MEASURING AMBIGUITY ATTITUDES FOR ALL (NATURAL) EVENTS

AURÉLIEN BAILLON[a], ZHENXING HUANG[b], ASLI SELIM[a], & PETER P. WAKKER[a]

a: Erasmus School of Economics, Erasmus University Rotterdam, Rotterdam, the Netherlands, Baillon@ese.eur.nl, Wakker@ese.eur.nl; asliselim@gmail.com

b: Shanghai University of Finance and Economics, School of Economics, Shanghai, China, ybhzxadam@gmail.com

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Measurements of ambiguity attitudes have so far focused on artificial events, where subjective beliefs can be derived from symmetry assumptions. For natural events such assumptions usually are not available, creating a difficulty in calibrating subjective beliefs and, hence, in measuring ambiguity attitudes. This paper introduces a simple control for subjective beliefs even when they are unknown. We thus allow for a tractable and completely revealed-preference based measurement of ambiguity attitudes for all events, including natural ones. We introduce indexes of ambiguity aversion and ambiguity perception (or understanding) that generalize and unify many existing indexes. Our indexes are valid under many ambiguity theories. They do not require expected utility for risk, which is desirable for empirical purposes. Furthermore, they are easy to elicit in practice. An experiment on ambiguity under time pressure shows the tractability of our method. It gives plausible results, supporting the validity of our indexes.

JEL-CLASSIFICATION: D81, C91

KEYWORDS: subjective beliefs; ambiguity aversion; Ellsberg paradox; sources of uncertainty; time pressure

1. INTRODUCTION

Ambiguity (unknown probabilities) is central in many practical decisions (Keynes 1921; Knight 1921). Ellsberg's paradox (1961) shows that fundamentally new models are needed to handle ambiguity. Since then many models have been proposed, not only to accommodate Ellsberg's paradox but also to explain anomalies in practice (Easley & O'Hara 2009; Guidolin & Rinaldi 2013). However, measurements of ambiguity have been lagging behind, using artificial laboratory events as in Ellsberg's paradox rather than the natural events that occur in practice.

To properly measure ambiguity aversion we need to control for subjective likelihood beliefs in the events of interest, which we need for calibrating the benchmark of ambiguity neutrality. But this control is difficult to implement for natural events. For example, consider a person who would rather receive $100 under the ambiguous event A of the copper price going up by at least 0.01% tomorrow, than under the event K (with known probability 0.5) of heads coming up in a coin toss tomorrow. This preference need not reflect ambiguity seeking; instead, it may have been induced by beliefs. The person may be ambiguity neutral but assign a higher subjective likelihood to A than K's probability of 0.5. Therefore, without proper control of subjective likelihoods, no conclusive implications can be drawn about people's ambiguity attitudes. However, how to control for subjective likelihood beliefs in a tractable manner has been unknown so far for naturally occurring events.

Controlling for subjective likelihoods is much easier for artificial events generated in the lab. Such events concern Ellsberg urns with color compositions kept secret from the subjects, or subjects only being informed about experimenter-specified intervals of possible probabilities of events. For these events, likelihoods can be derived from symmetry of colors or from symmetry about the midpoints of probability intervals. This explains why measurements of ambiguity have as yet focused on artificial cases.

Several authors warned against the focus on artificial ambiguities, arguing for the importance of natural events (Camerer & Weber 1992 p. 361; Ellsberg 2011 p. 223; Heath & Tversky 1991 p. 6). The difficulty to identify subjective likelihoods of such events from revealed preferences has as yet been taken as a problematic obstacle though. This paper introduces a simple method to measure ambiguity attitudes for natural events. The solution to the aforementioned problem is surprisingly easy: we control for likelihoods not by directly measuring them but by making them drop from the equations irrespective of what they are. The resulting method is tractable and easy to implement, as we demonstrate in an experiment. Hence, it can for instance be easily used as an add-on in large-scale surveys and field studies. Using natural events will increase external validity (Camerer & Weber 1992 p. 361 penultimate para).

We introduce two indexes of ambiguity attitudes that unify and generalize several indexes proposed before (§3). Empirical studies, discussed later, have shown that ambiguity is a rich phenomenon and that two indexes are needed to capture it. The first index measures the well-known aversion to ambiguity. The second index measures the degree of ambiguity, i.e., the perceived level of ambiguity. Hence Dimmock et al. (2015) called their special case of this index perceived level of ambiguity. The higher this level is, the less the decision maker discriminates between different levels of likelihood, and the more these levels are treated alike, as a blur. Hence the second index also reflects insensitivity toward likelihood changes, which is why the term a(mbiguity generated) insensitivity can be used (Maafi 2011; Baillon, Cabantous, & Wakker 2012). Our indexes generalize their predecessors by: (a) being directly observable; (b) not requiring expected utility for risk; (c) being valid for a large number of ambiguity theories; (d) requiring no assessment of subjective likelihoods and, hence, (e) being applicable to natural ambiguities that were not constructed artificially.

Dow and Werlang (1992), MacCrimmon & Larsson (1979), and others that we discuss later used a special case of our method, considering both an event and its complement so as to neutralize for unknown beliefs. We make this method operational, and theoretically and empirically valid, for ambiguity aversion. We further extend it to insensitivity. Because insensitivity has been less known than aversion to ambiguity, and different interpretations are possible for our insensitivity index, we test how our two indexes react to cognitive manipulations. Imposing time pressure (TP) is a well-known method for manipulating cognitive limitations. Hence our experiment investigates the effect of TP on ambiguity attitudes, where the ambiguity concerns a natural event (about the performance of the AEX—Amsterdam stock exchange—index). Despite the importance of TP in its own right, and the many studies of it under risk (known probabilities; see §4) there have not yet been studies of

TP under ambiguity. This provides an additional contribution of our paper. Our findings corroborate the interpretation of the indexes, supporting the validity of our method. In particular, they illustrate the usefulness of our second index.

The outline of this paper is as follows. Section 2 gives formal definitions of our ambiguity indexes and informal arguments for their plausibility. We present the indexes prior to assuming any decision theory so that empirically oriented readers can readily use them without need to study such a theory. Further, this way we show that the indexes have intuitive appeal without necessity to commit to one or other of the many ambiguity theories popular today. Section 3 gives formal arguments by proving the validity of our indexes under many ambiguity theories. Sections 4-5 demonstrates the validity of our indexes empirically, and Section 6 concludes. Proofs and experimental details are in the appendix, with further details in a web appendix.

2. MEASURING AMBIGUITY ATTITUDES WITHOUT MEASURING SUBJECTIVE LIKELIHOODS: DEFINITIONS OF OUR INDEXES

We focus on gain outcomes throughout this paper. Formally speaking, ambiguity does not concern just a single event E, but a partition, such as $\{E, E^c\}$, or, more generally, a source of uncertainty. We assume a minimal degree of richness of the sources of uncertainty considered: there should at least be three mutually exclusive and exhaustive nonnull events E_1, E_2, and E_3. In most situations where we start from a partition with two events we can extend it by properly partitioning one of those two events. For example, in the two-color Ellsberg urn we can involve other features of the ball to be drawn, such as shades of colors or numbers on the balls. In our experiment the events refer to the AEX stock index. For instance, in Part 1 of the experiment, $E_1 = (-\infty, -0.2), E_2 = [-0.2, 0.2]$, and $E_3 = (0.2, \infty)$, where intervals describe percentage increases of the AEX index. Thus they concern natural events with uncertainty that really occurred and that was of practical relevance to financial traders. E_{ij} denotes the union $E_i \cup E_j$ where $i \neq j$ is implicit. We call every E_i a *single event* and every E_{ij} a *composite event*.

Dimmock, Kouwenberg, & Wakker (2016, Theorem 3.1) showed that matching probabilities are convenient for measuring ambiguity attitudes. Karni (2009) discussed their elicitation. Matching probabilities entirely capture ambiguity attitudes, free of the complications of risk attitudes, as those drop from the equations and need not be measured. In particular, our measurements are not affected by the often-discussed heterogeneity of risk attitudes (Bruhin, Fehr-Duda, & Epper 2010). We will therefore use matching probabilities. For any fixed prize, €20 in our experiment, we define the *matching probability* m of event E through the following indifference:

Receiving €20 under event E is equivalent to receiving €20 with probability m. (2.1)

In each case it is understood that the complementary payoff is nil. Under ambiguity neutrality the matching probability of an event, say $m(E_1)$, and its complement, $m(E_{23})$, will add to 1, but under ambiguity aversion the sum will fall below 1. Its difference with 1 can then be taken as degree of aversion. We will take the average of this difference over the three events. We write $m_i = m(E_i)$, $m_{ij} = m\left(E_{ij}\right)$, $\overline{m_s} = (m_1 + m_2 + m_3)/3$ for the average single-event matching probability, $\overline{m_c} = (m_{23} + m_{13} + m_{12})/3$ for the average composite-event matching probability, and define:

DEFINITION 2.1. The *ambiguity aversion index* is

$$b = 1 - \overline{m_c} - \overline{m_s}. \tag{2.2}$$

Under ambiguity neutrality, $m_i = P(E_i)$ and $m_{ij} = P(E_i) + P(E_j)$ for additive subjective probabilities P. Then $\overline{m_s} = 1/3$ and $\overline{m_c} = 2/3$, implying $b = 0$. We have thus calibrated ambiguity neutrality, providing control for subjective likelihoods without knowing them. This happens because the subjective likelihoods drop from the equations irrespective of what they are. This observation is key to our method. Maximal ambiguity aversion occurs for $b = 1$, when matching probabilities for all events are 0. Ambiguity aversion is minimal for $b = -1$, when matching probabilities for all events are 1.

For the ambiguity aversion index, it is not necessary to consider a three-event partition. To reduce the measurement effort, we could also focus on only one event E_i

and its complement E_i^c, and substitute $m(E_i)$ for $\overline{m_s}$ and $m(E_i^c)$ for $\overline{m_c}$ in Eq. 2.2, maintaining the control for likelihood. This reduction is at the cost of reliability, but it makes it possible to elicit the first index even when the source has only two nonnull events. For the insensitivity index we essentially need three events.

Using only the first index to capture people's ambiguity attitude can be misleading, especially for low likelihood events. Empirical findings suggest a dependency of ambiguity aversion on likelihood: aversion peaks at certainty but drops as the likelihood of the uncertain event decreases. For moderate likelihoods, there is much ambiguity neutrality, and for low likelihoods ambiguity seeking is prevailing (reviewed by Trautmann & van de Kuilen 2015). Therefore, a prediction of universal ambiguity aversion based solely on the first index alone can even be in the wrong direction for low likelihoods. Our second index of ambiguity allows accommodating the aforementioned dependence of ambiguity aversion on likelihood. It can be interpreted as perceived level of ambiguity (Baillon et al. 2015; Dimmock et al. 2015) or as insensitivity to likelihood (Abdellaoui et al. 2011; Dimmock, Kouwenberg, & Wakker 2016).

The second index captures the extent to which matching probabilities (and event weights as defined in §3) regress towards fifty-fifty, with low likelihoods overvalued and high likelihoods undervalued. This leads to reduced differences $\overline{m_c} - \overline{m_s}$. In the most extreme case of complete ambiguity and, correspondingly, complete insensitivity (Cohen & Jaffray 1980), no distinction at all is made between different levels of likelihood (e.g. all events are taken as fifty-fifty), resulting in $\overline{m_c} - \overline{m_s} = 0$. These observations suggest that the second index can be interpreted as a cognitive component (Budescu et al. 2014 p. 3; Dimmock et al. 2015; Dimmock, Kouwenberg, & Wakker 2016; Einhorn & Hogarth 1985; Gayer 2010), an interpretation well supported by our results.

Dimmock et al. (2015) referred to their version of the second index as perceived level of ambiguity. Dimmock et al.'s term, and the multiple priors model underlying it, assume expected utility for risk and may serve best for normative applications. We allow for deviations from expected utility under risk, which is desirable for descriptive applications, the main purpose of this paper. For risk, insensitivity (i.e., inverse-S probability weighting) has been commonly found (Fehr-Duda & Epper 2012; Gonzalez & Wu 1999). Our second index naturally extends this insensitivity found under risk to ambiguity, where empirical studies have found that it is usually reinforced (Trautmann & van de Kuilen 2015). Hence, we follow Maafi (2011) and Baillon, Cabantous, & Wakker (2012) and use the term ambiguity-generated insensitivity (a-insensitivity) to refer to it. For this index, the following rescaling of $\overline{m_c}$–$\overline{m_s}$ is convenient.

DEFINITION 2.2. The *ambiguity-generated insensitivity (a-insensitivity) index*[1] is

$$a = 3 \times \left(1/3 - (\overline{m_c} - \overline{m_s})\right)\ . \tag{2.3}$$

Under ambiguity neutrality, with perfect discrimination between single and composite events, or under absence of ambiguity, $\overline{m_c} = 2/3$ and $\overline{m_s} = 1/3$, and their difference is 1/3. Index a measures how much this difference falls short of 1/3. We multiplied by 3 to obtain a convenient normalization with a maximal value 1 (maximal insensitivity, with $\overline{m_c} = \overline{m_s}$).

Ambiguity neutrality gives $a = 0$. We have again calibrated ambiguity neutrality here, controlling for subjective likelihoods by letting them drop from the equations. Empirically, we usually find prevailing insensitivity, $a > 0$, but there are subjects with $a < 0$. Hence it is desirable for descriptive purposes to allow $a < 0$, which we do. The α-maxmin model, however, does not allow $a < 0$ (§3), which is no problem for normative applications that take $a < 0$ to be irrational.

There have as yet only been a few studies measuring ambiguity attitudes for natural events. Many did not control for risk attitudes and therefore could not completely identify ambiguity attitudes (Baillon et al. 2015; Fox, Rogers, & Tversky 1996; Fox and Tversky 1998; Kilka & Weber 2001). Abdellaoui et al. (2011) measured indexes similar to ours but had to use complex measurements and data fittings, requiring measurements of subjective probabilities, utilities, and event weights. As regards the treatment of unknown beliefs, Brenner & Izhakian (2015) and Gallant, Jahan-Parvar, & Liu (2015) are close to us. They do not assume beliefs given beforehand, but, like Abdellaoui et al. (2011), derive them from preferences. We do not need such a derivation. Brenner & Izhakian (2015) and Gallant, Jahan-Parvar, & Liu (2015), deviate from our approach in assuming second-order

[1] Under multiple prior theories, this index can be called "perceived level of ambiguity."

probabilities to capture ambiguity. They make parametric assumptions about the first- and second-order probabilities (assuming normal distributions), including expected utility for risk with constant relative risk aversion, and then fit the remaining parameters to the data for a representative agent. Maccheroni, Marinacci, & Ruffino's (2013) theoretical analysis follows a similar approach.

Baillon & Bleichrodt (2015) used a method similarly tractable as ours. They, however, used different indexes[2], and they did not establish a control for likelihood. Several papers used indexes similar to those presented above but provided no controls for likelihoods, so that they had to use probability intervals or Ellsberg urns (Baillon, Cabantous, & Wakker 2012; Dimmock, Kouwenberg, & Wakker (2016), Dimmock et al. 2015, 2016). Li (2015), a follow-up of this paper, used our method.

3. RELATING OUR INDEXES TO EXISTING INDEXES

We defined our indexes in §2 without specifying any ambiguity theory. This section shows that our indexes are valid under many popular ambiguity theories because they generalize indexes proposed there. Empirically oriented readers who are willing to take our indexes at face value can skip this section. The section is essential though for the claims that our indexes are not ad hoc but theoretically founded, and that they generalize and unify many existing indexes.

Our analysis applies to any theory using the evaluation

$$x_E 0 \to W(E)U(x) \tag{3.1}$$

for prospects with one nonzero outcome. The prospect $x_E 0$ yields outcome x under event E and outcome 0 under the complementary event E^c. U is the *utility function* with $U(0) = 0$ and W is a nonadditive (*event) weighting function*; i.e., W is 0 at the empty event, 1 at the universal event, and it is *set-monotonic* ($A \supset B$ then $W(A) \geq W(B)$)). Our analysis includes binary RDU[3], also known as biseparable utility, which includes many theories such as Choquet expected utility or rank-dependent utility,

[2] They used five event-dependent indexes similar to Kilka & Weber (2001), and based on preference conditions of Tversky & Wakkers (1995), and adapted them to matching probabilities.

[3] RDU abbreviates rank-dependent utility.

prospect theory (because we only consider gains), multiple priors, and α-maxmin (Ghirardato & Marinacci 2002; Wakker 2010 §10.6). Eq. 3.1 additionally includes separate-outcome weighting theories $\left(x_E y \rightarrow W(E)U(x) + W(E^c)U(y)\right)$, Chateauneuf & Faro's (2009) confidence representation if the worst outcome is 0, and Lehrer & Teper's (2015) event-separable representation. Based on the heuristic considerations in §2 we conjecture that our indexes also capture features of ambiguity well under ambiguity theories not included here, but leave this as a topic for future research.

Our first index generalizes indexes by Abdellaoui et al. (2011), Chateauneuf et al. (2007), Dimmock et al. (2015, 2016), Dimmock, Kouwenberg, & Wakker (2016), Dow & Werlang (1992), Gajdos et al. (2008), Klibanoff, Marinacci, & Mukerji (2005 Definition 7), and Schmeidler (1989). Our second index generalizes indexes by Abdellaoui et al. (2011), Chateauneuf et al. (2007), Dimmock et al. (2015), Dimmock, Kouwenberg, & Wakker (2016), and Gajdos et al. (2008). The following subsections will provide an elaborate examination for various theories.

3.1. *Choquet Expected Utility*

We start with the first axiomatized ambiguity model: Schmeidler's (1989) Choquet expected utility. Schmeidler (1989) suggested the following index of ambiguity aversion in his example on pp. 571-572 and p. 574, assuming expected utility for risk:

$$b^* = 1 - W(E) - W(E^c). \tag{3.2}$$

Here W is a general event weighting function. Dow & Werlang (1992) proposed to use Eq. 3.2 in general, and this proposal has been widely followed since, always in models assuming expected utility for risk.[4] Eq. 3.2 already contains the basic idea of correcting for unknown beliefs by considering both an event and its complement. This was also used in the more general concept of source preference (Tversky & Wakker 1995).

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[4] References include Chateauneuf et al. (2007), Dimmock et al. (2015, 2016), Gajdos et al. (2008), and Klibanoff, Marinacci, & Mukerji (2005 Definition 7). Applications include Dominiak & Schnedler (2011), Ivanov (2011), and many others.

OBSERVATION 3.1. Under expected utility for risk, our ambiguity aversion index agrees with Eq. 3.2. That is, index b is Eq. 3.2 averaged over the events E_1, E_2, E_3. In Schmeidler's (1989) model, ambiguity aversion[5] implies $b > 0$, ambiguity neutrality implies $b = 0$, and ambiguity seeking implies $b < 0$. □

Two contributions of Observation 3.1 to Choquet expected utility are: (1) index b is also valid if expected utility for risk is violated; (2) ambiguity aversion b can be measured easily, with no need to further measure U or W. A difficulty when applying Eq. 3.2 is, for instance, that in general there is no direct way to measure W. Because of contribution (1), our method also works for the general Choquet expected utility model in Gilboa (1987) which, unlike Schmeidler (1989), does not assume expected utility for risk.

3.2. *The Source Method*

Choquet expected utility and prospect theory (Tversky & Kahneman 1992), which are equivalent because we consider only gains, are considered to be too general because there are too many nonadditive weighting functions for large state spaces.[6] Abdellaoui et al.'s (2011) source method is a specification that is more tractable. The specification essentially consists of adding Chew & Sagi's (2008) conditions, implying the existence of a-neutral probabilities defined later.

Although, based on Ellsberg's paradoxes, it was long believed that ambiguity aversion cannot be modeled using probabilities of any kind, Chew & Sagi (2008) showed that this is still possible, by allowing decision attitudes to depend on the source of uncertainty. For example, we can assign probability 0.5 to an ambiguous event and still prefer gambling on it less than gambling on an objective probability 0.5, by weighting the former probability more pessimistically than the latter. This way, ambiguity aversion and Ellsberg's paradox can be reconciled with the existence

[5] Schmeidler used the term uncertainty aversion.

[6] The findings of Hey, Lotito, & Maffioletti (2010) suggest to us that three states is already problematic for empirical purposes. Kothiyal, Spinu, & Wakker (2014) showed that the specification of the source method then is specific enough.

of subjective probabilities. Because the term subjective probability has too many connotations, we call the probabilities resulting from Chew & Sagi's model *a(mbiguity)-neutral probabilities*. An ambiguity neutral decision maker would indeed be entirely guided by these probabilities, irrespective of the underlying events.

The only implication of Chew & Sagi's conditions needed for our analysis is that Eq. 3.1 can be rewritten as:

$$W(E) = w_{So}(P(E)) \,. \tag{3.3}$$

Here P is Chew & Sagi's a-neutral probability and w_{So} is a *(probability) weighting function* ($w_{So}(0) = 0,\ w_{So}(1) = 1$, and w_{So} is nondecreasing). Crucial is that w_{So} can depend on the source So of uncertainty: $w_{So}(0.5)$ can be different for the known and the unknown Ellsberg urn. Tversky introduced the idea of *sources* of uncertainty (Heath & Tversky 1991; Tversky & Fox 1995). A source of uncertainty is a group of events generated by the same uncertainty mechanism. The unknown Ellsberg urn is a different source than the known urn, and the AEX index is a different source than the Dow Jones index. Different sources will have different weighting functions w_{So} and, correspondingly, W will have different properties for them. We study these properties for binary RDU models. For other models, such as the smooth model of ambiguity (Klibanoff, Marinacci, & Mukerji 2005), it will similarly be of interest to allow for different attitudes and perceptions for different sources of ambiguity, but this is beyond the scope of this paper.

In their calculations, the two papers Abdellaoui et al. (2011) and Dimmock, Kouwenberg, & Wakker (2016), abbreviated AD in this section, used best approximations of functions on the open interval $(0,1)$. This is done here for matching probabilities $m(E)$:

$$m(E) = \tau + \sigma P(E) \text{ for } 0 < P(E) < 1, \tag{3.4}$$

say by minimizing quadratic distance (as in regular regressions) where $\sigma \geq 0$ and τ are chosen to minimize that distance. P is again Chew & Sagi's (2008) a-neutral probability. Although our indexes were devised to avoid specifications of a-neutral probabilities $P(E)$, we do consider such probabilities here because otherwise the approaches of AD cannot be applied. AD defined

$$b' := 1 - 2\tau - \sigma,\ \ a' := 1 - \sigma \ \ (\textit{AD indexes}). \tag{3.5}$$

Here b' is an index of pessimism that reflects ambiguity aversion in our case (which concerns matching probabilities), and a' is an index of insensitivity, reflecting lack of discriminatory power. We write $p_i = P(E_i)$ and $p_{ij} = P(E_{ij})$.

As a preparation, we first show that our indexes are identical to the AD indexes if Eq. 3.4 holds exactly. Eq. 3.4 implies $\overline{m_s} = \tau + \sigma/3$ and $\overline{m_c} = \tau + 2\sigma/3$, where we immediately see that a-neutral probabilities drop. Observation 3.2 follows from simple substitutions.

OBSERVATION 3.2. Under Eq. 3.4, our indexes (Eqs. 2.2, 2.3) agree with the AD indexes (our Eq 3.5). That is, $a = 1 - (3\overline{m_c} - 3\overline{m_s}) = 1 - \sigma = a'$ and $b = 1 - (\overline{m_c} + \overline{m_s}) = 1 - 2\tau - \sigma = b'$. □

We now turn to the general case where Eq. 3.4 need not hold. Proofs of the following results are in the appendix. We first show that the aversion indexes b, b' also agree in the general case.

THEOREM 3.3. Our index b (Eq. 2.2) is always identical to the AD index b' (Eq. 3.5), independently of p_1, p_2, p_3, σ. □

Depending on the probabilities p_1, p_2, p_3 assumed by AD, the insensitivity indexes a, a' need not always be completely identical. These indexes estimate the same model (Eq. 3.4) but use different optimization criteria.[7] Thus the indexes can be slightly different, but they will not differ by much. We next show that they are identical in the most important cases. We first consider the case considered by Dimmock et al. (2015, 2016), Dimmock, Kouwenberg, & Wakker (2016), and most other studies (Camerer & Weber 1992 p. 361), where the ambiguity neutral p_i's directly follow from symmetry.

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[7] AD take the best fit of Eq. 3.4 for the three partitions $\{E_i, E_i^c\}$ in one blow. Our indexes can be interpreted as first giving best (even perfect) fit for each separate partition $\{E_i, E_i^c\}$, and next taking averages of the three estimations (for $i = 1,2,3$).

OBSERVATION 3.4. Index a is identical to AD's a' if events E_1, E_2, and E_3 are symmetric (i.e., $p_1 = p_2 = p_3$). □

We next turn to general nonsymmetric cases. We first consider the most plausible case, concerning the probabilities p_1, p_2, p_3 that best fit the data. For matching probabilities, set-monotonicity means that $m_{ij} \geq m_i$ for all i, j. A weaker condition, *weak monotonicity*, suffices for our purposes: for all distinct i, j, k: $m_{ij} + m_{jk} \geq m_i + m_k$.

THEOREM 3.5. Assume weak monotonicity, and assume that a, b, p_1, p_2, p_3 are such that Eq. 3.4 best fits by quadratic distance. Then our index a (Eq. 2.3) is identical to AD's index a' (Eq. 3.5). □

Thus our indexes and the AD indexes are close, and in most cases identical. This was confirmed in our data. Of course, the estimates of b and b' always completely agreed. The average absolute difference $|a' - a|$ was 0.007. In 91% of the cases a' and a were identical. The remaining 9% concerned vast violations of weak monotonicity, with maximal absolute difference $|a' - a| = 0.27$ for a highly erratic subject. We conclude that for all practical purposes we can assume that our indexes are the same as those of AD. Our contribution to the source method is that we need not restrict to Ellsberg events as did Dimmock, Kouwenberg, & Wakker (2016), Dimmock et al. (2015, 2016), and we avoid the extensive measurement of beliefs and utility for natural events of Abdellaoui et al. (2011).

3.3. *Multiple Priors*

We next consider multiple prior models. In maxmin expected utility (Gilboa and Schmeidler 1989) or α-maxmin (Ghiradato, Maccheroni, & Marinacci 2004), ambiguity is captured by a convex set C of priors (probability distributions over the state space). The decision maker then considers the worst expected utility over C (maxmin expected utility) or a convex combination of the worst and the best (α-maxmin). As with Choquet expected utility, the multiple priors model by itself is too general to be tractable because there are too many sets of priors. We start from a tractable subcase used in finance (Epstein and Schneider 2010) and insurance theory

(Carlier et al. 2003): the ε-contamination model. We take the tractable subclass considered by Baillon et al. (2015), Chateauneuf et al. (2007), and Dimmock et al. (2015), which received a preference foundation by Chateauneuf et al. (2007). It is a subclass of the ε-contraction model; the latter was axiomatized by Gajdos et al. (2008). Kopylov (2009) axiomatized a similar model.

To define our subclass, we assume a baseline probability Q, and an $\epsilon \in [0,1]$. The set of priors consists of all convex combinations $(1-\varepsilon)Q + \varepsilon T$ where T can be any probability measure. The larger ε, the larger the set of priors. We call the resulting model *ε-α-maxmin*. This model satisfies Chew & Sagi's (2008) assumptions, with a-neutral probabilities Q. The size of the set of priors, represented here by ε, is often taken as the level of perceived ambiguity (Alon & Gayer 2016; Chateauneuf et al. 2007 p. 543; Gajdos et al. 2008; Walley 1991 p. 222), and α as the aversion index. Baillon et al. (2015) and Dimmock et al. (2015) pointed out that the source method and ε-α-maxmin are closely related, with the relations in the following observation between indexes. These authors took the a and b indexes as in AD. Subsection 3.2 showed that those are essentially equivalent to our indexes, so we use the same notation.

OBSERVATION 3.6. Under ε-α-maxmin, the ambiguity-level index ε agrees with our a-insensitivity index a ($\varepsilon = a$), and the aversion parameter α is a rescaling of our aversion index b ($b = (2\alpha - 1)\varepsilon$). □

For the aversion indexes α and $b = (2\alpha - 1)\varepsilon$, the linear rescaling $b \to 2\alpha - 1$ is immaterial, but the subsequent multiplication by ε is of interest. Our index b reflects the total ambiguity aversion exhibited for the event by the decision maker, and is best suited to calculate ambiguity premiums[8]. The index α rather is the ambiguity aversion per perceived unit of ambiguity, and may serve better as a potentially person-specific and event-independent index. At any rate, the parameters a, b and α,ε can readily be transformed into each other and carry the same information. One restriction is that the α maxmin model, unlike our approach, does not allow $a = \varepsilon < 0$.

[8] Schmeidler (1989 p. 574) used the term uncertainty premium for his special case of this index.

We next discuss the alternative interpretations of Chateauneuf et al. (2007) in their equivalent neo-additive model. They assumed Eq. 3.4 for event weights rather than for matching probabilities. In their remark, expected utility is assumed for risk, so that event weights equal matching probabilities. Their Remark 3.2 explains that their model is equivalent to ε-α-maxmin and, hence, Observation 3.6 applies to their model. In our notation, Chateauneuf et al. interpret a as distrust in the subjective expected utility model and $\frac{b+a}{2a}$ as an index of pessimism.

Two contributions of Observation 3.6 to multiple priors theory, at least for the specification considered here, are: (1) our indexes are also valid if expected utility for risk is violated; (2) the ambiguity aversion and the perceived level of ambiguity can be measured very easily, with no need to measure utility U or the set of priors C. Contribution (2) was obtained before by Dimmock et al. (2015) for Ellsberg-urn events with beliefs available.

4. EXPERIMENT: METHOD

Background

This section presents the experiment. Appendix B gives further details. We investigate the effect of time pressure (TP) on ambiguity. The ambiguity concerns the performance of the AEX (Amsterdam stock exchange) index. TP is ubiquitous in applications,[9] and serves well to investigate ambiguity because it allows for easy manipulations. There have been many studies of its effects under risk,[10] but this study is the first for ambiguity. Using our method, we can study TP for natural events.

Subjects

[9] A survey is in Ariely & Zakay (2001). Recent studies include Reutskaja, Nagel, & Camerer (2011) for search dynamics, and Kocher & Stutter (2006), Sutter, Kocher, & Strauss (2003), and Tinghög et al. (2013) for game theory.

[10] See the references in Ariely & Zakay (2001), and Chandler & Pronin (2012), Kocher, Pahlke, & Trautmann (2013), Maule, Hockey, & Bdzola (2000), Payne, Bettman, & Luce (1996), and Young et al. (2012).

N = 104 subjects participated (56 male, median age 20). They were all students from Erasmus University Rotterdam, recruited from a pool of volunteers. They were randomly allocated to the control and the time pressure (TP) treatment.

The experiment consisted of two parts, Parts 1 and 2 (Table 1), consisting of eight questions each. They were preceded by a training part (Part 0) of eight questions, to familiarize subjects with the stimuli. All subjects faced the same questions, except that subjects in the time pressure treatment had to make their choices in Part 1 under time pressure. There were 42 subjects in the control treatment and 62 in the TP treatment. The TP sample had more subjects because we expected more variance there.

TABLE 1: Organization of the experiment

Within subject / Between subject	*Part 1*	*Part 2*
Time pressure treatment	**Time pressure**	No time pressure
Control treatment	No time pressure	No time pressure

Stimuli: Within- and between-subject treatments

Stimuli: Choice lists

In each question, subjects were asked to choose between two options.

OPTION 1: You win €20 if the AEX index increases/decreases by more/less than XX% between the beginning and the end of the experiment (which lasted 25 minutes on average), and nothing otherwise.

OPTION 2: You win €20 with p% probability and nothing otherwise.

We used choice lists to infer the probability p in Option 2 that leads to indifference between the two options. This p is the matching probability of the AEX event. For the TP treatment, a 25-second time limit was set for each choice in Part 1.

Stimuli: Uncertain events

In each part we consider a triple of mutually exclusive and exhaustive single events and their compositions; see Table 2.

TABLE 2: Single AEX-change events for different parts (unit is percentage)[11]

	Event E_1	Event E_2	Event E_3
Part 1	$(-\infty,-0.2)$	$[-0.2,0.2]$	$(0.2,\infty)$
Part 2	$(-\infty,-0.1)$	$[-0.1,0.3]$	$(0.3,\infty)$

For each part, we measured matching probabilities of all six single and composite events, of which two were repeated to test consistency. The order of the eight questions was randomized for each subject within each part.

Stimuli: Further questions

At the end of the experiment, subjects were asked to report their age, gender, and nationality.

Incentives

We used the random incentive system. All subjects received a show-up fee of €5 and one of their choices was randomly selected to be played for real.

Analysis

We compute ambiguity aversion and a-insensitivity indexes as explained in §2. Five subjects in the TP treatment did not submit one of their matching probabilities on time and were therefore excluded from the analysis, leaving us with 99 subjects. Some subjects gave erratic answers violating weak monotonicity; see Appendix C. We nevertheless kept them in the analysis. Excluding the indexes when weak monotonicity is violated does not affect our conclusions (see the full results in the Web Appendix) unless we report otherwise.

Because we obtain two values of each index per subject (one for each part), we run panel regressions with subject-specific random effects[12] to study the impact of TP on a-insensitivity and ambiguity aversion. In the baseline model (Model 1 in the

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[11] In the training Part 0, the events were $(-\infty,-0.4)$, $[-0.4,0.1]$, and $(0.1,\infty)$.

[12] Fixed effects would not allow us to observe the effect of the treatments because the treatment variable is constant for each subject.

result tables), we take Part 1 in the control treatment as the reference group and consider three dummy variables: part 2*control, part 1*TP and part 2*TP, where each variable takes value 1 if the observation is from the specific part in the specific treatment. We then add control variables (age, gender, and nationality in Model 2) to assess the robustness of the results.

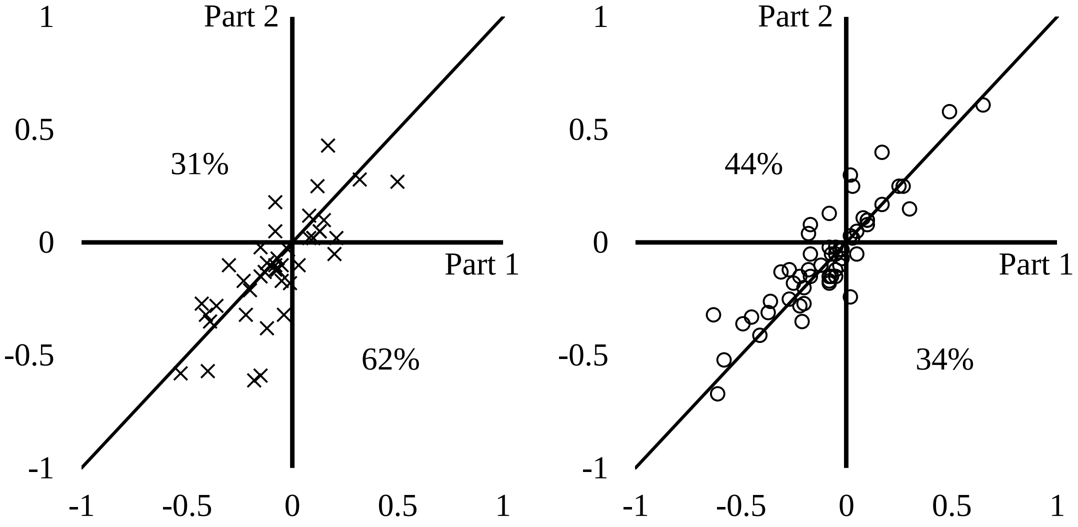
5. EXPERIMENT: RESULTS

In what follows, we report only differences that are significant, with the significance level indicated in the corresponding tables.

5.1. Ambiguity Aversion Index b

Figure 1 presents all *b* indexes of Part 2 as a function of the *b* indexes of Part 1. Correlations are high (ρ = 0.76 for the control treatment and ρ = 0.89 for the TP treatment) and most dots are in the lower left quadrant or in the upper right quadrant. It shows that subjects are consistently ambiguity averse or consistently ambiguity seeking across parts.

FIGURE 1: ambiguity aversion indexes *b*



A. Control treatment (ρ = 0.76) B. TP treatment (ρ = 0.89)

Percentages of observations above and below the diagonal have been indicated in the figures. Correlations ρ *are in the panel titles.*

Table 3 displays the results of the panel regressions for the *b* indexes. In Part 1, the control subjects are slightly ambiguity seeking (−0.07, reaching marginal significance), with the dots in panel A slightly to the left. Regarding our main research question: TP has no effect. The index *b* in TP does not differ significantly from that in the control in Part 1, with dots in panel B not more or less to the left than in panel A. The only effect we find is a learning effect for the control treatment, where Part 2 is a repetition of Part 1.[13] Here ambiguity aversion is lower in Part 2 than in Part 1. There is no learning effect for the TP treatment ($p = 0.14$) because TP in Part 1 prevented the subjects to familiarize further with the task.

All effects described, and their levels of significance, are unaffected if we control for age, gender, and nationality (Dutch / non-Dutch). There is one effect on ambiguity aversion though: older subjects are more ambiguity averse.[14] To test if ambiguity aversion, while not systematically bigger or smaller under TP, would become more or less extreme, we test absolute values of *b*, but find no evidence for such effects (see Web Appendix).

[13] The learning effect is not significant anymore if we exclude the subjects violating weak monotonicity (see Table WB.1 in Web Appendix).

[14] This effect is no more significant if we exclude violations of weak monotonicity.

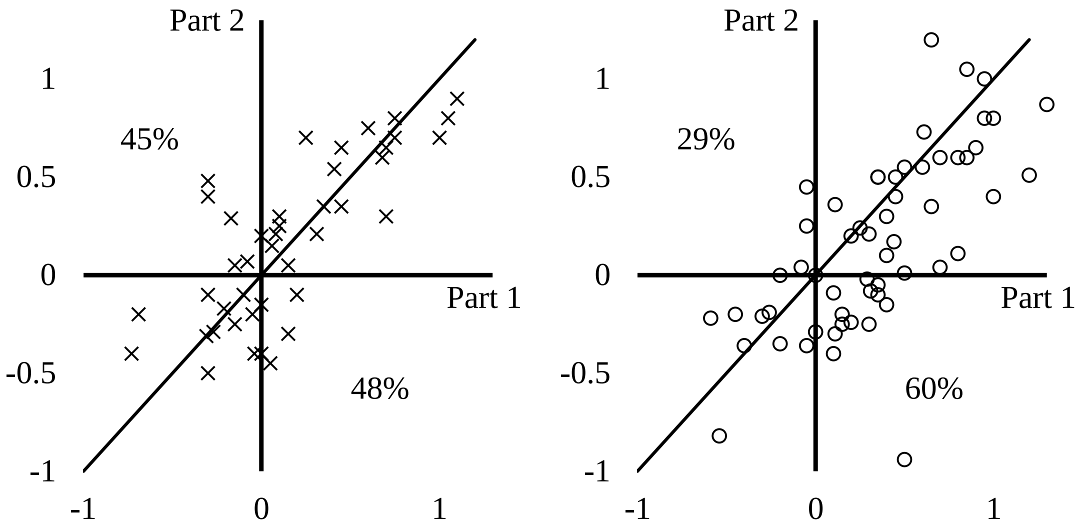
TABLE 3: ambiguity aversion indexes *b*

	Model 1	Model 2
intercept	–0.07^{+}	0.02
	(0.04)	(0.06)
part 1 * TP treatment	**–0.02**	**–0.03**
	(0.05)	**(0.05)**
part 2 * control treatment	–0.04*	–0.04*
	(0.02)	(0.02)
part 2 * TP treatment	0.00	–0.01
	(0.05)	(0.05)
male		–0.08^{+}
		(0.04)
Dutch		–0.07
		(0.05)
age – 20		0.02*
		(0.01)
Chi2	6.79^{+}	21.02**
N	198	198

$^{+}$ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001. Point estimates are followed by standard errors between brackets. The impact of TP is in bold. The variable age has been recoded as age − 20 so that the intercept corresponds to the *b* index of a 20 year-old subject (median age)

5.2. A-Insensitivity Index a

FIGURE 2: a-insensitivity indexes a



A. Control treatment (ρ = 0.77) B. TP treatment (ρ = 0.70)

Percentages of observations above and below the diagonal have been indicated in the figures. Correlations ρ are in *the panel titles.*

Figure 2 depicts all individual a indexes of Part 2 as a function of the a indexes of Part 1. Correlations are again high (ρ = 0.77 for the control treatment and ρ = 0.70 for TP). Table 4 displays the results of the panel regressions for the a index. The insensitivity index is between 0.15 and 0.17 for Parts 1 and 2 of the control treatment (no learning effect and points equally split above and below the diagonal in panel A), and also for Part 2 of the TP treatment. However, there is much more a-insensitivity for the TP questions (Part 1 of TP treatment), with $a = 0.34$ and with two-thirds of the dots in panel B to the right of the diagonal. These findings are robust to the addition of control variables (Model 2). Thus, we find a clear TP effect but no learning effect.

TABLE 4: a-insensitivity indexes a

	Model 1	Model 2
intercept	0.15^{*}	0.20^{+}
	(0.07)	(0.11)
part 1 * TP treatment	**0.19^{*}**	**0.18^{*}**
	(0.09)	**(0.09)**
part 2 * control treatment	0.02	0.02
	(0.05)	(0.05)
part 2 * TP treatment	0.02	0.01
	(0.09)	(0.09)
male		–0.05
		(0.08)
Dutch		–0.06
		(0.10)
age – 20		0.02
		(0.02)
Chi2	17.68^{***}	20.64^{**}
N	198	198

$^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Point estimates are followed by standard errors between brackets. The impact of TP is in bold. The variable age has been recoded as age − 20 so that the intercept corresponds to the a index of a 20 year-old subject (median age)

5.3. *Summary and Discussion of the Experiment*

We briefly summarize the results on response time, consistency, weak monotonicity, and set-monotonicity that are reported in full in Appendix C: subjects use less time in the TP questions. Consistency is violated only in the TP questions, and violations of set-monotonicity occur most frequently in the TP questions. All these results confirm Ariely & Zakay's (2001) observation that TP aggravates biases and irrationalities.

We next summarize the experimental results reported before. TP has no effect on the ambiguity aversion index b, but increases the insensitivity index a. It is plausible that TP harms the cognitive understanding of ambiguity, affecting the discrimination of likelihoods and the perception of ambiguity. Correspondingly, TP induces more violations of consistency and set-monotonicity. It does not lead to a more pronounced like or dislike of ambiguity. For interpreting our results, bear in mind that ambiguity is the difference between uncertainty and risk. TP may increase the aversion to uncertainty, but (and this is our finding), not more or less than the aversion to risk.

Our result on the insensitivity index shows that TP increases the lack of understanding of uncertainty more than that of risk.

Similar to our results, Young et al. (2012) also found that TP increases insensitivity in their context of risk (for losses, with no significance for gains). The effects of TP on risk aversion are not clear and can go in either direction (Young et al. 2012; Kocher, Pahlke, & Trautmann 2013), consistent with absence of an effect on ambiguity aversion in our study. Kocher, Pahlke, & Trautmann (2013) also found increased insensitivity toward outcomes under TP for risk. Our second index will therefore be central for future studies, nudging techniques, and policy recommendations regarding TP.

The absence of ambiguity aversion in our results is not surprising in view of recent studies with similar findings, especially because we used natural events rather than Ellsberg urns (Binmore, Stewart, & Voorhoeve 2012; Charness, Karni, & Levin 2013; Trautmann & van de Kuilen 2015). An additional experimental advantage of using natural events—that suspicion about experimenter-manipulated information is avoided—may have contributed to the absence of ambiguity aversion in our study. Finally, the increase in preference (index b) in Part 2 of the control treatment is in agreement with the familiarity bias (Chew, Ebstein, & Zhong 2012; Fox & Levav 2000; Kilka & Weber 2001).

The events in our experiments were natural in the sense of not involving any artificial concealing of information. We did not consider them in an actually occurring natural decision situation or in a field setting, and the decision situations considered were experimental. However, we used uncertainty that actually occurred and that was relevant to financial traders.

6. GENERAL DISCUSSION

Indexes are simplifying summaries of complex realities. Our indexes cannot be expected to perfectly capture ambiguity attitudes in the same way as the well-known index of relative risk aversion (IRR) cannot be expected to perfectly capture risk attitudes for every decision and every theory. As such, the IRR perfectly describes risk attitudes under expected utility with CRRA utility. In general, it will only work well on restricted domains of outcomes (Wakker 2008). Similarly, our indexes perfectly describe ambiguity attitudes under neo-additive event weighting for several ambiguity theories (§3). In general, they will work well if none of the events in the partition is very unlikely. Violations of event additivity and neo-additive weighting occur primarily for extreme events where no theory describes the many irregularities very well.[15]

Many studies used introspective likelihood measurements (de Lara Resende & Wu 2010; Fox, Rogers, & Tversky 1996; Fox & Tversky 1998; Ivanov 2011) to capture beliefs for natural events. Professional forecasts and survey data are useful for establishing such beliefs (Anderson, Ghysels, & Juergens 2009). But those are not revealed-preference based and the beliefs may be nonadditive. Then ambiguity attitudes may be captured partially by those nonadditive stated beliefs and partially by their weighting functions, and thus, ambiguity attitudes cannot be clearly isolated. Our paper focuses on revealed-preference based concepts.

How ambiguity attitudes are related across different sources of uncertainty, and across different persons, is an important topic for future research in ambiguity theory. The isolation of ambiguity attitudes from beliefs provided by this paper will be useful for such research.

If we have n-fold partitions in a source available with $n > 3$, then natural extensions of our indexes can be defined. We can take average matching probabilities over all events to generalize the aversion index b, and average differences of matching probabilities between events and their subsets to generalize the insensitivity index, where we properly normalize these averages. For big n such calculations quickly become intractable. A more tractable approach is to take some representable three-fold partitions, calculate the indexes for those, and then take averages of those.

[15] Thus, for risk, Kahneman & Tversky (1979 pp. 282-283) explicitly refrained from specifying any shape of probability weighting for extreme probabilities.

7. CONCLUSION

Measuring ambiguity attitudes from revealed preferences up to now was only possible for artificially created ambiguity using Ellsberg urns or probability intervals, with information concealed by an experimenter, or through complex model-fitting. We introduced indexes of ambiguity that do not have these limitations. Our indexes unify and generalize several existing indexes. They: (a) are valid for many ambiguity theories; (b) correct for likelihood dependence of ambiguity aversion; (c) retain validity if expected utility for risk is violated; (d) correct for subjective likelihoods also if unknown; (e) can be used for all, artificial and natural, events. Using natural events will increase external validity. We applied our method in a study on time pressure under ambiguity where our findings are psychologically plausible, confirming the validity of our indexes: time pressure affects cognitive components (understanding, or perceived level of ambiguity) but not motivational components (ambiguity aversion). Correlations between successive measurements of our indexes were high, supporting the reliability of our method.

We can now measure ambiguity attitudes without knowing beliefs and, hence, for all events. We proved this mathematically and demonstrated it empirically.

APPENDIX A. PROOFS FOR §3

PROOF OF OBSERVATION 3.1. Under expected utility for risk, matching probabilities are equal to event weights; i.e., $m_i = W(E_i)$ and $m_{ij} = W(E_i \cup E_j)$. Thus our b is the average of the three values $1 - W(E_i) - W(E_i^c)$. Schmeidler defined ambiguity aversion [neutrality; seeking] as quasiconvexity [linearity; quasiconcavity] of preference with respect to outcome (2nd stage probabilities) mixing, which implies positivity [nullness; negativity] of Eq. 3.2 for all i and, hence, of our b. □

PROOF OF THEOREM 3.3. The distance to be minimized is

$$(m_1 - \tau - \sigma p_1)^2 + (m_2 - \tau - \sigma p_2)^2 + (m_3 - \tau - \sigma p_3)^2$$
$$+(m_{23} - \tau - \sigma p_{23})^2 + (m_{13} - \tau - \sigma p_{13})^2 + (m_{12} - \tau - \sigma p_{12})^2 \,. \qquad \text{(A.1)}$$

The first order condition of Eq. A.1 with respect to τ, divided by -2, gives
$m_1 - \tau - \sigma p_1 + m_2 - \tau - \sigma p_2 + m_3 - \tau - \sigma p_3 + m_{23} - \tau - \sigma p_{23} + m_{13} - \tau - \sigma p_{13} + m_{12} - \tau - \sigma p_{12} = 0 \Rightarrow$

$$\overline{m_c} + \overline{m_s} = 2\tau + \sigma \,. \qquad \text{(A.2)}$$

In words, the level of the best-fitting line, determined by τ, should be such that the line passes through the center of gravity of the data points, being $(\frac{1}{2}, \frac{\overline{m_c} + \overline{m_s}}{2})$. The AD index b' is $1 - 2\tau - \sigma = 1 - \overline{m_c} - \overline{m_s} = b$. □

PROOF OF OBSERVATION 3.4. We already use Eq. A.3 that will be stated in the proof of Theorem 3.5 for convenience. We substitute $p_1 = p_2 = p_3 = \frac{1}{3}$ in Eq. A.3:

$$2\overline{m_c} + \overline{m_s} = 3\tau + \frac{5}{3}\sigma \,.$$

From Eq. A.2 we have $\tau = (\overline{m_c} + \overline{m_s} - \sigma)/2$. We substitute it in the equation above:

$$2\overline{m_c} + \overline{m_s} - \frac{3}{2}\overline{m_c} - \frac{3}{2}\overline{m_s} = -\frac{3}{2}\sigma + \frac{5}{3}\sigma = \frac{1}{6}\sigma$$
$$\Rightarrow \sigma = 3\,(\overline{m_c} - \overline{m_s}).$$

AD defined $a' = 1 - \sigma$ which equals to our index a. □

PROOF OF THEOREM 3.5. We allow p_1, p_2 to be any real value, so that we can apply first order conditions to them. We always take $p_3 = 1 - p_1 - p_2$. Weak monotonicity will imply that p_1, p_2, p_3 are still probabilities; i.e., they are nonnegative.

The first order condition of Eq. A.1 with respect to σ, divided by -2, is:

$$p_1(m_1 - \tau - \sigma p_1) + p_2(m_2 - \tau - \sigma p_2) + p_3(m_3 - \tau - \sigma p_3) +$$
$$p_{23}(m_{23} - \tau - \sigma p_{23}) + p_{13}(m_{13} - \tau - \sigma p_{13}) +$$
$$p_{12}(m_{12} - \tau - \sigma p_{12}) = 0. \tag{A.3}$$

We first consider the case of $\sigma = 0$. Then $\alpha' = 1 - \sigma = 1$. Further, the optimal fit must then hold for all probabilities p_1, p_2, p_3, because they do not affect the distance of the neo-additive function to the data points. Substituting $p_i = 1$, $p_j = p_k = 0$ (with i, j, k distinct) in Eq, A.3 implies $m_i + m_{ij} + m_{ik} = 3\tau$ for all i . Summing over i gives $\overline{m_s} + 2\overline{m_c} = 3\tau$. Subtracting Eq. A.2 gives $\overline{m_c} = \tau$. Then also $\overline{m_s} = \tau$, and $a = 1$. Hence, if $\sigma = 0$ then $\alpha = \alpha'$ and we are done. From now on we assume

$$\sigma \neq 0. \tag{A.4}$$

To substitute the probabilities in Eq. A.3, we consider the first order condition for p_1, divided by -2σ:

$$m_1 - \sigma p_1 - m_3 + \sigma(1 - p_1 - p_2) - m_{23} + \sigma(1 - p_1)$$
$$+ m_{12} - \sigma(p_1 + p_2) = 0\ . \tag{A.5}$$

Then

$$4p_1 = \frac{m_1 - m_3 - m_{23} + m_{12}}{\sigma} + 2 - 2p_2\ .$$

Substituting

$$2p_2 = \frac{m_2 - m_3 - m_{13} + m_{12}}{2\sigma} + 1 - p_1\ .$$

gives

$$p_1 = \frac{3(\overline{m_c} - \overline{m_s}) + 3m_1 - 3m_{23} + 2\sigma}{6\sigma}\ . \tag{A.6}$$

Similarly,

$$p_2 = \frac{3(\overline{m_c}-\overline{m_s})+3m_2-3m_{13}+2\sigma}{6\sigma} \ . \qquad \text{(A.7)}$$

$$1 - p_1 - p_2 = p_3 = \frac{3(\overline{m_c}-\overline{m_s})+3m_3-3m_{12}+2\sigma}{6\sigma} \ . \qquad \text{(A.8)}$$

Substituting Eqs. A.6-A.8 in Eq. A.3, using Eq. A.2, and some tedious but straightforward algebraic moves (see Web Appendix) gives

$$\sigma(3\overline{m_c} - 3\overline{m_s} - \sigma) = 0.$$

Eq. A.4 precludes $\sigma = 0$, and therefore

$$\sigma = 3\overline{m_c} - 3\overline{m_s}. \qquad \text{(A.9)}$$

It implies $a' = 1 - \sigma = 1 - (3\overline{m_c} - 3\overline{m_s}) = a$, which is what we want. We are done if we show that the p_j's are nonnegative, so that they are probabilities.

First note that weak monotonicity ($m_{ij} + m_{jk} \geq m_i + m_k$), when summed over the three i values, implies $\overline{m_c} \geq \overline{m_s}$, so $\sigma \geq 0$. By Eq. A.4, $\sigma > 0$.

We finally show that $p_i \geq 0$ for all i. Substituting Eq. A.9 in Eq. A.6 yields

$$p_1 = \frac{m_{12}+m_{13}-m_2-m_3}{2(\overline{m_c}-\overline{m_s})} \ .$$

The denominator is positive and, by weak monotonicity, the numerator is nonnegative. Hence, $p_1 \geq 0$. Similarly, $p_2 \geq 0$ and $p_3 \geq 0$. Because $p_3 \geq 0$, $p_1 + p_2 \leq 1$. The p_j's are probabilities. □

PROOF OF OBSERVATION 3.6. We prove the result for α-maxmin with α the weight assigned to the worst expected utility, satisfying $0 \leq \alpha \leq 1$. Maxmin expected utility is the special case $\alpha = 1$. To determine the matching probability of an event E, we express outcomes in utility units and calculate the value according to the theory for prospect 1_E0. Because expected utility is assumed for risk, this value is m(E).

$$\begin{array}{llll} 1_E0 \rightarrow & \alpha \inf\{P(E): P \in C\} & + & (1-\alpha)\sup\{P(E): P \in C\} \quad = \\ & \alpha\big((1-\varepsilon)\,Q + \varepsilon\times 0\big) & + & (1-\alpha)\big((1-\varepsilon)\,Q + \varepsilon\times 1\big) \quad = \\ & (1-\varepsilon)\,Q & + & (1-\alpha)\,\varepsilon \end{array}$$

Hence, $\overline{m_s} = (1-\varepsilon)/3 + (1-\alpha)\varepsilon$ and $\overline{m_c} = 2(1-\varepsilon)/3 + (1-\alpha)\varepsilon$.

Therefore, $b = 1 - \overline{m_c} - \overline{m_s} = (2\alpha-1)\varepsilon$ and $a = 1 + 3(\overline{m_s} - \overline{m_c}) = \varepsilon$. □

APPENDIX B. DETAILS OF THE EXPERIMENT

Procedure

In the experiment, computers of different subjects were separated by wooden panels to minimize interaction between subjects. Brief instructions were read aloud, and tickets with ID numbers were handed out. Subjects typed in their ID numbers to start the experiment. The subjects were randomly allocated to treatment groups through their ID numbers. Talking was not allowed during the experiment. Instructions were given with detailed information about the payment process, user interface, and the type of questions subject would face. The subjects could ask questions to the experimenters at any time. In each session, all subjects started the experiment at the same time.

In the TP treatment, we took two measures to make sure that TP would not have any effects in Parts 0 and 2. First, we imposed a two-minute break after Parts 0 and 1, to avoid spill-over of stress from Part 1 to Part 2. Second, we did not tell the subjects that they will be put under TP prior to Part 1, so as to avoid stress generated by such an announcement in Part 0 (Ordonez & Benson 1997).

Stimuli: Choice lists

Subjects were asked to state which one of the two choice options in §2 they preferred for different values of p, ascending from 0 to 100 (Figures B.1 and B.2). The midpoint between the two values of p where they switched preference was taken as their indifference point and, hence, as the matching probability.

To help subjects answer the questions quickly, which was crucial under TP, the experimental webpage allowed them to state their preferences with a single click. For example, if they clicked on Option 2 when the probability of winning was 50%, then for all $p > 50\%$, the option boxes for Option 2 were automatically filled out and for all $p < 50\%$ the option boxes for Option 1 were automatically filled out. This procedure also precluded violations of stochastic dominance by preventing multiple preference switches. After clicking on their choices, subjects clicked on a “Submit” button to move to the next question. The response times were also tracked.

In Part 1 of the TP treatment, a timer was displayed showing the time left to answer. If subjects failed to submit their choices before the time limit expired, their choices would be registered but not be paid. This happened only 5 out of the 496 times (62 subjects × 8 choices). In a pilot, the average response time without TP was 36 seconds, and another session of the pilot showed that, under a 30-second time limit, subjects did not experience much TP. Therefore, we chose the 25 seconds limit.

Figure B.1: Screenshot of the experiment software for single event E_3 in Part 0

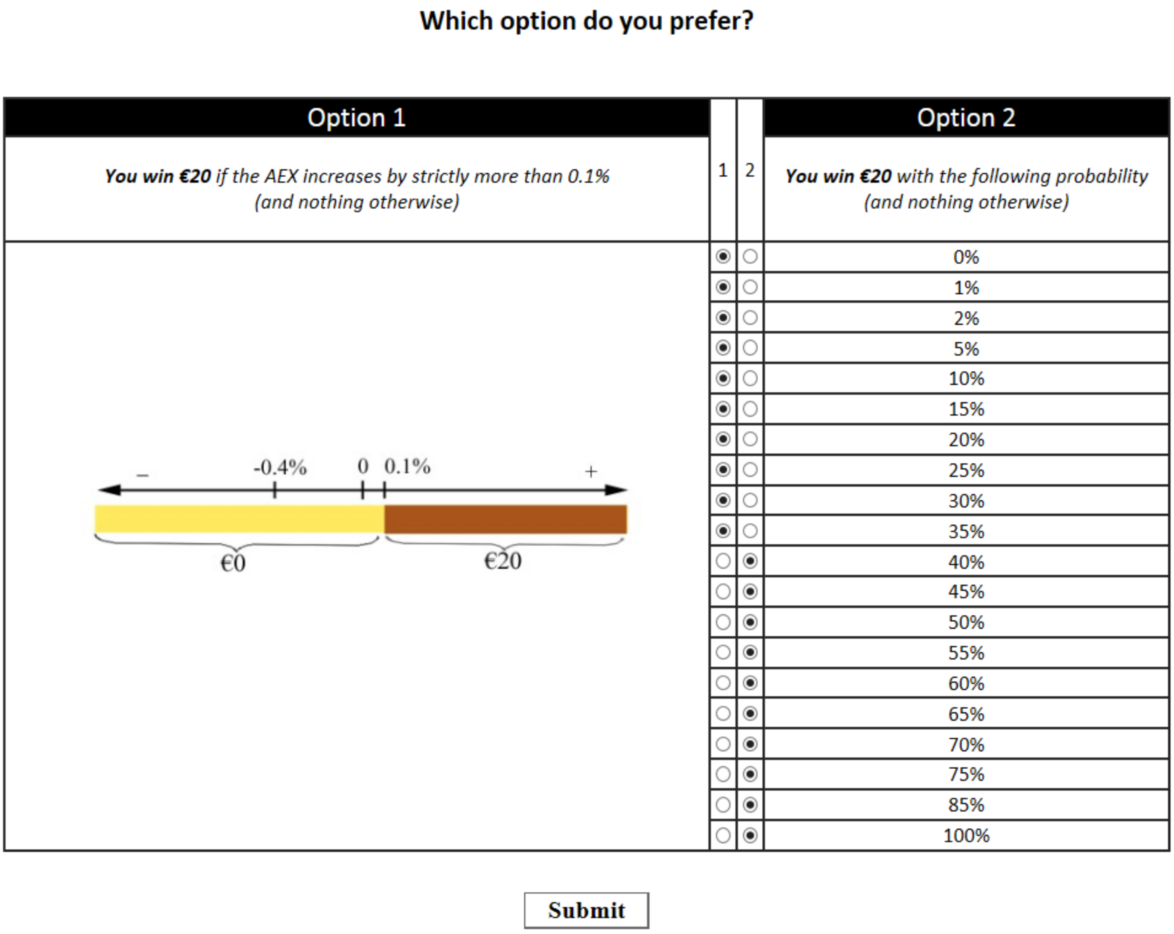
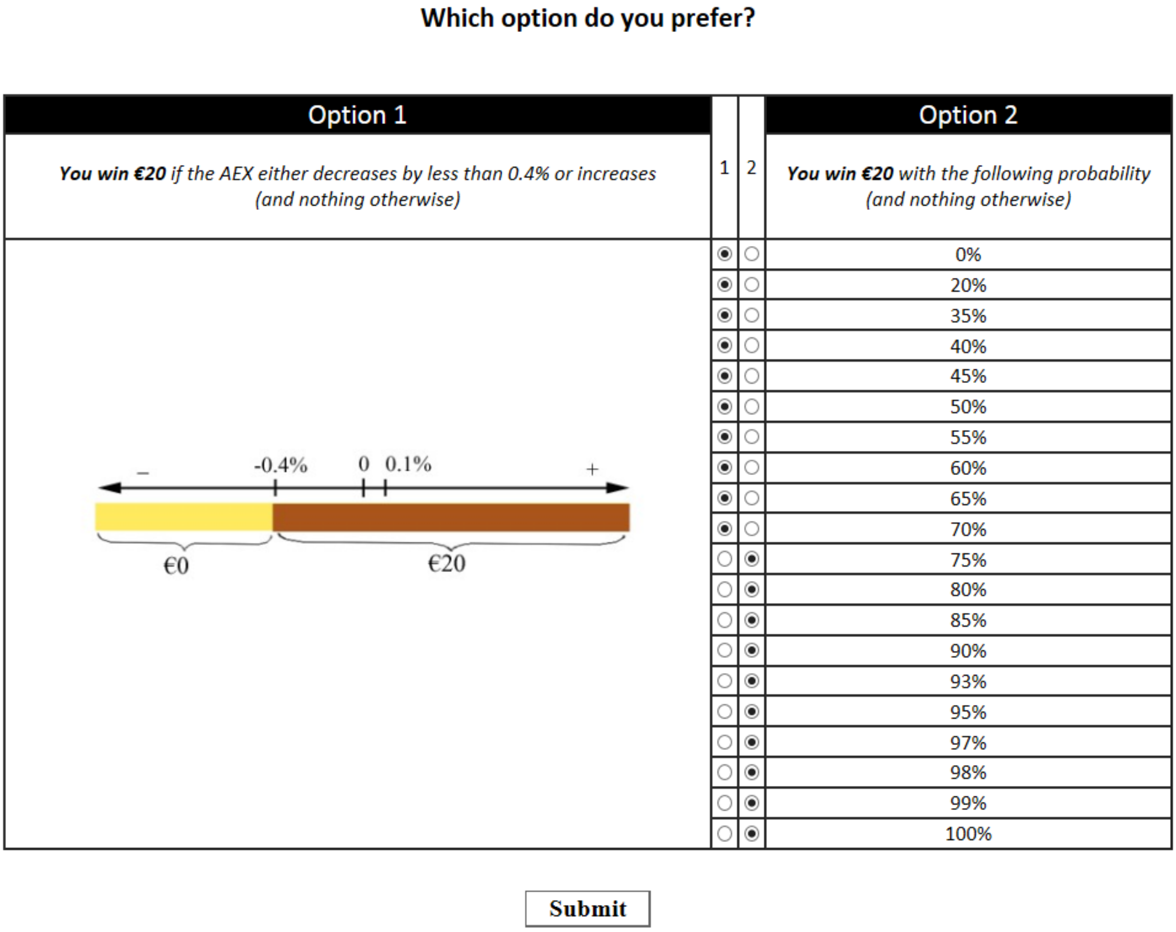


Figure B.2: Screenshot of the experiment software for composite event E_{23} in Part 0



Stimuli: Avoiding middle bias

The middle bias can distort choice lists: subjects tend to choose the options, in our case the preference switch, that are located in the middle of the provided range (Erev & Ert 2013; Poulton 1989). TP can be expected to reinforce this bias. Had we used a common equally-spaced choice list with, say, 5% incremental steps, then the middle bias would have moved matching probabilities in the direction of 50% (both for the single and composite events). This bias would have enhanced the main phenomenon found in this paper, a-insensitivity, and render our findings less convincing. To avoid this problem, we designed choice lists that are not equally spaced. In our design, the middle bias enhances matching probabilities 1/3 for single events and probabilities 2/3 for composite events. Thus, this bias enhances additivity of the matching probabilities, decreases a-insensitivity, and moves our a-insensitivity index toward 0. It makes findings of nonadditivity and a-insensitivity more convincing.

Table B.1 lists the AEX events that we used. Some questions were repeated for consistency checks. The corresponding events are listed twice.

TABLE B.1: List of events on which the AEX prospects were based

Part	Event	Event description
0 (Training)	E_1	the AEX decreases by strictly more than 0.4%
	E_1	the AEX decreases by strictly more than 0.4%
	E_2	the AEX either decreases by less than 0.4% or increases by less than 0.1%
	E_3	the AEX increases by strictly more than 0.1%
	E_{12}	the AEX either increases by less than 0.1% or decreases
	E_{23}	the AEX either decreases by less than 0.4% or increases
	E_{23}	the AEX either decreases by less than 0.4% or increases
	E_{13}	the AEX either decreases by strictly more than 0.4% or increases by strictly more than 0.1%
1	E_1	the AEX decreases by strictly more than 0.2%
	E_2	the AEX either decreases by less than 0.2% or increases by less than 0.2%
	E_2	the AEX either decreases by less than 0.2% or increases by less than 0.2%
	E_3	the AEX increases by strictly more than 0.2%
	E_{12}	the AEX either increases by less than 0.2% or decreases
	E_{12}	the AEX either increases by less than 0.2% or decreases
	E_{23}	the AEX either decreases by less than 0.2% or increases
	E_{13}	the AEX either decreases by strictly more than 0.2% or increases by strictly more than 0.2%
2	E_1	the AEX decreases by strictly more than 0.1%
	E_2	the AEX either decreases by less than 0.1% or increases by less than 0.3%
	E_3	the AEX increases by strictly more than 0.3%
	E_3	the AEX increases by strictly more than 0.3%
	E_{12}	the AEX either increases by less than 0.3% or decreases
	E_{23}	the AEX either decreases by less than 0.1% or increases
	E_{13}	the AEX either decreases by strictly more than 0.1% or increases by strictly more than 0.3%
	E_{13}	the AEX either decreases by strictly more than 0.1% or increases by strictly more than 0.3%

Incentives

For each subject, one preference (i.e., one row of one choice list) was randomly selected to be played for real at the end of the experiment. If subjects preferred the bet on the stock market index, then the outcome was paid according to the change in the stock market index during the duration of the experiment. Bets on the given probabilities were settled using dice. In the instructions of the experiment, subjects were presented with two examples to familiarize them with the payment scheme. If the time deadline for a TP question had not been met, the worst outcome (no payoff) resulted. Therefore, it was in the subjects' interest to submit their choices on time.

APPENDIX C. RESPONSE TIME, CONSISTENCY, AND MONOTONICITY

Analysis

We analyze response time to verify that subjects answered faster in the TP treatment. To do so, we will run panel regressions for the response time as described below. For some events we elicited the matching probabilities twice to test for consistency, since TP can be expected to decrease consistency. For each treatment and each part, we compare the first and second elicitation of these matching probabilities using t-tests with the Bonferroni correction for multiple comparisons. In the rest of the analysis, we only use the first matching probability elicited for each event.

By set- monotonicity, the matching probability of a composite event should exceed the matching probability of either one of its two constituents. Thus, we can test set-monotonicity six times in each part. Weak monotonicity is defined by $m_{ij} + m_{jk} \geq m_i + m_k$ for all distinct i, j, k. Thus, we can test weak monotonicity three times in each part. We will run non-parametric analysis (Wilcoxon tests and Mann-Whitney U tests) to test whether time pressure had an impact on the number of weak and set- monotonicity violations

Results

The average response time in the training part is more than 25 seconds, but it gets much lower in Part 1 and then again in Part 2 for both the control and the TP treatment. Understandably, subjects needed to familiarize with the task. In Table C.1, the benchmark model (Model 1) shows that the average response time of the control subjects in Part 1 is about 17s per matching probability. It is about 4s longer than for subjects under TP, even though the TP-treatment subjects could spend up to 25s to answer. In Part 2, the control subjects answered faster than in Part 1.

TABLE C.1: Response time

	Model 1	Model 2
intercept	16.63***	16.66***
	(1.00)	(1.62)
part 1 * TP treatment	**–4.13****	**–4.44*****
	(1.32)	**(1.33)**
part 2 * control treatment	–2.33**	–2.33**
	(0.71)	(0.71)
part 2 * TP treatment	–1.77	–2.08
	(1.32)	(1.33)
male		–1.45
		(1.24)
Dutch		0.99
		(1.43)
age – 20		0.48
		(0.33)
Chi2	27.82***	31.36***
N	1584	1584

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Point estimates are followed by standard errors between brackets. The impact of TP is in bold. The variable age has been recoded as age − 20 so that the intercept corresponds to the response time of a 20 year-old subject (median age)

We next analyze the consistency of the matching probabilities by comparing repeated elicitations of matching probabilities for some events. Pairwise comparisons for each pair of matching probabilities with the Bonferroni correction indicate one difference, in one of the two tests in Part 1 for the TP treatment: the second matching probability m_{13} is higher than the first one (mean difference = 0.04; $p = 0.01$). The other differences are not significant.

A similar pattern is found within the set-monotonicity tests. Out of 6 monotonicity tests, the average number of violations is 0.58 in Part 1 for the TP treatment, while it is only 0.30 in Part 2 for the same treatment and 0.36 and 0.24 in Parts 1 and 2, respectively, for the control treatment. The difference between Parts 1 and 2 in the TP treatment is significant (within-subject Wilcoxon signed-ranks test; $Z = -2.61$, $p = 0.01$) and the difference between the TP and the control treatment in Part 1 is marginally significant (between-subject Mann-Whitney U test; $Z = -1.71$, p =

0.09[16]). Out of 3 weak monotonicity tests, the average number of violations is 0.16 and 0.11 in Parts 1 and 2 for the TP treatment, and 0.17 and 0.02 in Parts 1 and 2 for the control treatment. None of the differences are significant.

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