the α-MEU model, the implications for the signs of the two re-defined premium variables are exactly as for the original definitions, since the model predicts indifference between *mix* and f_2 for any value of $CE(g_1) – CE(g_2)$. These points are unaffected by whether $CE(g_1) – CE(g_2)$ indicates ambiguity attitude.

The redefinition can only affect subjects for whom $CE(f_1$,2-ball$) \neq CE(f_2$,2-ball$)$. (These subjects are spread roughly evenly across the three categories by sign of $CE(g_1) – CE(g_2)$.) The sign of the mixed act premium (2-ball) is changed by the redefinition in only nine cases and the sign of the difference between mixed act premia in only three. Clearly, the latter point makes little difference, so we focus on the former, using the stricter coding rule for a zero sign on a premium, because the redefinition of the mixed act premium (2-ball) makes it the difference of two certainty equivalents elicited next to each other. For the mixed act premium (2-ball), the number of subjects with a zero sign rises from 30 to 35, whereas the number with a sign matching that of $CE(g_1) – CE(g_2)$ falls from 53 to 51. Though this impact of the redefinition slightly favors the α-MEU model, the sign-matching rate for all subjects still exceeds the sign-zero rate by more than 16 percentage points. Thus, the main qualitative conclusions of the categorical analysis (Section 4.4) are not affected. Moving beyond signs, the median mixed act premium (2-ball) rises by 2 (resp. 0) euro cents among subjects with $CE(g_1) – CE(g_2) >$ (resp. =) 0. Hence, for these subjects the modification makes very little difference to the central tendency. But, among those with $CE(g_1) – CE(g_2) < 0$, the median mixed act premium (2-ball) rises from €0.46 to €0.85. This is a movement in the wrong direction for *both* smooth and α-MEU models. Hence, it does not affect the conclusions that we drew from our analysis of the central tendencies of certainty equivalents in Section 4.3, about the *relative* performance of those models.

Appendix B: Non-Expected Utility for Risk

A possible concern about our analysis is its reliance on expected utility theory as the underlying model of choice under risk. In this respect, our analysis is true to the smooth ambiguity and α-MEU models as usually formulated. But, as there is evidence from many experiments that subjects deviate from expected utility theory under risk (Starmer, 2000), one might therefore wonder how our analysis would be affected if this were true of our subjects too. As a theoretical matter, it is possible to imagine more general formulations of these models which allow an induced probability distribution on consequences to be evaluated by a quite general non-expected utility functional (see, for example, the discussion of Corollary 1 in KMM). In this appendix, we show that our testing strategy for distinguishing between the α-MEU and the smooth ambiguity models, and our conclusions about the relative support of each model in the data, would be robust to such reformulation.

An act together with a *given* probability distribution on the state space induces a probability distribution on consequences. Since consequences may be lotteries, the induced distribution is, in general, a distribution over lotteries and hence, a two-stage lottery. Both the α-MEU and the smooth ambiguity model, in their standard formulations, evaluate such an induced distribution just as would an expected utility model (see, e.g., Klibanoff *et al.* (2012), Equation 1.1 and 1.2). In particular, the induced two-stage lottery is evaluated by reducing it to the corresponding one-stage lottery and computing the expected utility of the reduced lottery. We now consider a reformulation of the evaluation of such induced probability distributions that is more general than expected utility.

For brevity, we consider only a domain of induced probability distributions comprising one-stage and two-stage objective lotteries defined on the set of final monetary consequences used in our experiment, i.e. on {€20, €0}. Let preferences over such lotteries be represented by maximization of *any* real-valued function *V*, defined on the lotteries, such that (i) preferences respect first-order stochastic dominance; and (ii) *V* satisfies the following *Limited Reduction* condition: *V*({{€20, ½; €0, ½}, *r*; {€0,1}, 1 – *r*}}) = *V*({€20, *r*/2; €0, 1 – (*r*/2)} for any 1 > *r* > 0. Notice that, Limited Reduction is much weaker than the standard reduction principle for compound lotteries and does not, *by itself*, impose any restriction on preferences over *one-stage* lotteries. It simply links preferences over two-stage lotteries of a particularly simple form to preferences over particular one-stage lotteries, in the way specified. This would be quite compatible, for example, with preferences over one-stage lotteries being generated by cumulative probability-weighting with a distorted (monotonic) weighting function.

We now generalize the α-MEU and smooth ambiguity models by using maximization of *V*, defined as above, in place of maximization of the expectation of *u*, as their representation of preferences with a *given* probability distribution on the state space. How are the acts considered in the experiment evaluated under this generalization? To compress notation, let *V*({€20, 0.4; €0, 0.6}) = *x*, *V*({€20, 0.25; €0, 0.75} = *y*, and *V*({€20, 0.1; €0, 0.9}}) = *z*. From first-order stochastic dominance, $x > y > z$. Moreover, applying Limited Reduction where necessary, the values of V for each act and deck composition are given in Table A1 (to be compared with Table 3 in the main text).

Table A.1: Values of V

	p_1	p_2	p_3	p_4
f_1	z	z	x	x
f_2	z	x	z	x
mix	z	y	y	x
g_1	y	y	y	y
g_2	y	x	z	y

From here, the theoretical analysis of the 2-ball condition can proceed essentially just as in the main text. For the α-MEU model, as generalized in the preceding paragraph, the DM's preferences must satisfy $f_1 \sim f_2 \sim mix$. For the smooth ambiguity model, as generalized in the preceding paragraph (but imposing, as in the paper, that $\mu(p_2) = \mu(p_3)$), the DM's preferences must satisfy the conditions that $f_1 \sim f_2$; and that, for $f \in \{f_1, f_2\}$, $g_1 \succ g_2 \Leftrightarrow mix \succ f$; $g_1 \sim g_2 \Leftrightarrow mix \sim f$; and $g_1 \prec g_2 \Leftrightarrow mix \prec f$. Thus, the predictions of Section 2.2 about how preferences over acts in $\{f_1, f_2, mix\}$ are (or are not) related to preferences over $\{g_1, g_2\}$ under the two models are robust to the generalizations of the preceding paragraph. What matters for those predictions is just that preferences over one-stage lotteries satisfy first-order stochastic dominance and cohere with preferences over two-stage lotteries to the extent required by Limited Reduction. This is compatible with departures from expected utility theory in preferences over one-stage objective risk, even quite marked departures.

Appendix C: Divide Across Models

For concreteness in the main text, it was convenient to analyze and interpret the results purely in terms of the α-MEU model and the smooth ambiguity model. However, the divide between models addressed by our design is broader, as we explain in this appendix. Consider the formal representation of the setting in our experiment. Think of $\mathcal{P}$ as the set of probabilities (on the set of states $S = \{B_1, B_2, R_1, R_2\}$) in an α-MEU model, or the set of probabilities in the core of the convex capacity in the representation of a Schmeidler (1989) model of uncertainty aversion, or the support of μ in a smooth ambiguity model. Let Z denote a component, $Z = B, R$. With slight abuse of terminology let B_1 and R_1 be the "first" elements of their respective component, and B_2 and R_2 be the "second" elements. (For interpretation, B_1 equals spade, B_2 equals club, R_1 equals heart, R_2 equals diamond.) Let Z_i denote the *i*-th element from the Z^{th}-component, $i = 1,2$. Denote the set of probabilities of drawing a first element from the Z^{th}-component by $\Gamma_Z = \{p(Z_1): p \in \mathcal{P}\}$. Let $\mathcal{P}$ satisfy the following properties:

Property One: $\Gamma_R = \Gamma_B$.
Property Two: Γ_Z is non-singleton, for $Z = \{B, R\}$.
Property Three: If $q \in \Gamma_R$ and $q' \in \Gamma_B$, there is $p \in \mathcal{P}$ such that $p(R_1) = q$ and $p(B_1) = q'$.
Property Four: If $p \in \mathcal{P}$ then $p(\{Z_1, Z_2\}) = 0.5$.

In our experiment, in the 2-ball condition that implements the theoretical set up of Section 2.2, the information given to subjects satisfies all four properties. Proposition 3.1 in Klibanoff *et al*. (2012) shows how the predictions of the α-MEU model and the smooth ambiguity model differ when Properties One through Four are assumed. [25, 26] Preferences of the Schmeidler (1989) model under *uncertainty aversion* (i.e., with a convex *capacity* representation) coincide with those in an MEU model where the representation set of probabilities is the *core* of the convex capacity.[27] Hence, a

[25] For the case of the smooth ambiguity model, Proposition 3.1 further assumes that the weights μ are uniform. This assumption is, of course, consistent with the information given to subjects under both 1-ball and 2-ball conditions. But the assumption is not necessary for the application to the theoretical setup of Section 2.2 provided $\mu(p_2) = \mu(p_3)$).

[26] As was noted in Footnote 2, the formal details of the set up in Klibanoff *et al*. (2012) are slightly different from what we have here. In particular, they have a product state space, unlike here. So, Property Four stated here does not apply literally to their setup. However, the belief about each component of the product space is unambiguous: it is a probability known to the DMs. Hence, the substantive element of Property Four is implicitly assumed in Proposition 3.1, even though the assumption is not explicitly stated in the proposition.

[27] A convex capacity is a set function $\nu: 2^S \to [0,1], \nu(E) \geq 0, \nu(S) = 1, \nu(E \cup F) \geq \nu(E) + \nu(F) - \nu(E \cap F)$. Every convex capacity ν has an non-empty *core*, a compact, convex set of probability measures defined as follows: $\mathcal{P}(\nu) \equiv Core(\nu) \equiv \{p \in \Delta(S) \mid p(E) \geq \nu(E), for\ all\ E \subset (S)\}$, where $\Delta(S)$ denotes the set of all probability measures on S. Furthermore, $\nu(E) = min_{p \in \mathcal{P}(\nu)} p(E)$. A *belief function* (also known as a *totally monotone capacity*) satisfies a stronger version of the third property specified for a convex capacity: for every

Schmeidler model under uncertainty aversion whose convex capacity in the representation has a core that satisfies Properties One through Four, will share the predictions of an α-MEU (with $\alpha = 1$) as specified in Proposition 3.1. In particular, this implies that there will be no strict preference for the mixed act over its constituents in the 2-ball condition. An example of a capacity whose core satisfies the four properties is a belief function $\nu: 2^S \to [0,1]$, satisfying the following further conditions: $\nu(Z_1) = r = \nu(Z_2), 0.2 \geq r > 0$; $\nu(\{Z_1, Z_2\}) = 0.5$. [28]

However, under the 1-ball condition, the subject's information violates Property Three, since the information implies that $p(R_1) = p(B_1)$. Under such a restriction the ambiguity in each component resolves in an identical way implying mixing cannot in any way help with ambiguity hedging. As Klibanoff *et al*. (2012) point out, in this case the smooth model will, like the α-MEU model, also predict indifference to mixing.

Notice, given the set of probability distributions $\{p_1, \ldots, p_4\}$ in Table 2, if a DM weights the distributions uniformly, the reduced probability measure on S under the 1-ball condition is the same as that under the 2-ball condition.[29] Thus, under the information available to the subjects it is natural to expect that a probabilistically sophisticated DM (Machina and Schmeidler (1992)) will choose the same way under the 1-ball and 2-ball conditions. An example of a probabilistically sophisticated DM is a rank dependent utility maximizer à la Quiggin (1982). A rank dependent utility preference is another prominent member of the class of preferences representable as a Choquet expected utility. Such a preference is represented by a capacity obtained via a distortion of the probability measure on outcomes induced (jointly) by the given probability measure on S and an act. Hence, a subject in the experiment with such preferences, given the information available, should not choose differently under 1-ball and 2-ball conditions, since the reduced probability measure on states is identical under both conditions. Thus, these preferences do not predict a difference between the premia under the two conditions. In this sense, the prediction rank dependent utility is similar to that of an MEU preference rather than to a smooth ambiguity preference. Thus, two very prominent types of CEU preferences,

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$n > 0$ and every collection $E_1, \ldots, E_n \in 2^S$, $\nu(\bigcup_{i=1}^n E_i) \geq \sum_{I \subseteq \{1,\ldots,n\}, I \neq \emptyset} (-1)^{|I|+1} \nu(\bigcap_{i \in I} E_i)$, where $|I|$ denotes the cardinality of I.

[28] In Jaffray (1989), the model is a special case of α-MEU in that the set of probabilities is restricted to be given by the core of a belief function. In Jaffray's presentation, the set of probabilities represent objectively given imprecise information. Gul and Pesendorfer (2013) axiomatize a fully subjective, "Savage-style" version of Jaffray's "von Neumann-Morgenstern style" model. Olszewski (2007) posits and axiomatizes an α-MEU style model with an objectively given set of probabilities (on outcomes). Gajdos *et al*. (2008)'s "contraction" model, which falls in the MEU class, also takes the set of probabilities as objectively given to the DM. All these models share the prediction of the MEU model in our experiment.

[29] Actually, the assumption of a uniform distribution is not necessary for the conclusion in the context of our experiment. The reduced probability on suits is the same in the 1-ball and the 2-ball conditions, for any subject who obtains it by (standard) reduction from probability distributions over the possible compositions *and for whom the latter conform with the information given.* The significance of the italicized phrase is that, in order for the claim to hold, the DM must treat the draws from the black bag that determine, respectively, Deck 2, the Black-component of Deck 3, and the Red-component of Deck 3, as separate realizations of the same process. Given this, it does not matter whether the DM thinks "1" and "4" equally probable in that process.

the uncertainty averse Schmeidler model and the rank dependent expected utility model, may be seen to share the predictions of the MEU model taking into account the information available to the subjects. [30]

Turning to the other side of the divide, we note first that the models of Ahn (2008), Ergin and Gul (2009), Nau (2006), Nielsen (2010) and Seo (2009) have substantial subcategories that share the same functional form representation with the smooth ambiguity model, and these must share the predictions of the smooth model in this experiment. Second, a smooth ambiguity preference with the ambiguity attitude function $\phi(.)$ given by a negative exponential (constant absolute ambiguity aversion) is a Variational preference (Maccheroni *et al*. 2006). Finally, given the set of probability distributions $\{p_1, \ldots, p_4\}$ in Table 2, if a DM weights the distributions uniformly, a smooth ambiguity preference is also a Vector Expected Utility (VEU) preference since the sufficient conditions noted in Table II (pp. 826) of Siniscalchi (2009) are then met. Hence, our results also support these cases in so much as they support the smooth ambiguity model.

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[30] It is possible to have instances of CEU utility preferences that ignore part of the information available to the subjects and make predictions in our experiment akin to the smooth ambiguity model. Simon Grant gave us an example of such a convex capacity which violated Property Four and had choice implications like the smooth ambiguity model in the 2-ball condition case. Peter Wakker has also explained to us about these possibilities.

Appendix D: Experimental Details

D1. *Supplementary information on procedures*

Subjects were recruited using an online recruitment system developed by Tilburg University. On average, sessions contained approximately 10 subjects and lasted for about 45 minutes. 62 subjects participated under the tabular protocol and 35 under the verbal protocol. Differences between protocols are indicated in the main text; complete instructions for the tabular protocol are given in Section D2 below.

D2. *Instructions*

<The following was distributed on paper and read aloud.>

This experiment involves several decision tasks, each of which is a choice between two options involving amounts of money and/or chance. At the end of the experiment, the computer will select one choice to be for real for you. This means that you will be paid on the basis of what you chose in that choice, after any relevant chances have been resolved. In addition to the payment based on your responses to the choices, we will pay you €5 simply for participating.

Each choice is between two options. One of the options will be a certain amount of money whereas the other option will be a gamble. Here are some examples of gambles and an illustration of the kind of table with which your screen will display them:

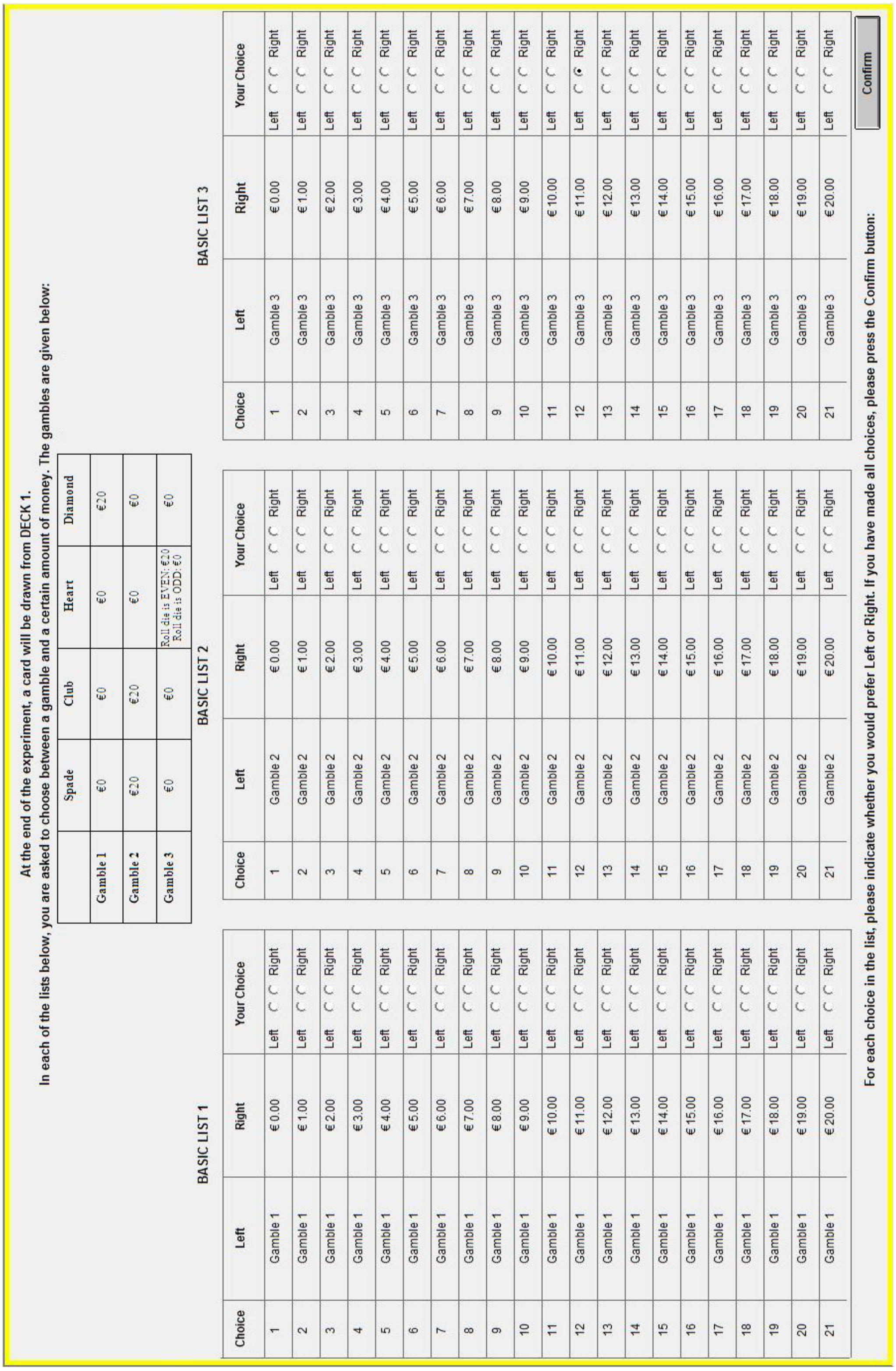
	Spade	**Club**	**Heart**	**Diamond**
Gamble 1	€0	€0	€0	€20
Gamble 2	€20	€20	€0	€0
Gamble 3	€0	€0	Roll die is EVEN: €20 Roll die is ODD: €0	€0

Looking at these examples, you can see that all gambles yield an outcome depending on the suit of a card drawn at the end of the experiment from a particular deck of cards. For example, Gamble 1 yields €20 if a diamond is drawn, and €0 otherwise. Similarly, Gamble 2 yields €20 if a spade or club is drawn, and €0 otherwise. As you can see, the outcome of Gamble 3 depends not only on the suit of a card drawn from a deck, but also on the roll of standard six-sided die performed at the end of the experiment. In this case, if a Spade, Club, or Diamond is drawn from the deck, Gamble 3 yields €0. If a heart is drawn from the deck, and the roll of the six sided die is even, Gamble 3 yields €20. If a heart is drawn, and the roll of the six sided die is odd, Gamble 3 yields €0.

The tasks will be grouped together in choice lists. Your computer will show you either 2 or 3 of these lists at once on a screen. An example screen is on the separate sheet provided. This example is not a screen that you will actually encounter during the experiment, but merely illustrates the general format. At the top of each screen, you can see a line of text that tells you that a card will be drawn at the end of the experiment from a particular deck, here Deck 1. During the experiment, you will encounter Decks 1, 2 and 3. I will tell you more about the decks later. For now, just note that the top of each screen will tell you the deck from which the card will be drawn to determine the outcome of the gambles described on that screen. Just below this, some gambles whose outcomes depend on the card drawn from the deck are presented in a table. In this example, there are three such gambles – the same ones in the table above. In the bottom half of the screen, you can see three lists, labelled Basic List 1, Basic List 2 and Basic List 3. Each of them is a numbered list of 21 choices with the same basic format, which I will now explain by talking you through Basic List 1 on the example screen.

Each numbered row of List 1 describes a choice between two options labelled Left and Right. In each case, Left corresponds to choosing Gamble 1 described at the top of the screen, whereas Right corresponds to choosing a certain amount of money. Note that as you move down List 1, Left always corresponds to the gamble, whereas the amount of money offered by Right rises as you move down the list, starting at €0 and rising in increments of €1 to €20. This basic structure will be the same for every list that you encounter.

Figure A.1: Example Choice List


At the end of the experiment, a card will be drawn from DECK 1.

In each of the lists below, you are asked to choose between a gamble and a certain amount of money. The gambles are given below:

	Spade	Club	Heart	Diamond
Gamble 1	€0	€0	€0	€20
Gamble 2	€20	€20	€0	€0
Gamble 3	€0	€0	Roll die is EVEN: €20 Roll die is ODD: €0	€0

BASIC LIST 1

Choice	Left	Right	Your Choice
1	Gamble 1	€ 0.00	Left ○ ○ Right
2	Gamble 1	€ 1.00	Left ○ ○ Right
3	Gamble 1	€ 2.00	Left ○ ○ Right
4	Gamble 1	€ 3.00	Left ○ ○ Right
5	Gamble 1	€ 4.00	Left ○ ○ Right
6	Gamble 1	€ 5.00	Left ○ ○ Right
7	Gamble 1	€ 6.00	Left ○ ○ Right
8	Gamble 1	€ 7.00	Left ○ ○ Right
9	Gamble 1	€ 8.00	Left ○ ○ Right
10	Gamble 1	€ 9.00	Left ○ ○ Right
11	Gamble 1	€ 10.00	Left ○ ○ Right
12	Gamble 1	€ 11.00	Left ○ ○ Right
13	Gamble 1	€ 12.00	Left ○ ○ Right
14	Gamble 1	€ 13.00	Left ○ ○ Right
15	Gamble 1	€ 14.00	Left ○ ○ Right
16	Gamble 1	€ 15.00	Left ○ ○ Right
17	Gamble 1	€ 16.00	Left ○ ○ Right
18	Gamble 1	€ 17.00	Left ○ ○ Right
19	Gamble 1	€ 18.00	Left ○ ○ Right
20	Gamble 1	€ 19.00	Left ○ ○ Right
21	Gamble 1	€ 20.00	Left ○ ○ Right

BASIC LIST 2

Choice	Left	Right	Your Choice
1	Gamble 2	€ 0.00	Left ○ ○ Right
2	Gamble 2	€ 1.00	Left ○ ○ Right
3	Gamble 2	€ 2.00	Left ○ ○ Right
4	Gamble 2	€ 3.00	Left ○ ○ Right
5	Gamble 2	€ 4.00	Left ○ ○ Right
6	Gamble 2	€ 5.00	Left ○ ○ Right
7	Gamble 2	€ 6.00	Left ○ ○ Right
8	Gamble 2	€ 7.00	Left ○ ○ Right
9	Gamble 2	€ 8.00	Left ○ ○ Right
10	Gamble 2	€ 9.00	Left ○ ○ Right
11	Gamble 2	€ 10.00	Left ○ ○ Right
12	Gamble 2	€ 11.00	Left ○ ○ Right
13	Gamble 2	€ 12.00	Left ○ ○ Right
14	Gamble 2	€ 13.00	Left ○ ○ Right
15	Gamble 2	€ 14.00	Left ○ ○ Right
16	Gamble 2	€ 15.00	Left ○ ○ Right
17	Gamble 2	€ 16.00	Left ○ ○ Right
18	Gamble 2	€ 17.00	Left ○ ○ Right
19	Gamble 2	€ 18.00	Left ○ ○ Right
20	Gamble 2	€ 19.00	Left ○ ○ Right
21	Gamble 2	€ 20.00	Left ○ ○ Right

BASIC LIST 3

Choice	Left	Right	Your Choice
1	Gamble 3	€ 0.00	Left ○ ○ Right
2	Gamble 3	€ 1.00	Left ○ ○ Right
3	Gamble 3	€ 2.00	Left ○ ○ Right
4	Gamble 3	€ 3.00	Left ○ ○ Right
5	Gamble 3	€ 4.00	Left ○ ○ Right
6	Gamble 3	€ 5.00	Left ○ ○ Right
7	Gamble 3	€ 6.00	Left ○ ○ Right
8	Gamble 3	€ 7.00	Left ○ ○ Right
9	Gamble 3	€ 8.00	Left ○ ○ Right
10	Gamble 3	€ 9.00	Left ○ ○ Right
11	Gamble 3	€ 10.00	Left ○ ○ Right
12	Gamble 3	€ 11.00	Left ○ ● Right
13	Gamble 3	€ 12.00	Left ○ ○ Right
14	Gamble 3	€ 13.00	Left ○ ○ Right
15	Gamble 3	€ 14.00	Left ○ ○ Right
16	Gamble 3	€ 15.00	Left ○ ○ Right
17	Gamble 3	€ 16.00	Left ○ ○ Right
18	Gamble 3	€ 17.00	Left ○ ○ Right
19	Gamble 3	€ 18.00	Left ○ ○ Right
20	Gamble 3	€ 19.00	Left ○ ○ Right
21	Gamble 3	€ 20.00	Left ○ ○ Right

For each choice in the list, please indicate whether you would prefer Left or Right. If you have made all choices, please press the Confirm button:

Confirm

Note that Lists 2 and 3 are exactly the same, except that Left corresponds to choosing Gamble 2 in List 2 and in List 3, Left corresponds to choosing Gamble 3. The way you complete the lists is to select either Left or Right for each choice task on each list by using your mouse to click in the corresponding circle on that row. *You cannot proceed to the next screen until you have selected either Left or Right in each row of each list*. Now look closer at choice 1 in List 1. We imagine that all of you will wish to choose Left in Choice 1. This is because Left offers a chance of €20, whereas Right in Choice 1 yields a certain €0. We also imagine that, as the certain amount of money offered by Right increases, at some point you will wish to switch to Right. For example, even if you have not done so in an earlier choice, we imagine that you will wish to choose Right in Choice 21 because, in that choice, Right yields a certain €20, whereas Left only offers a chance of €20. A similar logic applies to each list on the screen. Thus, for each list, we imagine that all of you will wish to select Left in the first choice and perhaps in some further choices until, at some point, you will switch to Right and then stick with Right until the final choice in the list. In fact, *the computer will only accept choices that do have this form of just switching once from Left to Right in each list.* The reason for this is that we assume throughout that, if you choose the gamble offered by Left in some choice over a particular sum of money, then you would also wish to choose that gamble over any smaller sum of money, and, similarly, if you would choose a particular sum of money over the gamble, you would also choose any larger sum of money over that gamble. However, note that it is entirely up to you at which point in a choice list to make your switch from selecting Left to selecting Right. Just recall that each choice in the list could prove to be the one selected at the end to determine your payment. In that case, the choice you have made in it, together with any card draws or die roll, will determine the payment you receive. After completing all the lists on a screen, you press the Confirm button. When you press the Confirm button, the computer will check that you have made a selection in each choice on the decision screen and that, in each list, you have switched exactly once from Left to Right, as you move down the list. If this is not the case, the computer will prompt you to make alterations. But, if it is the case, the computer will accept your confirmed selections. You cannot make any changes to them after that. Note that we will wait for everybody to complete each list before proceeding; you might have to wait a while before a new list appears on your screen.

In the experiment, there are two kinds of lists, called "basic" and "zoomed-in." At the top of each list, it will say which list it is. Basic lists have the form described above. When you have completed such a list, you will have switched from Left to Right at some point. The computer will then show you a "zoomed-in" list. This is generated from the basic list by zooming-in on that part of the basic list where you switched from Left to Right. Thus, the gambles listed at the top of the screen and the deck from which a card will be drawn are the same as on the previous screen. However, in the zoomed-in list, the top choice will be the last choice in which you selected Left in the corresponding basic list, and the bottom choice will be the first choice in which you selected Right. These choices will be filled in for you, because you have already confirmed them. The computer then requires you to make further choices in which the sums of money offered by Right are intermediate between those on the top and bottom rows and rise as you move down the list, but now in increments of 5 cents. Again, you must make a selection for each choice of each list, and are only allowed to switch just once from Left to Right in the zoomed-in lists as well.

Each screen will have the format described above. The gambles that depend on a draw of a card will be described at the top of the screen, together with the name of the deck from which a card will be drawn. Remember that these things can differ from one screen to another so you should always study them carefully before completing each choice list. So far, I have not said much about the decks. This is because, at each point in the experiment, I will project on the whiteboard here some important information about the deck that the decisions you are currently taking relate to. At the end of the experiment, I will construct the decks in the ways specified by the information I will project on the board. Then, each deck will be shuffled, and a 10-sided die with numbers 1-10 will be rolled by one of you. Then, I will draw the card whose position from the top is given by the number on the die. For example, if the die roll is a 3, I will draw the 3rd card from the deck. This card will determine the outcome of any gamble that depends on a draw from that deck.

After I have done this for each deck, I will roll the 6-sided die to determine the outcome of any gamble that depends on this roll. Finally, your computer will select one choice at random from all of those in all the basic lists that you have completed. If this choice is not either the last choice in the list in which you chose Left or the first in which you chose Right, then what you chose in that choice task will determine your payment. On the other hand, if the randomly selected choice is either the last choice in the list in which you chose Left or the first in which you chose Right, the computer will move to the corresponding list on the relevant zoomed-in screen and randomly select one choice from that zoomed-in list. What you chose in that task will then determine your payment. Note that this simply means that every choice that you face could prove to be the one selected to be paid for real. If you chose Right in that choice, you will receive the amount of money specified by Right. If you chose Left, you will receive either €20 or €0, depending on the card drawn from the relevant deck and/or on the roll of the six-sided die.

<the following was read aloud only>

Each deck that we use in the experiment will have ten cards. First, I will ask you to complete some tasks relating to Deck 1. This slide gives you some information about Deck 1. It says that Deck 1 has seven spades and three hearts. At the end of the experiment, I will construct Deck 1 to contain ten cards in total, made up of seven spades and three hearts. Then I will draw a card from Deck 1, as explained in the instructions. Your computer will now display several lists involving Deck 1. Please complete them at your own pace.

<subjects complete lists for Deck 1>

Next, I will ask you to complete some tasks relating to Deck 2. This slide gives you some information about Deck 2. It says that Deck 2 has 5 black cards (i.e. either spade or club) and 5 red cards (i.e. either heart or diamond). However, it does not tell you exactly how many cards of each suit there will be in Deck 2. The way this will be resolved is as follows. I have here an opaque bag containing balls, all of which are numbered either 1 or 4. Here is an example of each. I am not going to tell you how many balls of each kind there are. At the end of the experiment, I will shake the bag and draw one ball from it. The number on this ball will give *both* the number of spades *and* the number of hearts in Deck 2. The numbers of clubs and diamonds will adjust accordingly, to make up 5 black and 5 red cards in total. Thus, as this slide shows, there are two possibilities for the composition of Deck 2, depending on the ball that is drawn. If that ball is numbered 1, the deck will contain 1 spade, 4 clubs, 1 heart and 4 diamonds. But, if the ball is numbered 4, the deck will contain 4 spades, 1 club, 4 hearts and 1 diamond. In both cases, there are 5 black cards and 5 red cards in total; and the number of spades equals the number of hearts. Note that how likely each of the two possible compositions of Deck 2 is depends on the contents of the bag of balls, which I have not revealed. The two compositions may or may not be equally likely. Your computer will now display several lists involving cards drawn from Deck 2. Please complete them at your own pace.

<subjects complete lists for Deck 2>

Next, I will ask you to complete some tasks relating to Deck 3. This slide gives you some information about Deck 3. Thus, as with Deck 2, Deck 3 has 5 black cards and 5 red cards, but the slide does not tell you how many cards of each suit there will be in Deck 3. For Deck 3, this will also be resolved in a similar way to for Deck 2, except that this time two balls will be drawn from the bag rather than one. I will shake the bag and draw one ball from it; then I will show you the number of this ball and place it back, before shaking the bag again and drawing another ball. The number on the *first* ball will give the number of spades in Deck 3 and the number of the *second* ball will give the number of hearts. As before, the numbers of clubs and diamonds will adjust accordingly to make up 5 black and 5 red cards in total. Thus, as the slide shows, there are four possibilities for the composition of Deck 3. If both balls drawn are numbered 1, Deck 3 will contain 1 spade, 4 clubs, 1 heart, and 4 diamonds. If the first ball drawn is numbered 1 and the second ball numbered 4, Deck 3 will contain 1 spade, 4 clubs, 4 hearts, and 1 diamond. If the first ball drawn is numbered 4 and the second ball numbered 1, the Deck

3 will contain 4 spades, 1 club, 1 heart, and 4 diamonds. If both balls drawn are numbered 4, the Deck 3 will contain 4 spades, 1 club, 4 hearts, and 1 diamond. In all cases, there are 5 black cards and 5 red cards in total; the composition of the black cards is determined separately from the composition of the red cards, because the former is determined by the first ball and the latter by the second ball. Thus, unlike Deck 2, the number of spades and hearts may or may not be equal in Deck 3. Note that how likely each of the four possible compositions of Deck 3 is depends on the contents of the bag of balls, which I have not revealed. The four compositions may or may not all be equally likely. Your computer will now display several lists involving cards drawn from Deck 3. Please complete them at your own pace.

D3 *Summary statistics of certainty equivalents for all acts, by type*

Table A2: Certainty Equivalents (in €, rounded to nearest cent)

	Ambiguity Seeking (n=16)			Ambiguity Neutral (n=50)			Ambiguity Averse (n=31)			All Subjects (n=97)		
	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD	Mean	Median	SD
Ambiguous acts												
$CE(f_1$,2-ball)	5.47	4.45	2.91	4.51	4.33	2.19	5.84	4.93	3.02	5.09	4.53	2.64
$CE(f_2$,2-ball)	5.28	4.50	3.01	4.65	4.15	2.74	6.03	4.98	3.24	5.19	4.48	2.98
$CE(mix$,2-ball)	5.67	5.38	3.56	4.87	4.48	2.81	6.99	5.68	3.99	5.68	4.98	3.45
$CE(g_1$,2-ball)	5.43	5.25	2.59	4.95	4.53	3.01	7.43	5.23	4.19	5.82	4.98	3.53
$CE(g_2$,2-ball)	6.35	6.25	3.31	4.95	4.53	3.01	5.53	4.53	2.82	5.37	4.98	3.02
Ambiguous acts												
$CE(f_1$,1-ball)	5.43	4.95	2.29	5.28	4.93	2.72	6.06	5.03	2.48	5.56	4.98	2.58
$CE(f_2$,1-ball)	5.75	5.45	2.20	5.45	4.98	2.68	6.04	5.53	2.29	5.69	5.03	2.47
$CE(mix$,1-ball)	6.27	5.73	3.30	5.45	4.33	3.60	6.17	5.03	3.10	5.81	4.88	3.38
Risky acts (denoted by chance of €20)												
CE(0.7)	11.65	11.33	3.30	10.58	11.18	3.80	11.53	12.78	3.00	11.06	11.73	3.48
CE(0.3)	5.59	5.25	2.00	6.19	5.73	3.05	6.39	5.73	2.82	6.15	5.58	2.81