# FURTHER THOUGHTS ON POSSIBILISTIC PREVISIONS: A REJOINDER

GERT DE COOMAN

ABSTRACT. This is a rejoinder to the comments [19, 20, 22, 27] by a number of authors on my paper [10] about possibilistic previsions.

I wish to thank the respondents Serafín Moral [19], Romano Scozzafava [20], Lev Utkin [22] and Lotfi Zadeh [27] for their insightful comments on my paper [10]. In this rejoinder, I shall try and address their questions and/or objections individually, and in some detail. Let the reader be advised: my reply falls somewhat short of the usual definition of a rejoinder as "a quick and often angry or amusing answer" (Cambridge Advanced Learner's Dictionary).

## SERAFÍN MORAL [19]

Fuzzy set (and possibility) theory would benefit from a stronger focus on interpretation. This is one of the main motivations for most of the work I have done in this field [6, 7, 8, 11, 13, 17, 18, 24, 25] and in particular for writing the present paper. I agree with Dr. Moral that a similar approach would be (and in some cases already has turned out to be) quite useful in dealing with other topics in fuzzy set theory, and not only for fuzzy probability. In fact, I shall discuss another example further on, in my response to one of Dr. Utkin's questions.

The comments in Sections 2, 3 and 6 of Serafín Moral's reply are very closely related, and I shall address them jointly. Reasonability indeed is not a rationality requirement, although it can be formally linked with the notion of avoiding sure loss in the theory of imprecise probabilities (see Table 1 and Theorems 19–21 in my paper). The underlying idea for all the notions in my paper is that the local models (possibilistic previsions) should be compatible with a global, second-order, model *that is a possibility measure*. So reasonability is a *consistency*, rather than a rationality, criterion (unless you want to maintain that representing local models by global ones that are not possibility measures, is irrational). It is necessary to impose reasonability, if we want to consistently remain within a possibilistic context, and if we want to infer from a given possibilistic prevision on a certain domain, information about other gambles that is still possibilistic in nature (i.e., still is a possibility distribution) – this is what I have called natural extension in my paper. I had to impose a condition like reasonability, simply because I wanted to end up with a theory that could still be related to Zadeh's notion of a fuzzy probability. Indeed, in this light it seems fair to state that whether it makes sense to use fuzzy or possibilistic probabilities hinges on whether reasonability is a reasonable requirement. And, as Dr. Moral rightly points out, this will depend on the context.

Of course, since possibility measures generally represent rather weak states of information, we could try and represent the local models by a global model that is not a possibility measure; a general lower or upper probability would be an obvious choice. This would lead to weaker versions of the requirements of reasonability (the global model must now avoid sure loss) and representability (the global model must now be coherent), and therefore to stronger inferences (we can now use the natural extension of the global model). This, I

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believe, is the essence of the points Serafín Moral is making, and his examples serve to illustrate these ideas quite well.

In fact, in another paper [9], I have presented a behavioural second-order model, called a *lower desirability function*, which formalises precisely these ideas, and the suggestions Dr. Moral is making in Section 6 of his reply. A modeller's lower desirability $\underline{\mathfrak{d}}(X)$ of a gamble $X$ is defined there as her lower probability for the event that a subject will (marginally) accept $X$.[1] This model encompasses all of the uncertainty models in the literature that I am aware of. Moroever, the requirements of rationality and the notion of natural extension that I introduce for them are essentially the ones I described above, and which Dr. Moral also suggests in his comments. Interestingly, for this more general second-order model, we also have *precision-imprecision equivalence* results, similar to the ones given in Section 8 of the present paper. For these functions there are results closely akin to the Eq. (33) that so surprised Dr. Moral; it appears that they indeed represent the essence of precision-imprecision equivalence. And his intuition that this equation is related to our only concentrating on certain types of second-order events is correct: as I show in Example 6.1 of [9], precision-imprecision equivalence disappears when we look at other (complementary) second-order events.

Section 4 of Dr. Moral's reply deals with my comments on Zadeh's fuzzy expectation (Remark 3 in my paper). I agree that if we have an assessment of possibilistic or fuzzy probabilities $\widetilde{P}(\{\omega_k\})$ on the atoms of a finite space $\{\omega_1,\dots,\omega_n\}$ that is reasonable, i.e., such that $\sum_{k=1}^n \widetilde{P}(\{\omega_k\}) \geq 1$, then Zadeh's fuzzy expectation will generally be less informative than, i.e., dominated pointwise by, the natural extension of this assessment. It will therefore be representable only if it coincides with this natural extension, and as I argued in my paper, this will only happen when the $\widetilde{P}(\{\omega_k\})$ are actually precise numbers. I have called such an assessment a *possibilistic probability mass function* in another paper [15], where I and my co-authors also give conditions for its representability and expressions for its natural extension, based on previous work by, amongst others, Serafín Moral [5].

## ROMANO SCOZZAFAVA [20]

Among all the discussants, Romano Scozzafava has the least sympathetic view on my paper. I am grateful for his comments, because they have allowed me to clarify my position.

Dr. Scozzafava's comments in Point 2 reveal that he believes that lower and upper previsions are interpreted, within the behavioural theory of imprecise probabilities, as lower and upper bounds for a true prevision. This view is incorrect. As I tried to make clear in Section 2 of my paper, and as is repeatedly stressed in [23], lower and upper previsions are defined *directly* in terms of a subject's dispositions to buy and sell gambles, *without any reference to an underlying fair price or prevision*. In fact, a fair price is derived as a secondary notion: the common value of a lower and an upper prevision when the two happen to coincide. In addition, the rationality criteria of avoiding sure loss and coherence are defined and motivated using the direct behavioural interpretation of a lower prevision as a supremum acceptable buying price. In summary, lower and upper previsions have nothing to do with Scozzafava's statement that a subject is "unable to assess a precise number" but is "instead able to assess two bounds for that number". This precise number is not *a priori* assumed to exist, it doesn't even enter the picture conceptually, and lower and upper previsions are neither defined nor even considered to be bounds for such a precise number.

It is true that coherent lower previsions can be seen, *a posteriori* and only *mathematically* speaking, as lower envelopes of a set of precise previsions; and that many mathematical results involving coherent lower previsions (for instance the many lower envelope theorems, and the Generalised Bayes' Rule in [23]) can be interpreted *as if* there were a true precise prevision that is only known to belong to a certain set. But the fact that this

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[1] Under additional continuity conditions, it can be related to the buying functions in the present paper [10] by $\beta_X(x) = 1 - \underline{\mathfrak{d}}(X - x)$. See [9, Section 7] for further discussion of the connection between the two models.

so-called *Bayesian sensitivity analysis interpretation* is in many cases (but not all, see for instance the discussion of independence in [23, Chapter 9]) mathematically compatible with the results derived on the basis of the direct behavioural interpretation, should not lead to Scozzafava's mistaken claim about the meaning of imprecise probabilities.

In the previous paragraph, I have emphasised '*as if*', because this is important for dealing with the comments in Dr. Scozzafava's Points 1 and 3. I think I voiced my suspicions against assuming the existence of "true probabilities" in the second paragraph of Section 4 of my paper, and again in the first paragraph of Section 8.1. To rule out any doubts that I now realise can arise: I should have said there that "since $P_T$ is unknown, *or may not even be meaningfully assumed to exist*, we cannot determine whether this event occurs*; in fact it may not be meaningful to consider it as an event.*". So in fact, I tend to agree with Scozzafava on these issues, although I confess to sometimes thinking there are such things as physical probabilities. But my point is that this is of no consequence, and I believe Romano Scozzafava to be mistaken in thinking that I do "not succeed in avoiding the epistemological problems connected with second order uncertainty". In Section 8 of my paper, I define buying (and selling and price) functions in an operationalisable way, and give these notions a direct behavioural interpretation, *without making any assumption about the existence of (anybody's) probabilities*. The underlying events $B(X,x)$ that the subject refuses to buy the gamble $X$ for price $x$ are real events that are "verifiable (at least potentially)"[2], and a buying function summarises a modeller's upper probabilities for such events. I then impose consistency criteria (called pos-coherence in [13] and representability in the present paper, see also Remark 4 in Section 8), which essentially amount to imposing Properties B1–B5 of Theorem 37 (along with a technical continuity condition) if the gambles $X$ belong to a linear space. If the gambles $X$ for which the buying functions are assessed do not make up a linear space, then the consistency criteria amount to requiring that the collection of buying functions can be extended to a consistent collection on a linear space. Observe again that these criteria can be defined *directly* in terms of collections of buying functions, and are in no way based on any underlying assumptions about the existence of ideal or true probabilities. But I am also able to show that there is a one-to-one correspondence between consistent collections of buying functions (satisfying an additional continuity condition) and the full possibilistic previsions of the previous sections. This shows that, very much like coherent lower previsions, consistent (or representable) collections of buying functions can also, besides their direct behavioural interpretation, be given a Bayesian sensitivity analysis interpretation, *as if* there were a precise underlying true probability measure that a modeller provides possibilistic information about. In this sense, the notion of a true underlying prevision or probability can be seen as a (sometimes) useful fictional device, which I have exploited in my paper to serve the didactical purpose of providing a link with existing theories.

With respect to Point 4, I am well aware that conditioning a prior probability with respect to an imprecise likelihood (as in Scozzafava's counterexample) may increase imprecision, i.e., may drive posterior lower and upper probabilities away from each other. This may also happen with an imprecise prior and a precise likelihood function, a phenomenon called *dilation* in the literature. Both types of behaviour are well documented in the literature, see for instance [16, 21, 23, 26]. But when I state in Section 3 of my paper that more information or evidence tends to drive lower and upper probabilities together, I had something different in mind.

But before explaining what I intended to say, let me ask a somewhat rhetorical question: Can any probability be seen as a posterior probability conditional on some event, or in other words, are all beliefs and dispositions results of observations of the occurrence of real events (in Scozzafava's sense)? I think not, and I most certainly did not intend my

[2] assuming we can ask a subject whether he refuses to buy $X$ for $x$, and he is willing and able to answer this question.

statement in Section 3 to imply that coming up with lower and upper probabilities for some event $A$ is the result of conditioning some prior (imprecise) probability – where does this one come from? – on the available information or evidence, represented as an event, nor that such a conditioning process always increases precision (it obviously doesn't).

Let me clarify what I did intend to say by means of an example. Suppose I have a large bag filled with circular metallic discs – they could be coins, or laundry tokens, but you are told nothing about what exactly they are. I will then ask you for your lower and upper probabilities for the event $A$ that the next disk I take out of the bag, will turn out to be a dime. Given that you know almost nothing about the contents of the bag – for all you know there may not be a single dime in the bag, yet on the other hand it might contain nothing but dimes – you should not be disposed to engage in any bet on or against the event $A$, so your lower probability $\underline{P}(A)$ will be (very close to) zero, and your upper probability $\overline{P}(A)$ (very close to) one. Assume on the other hand that you know that I have just returned from a five-year stay in the United States. This might make it more likely to you that the bag contains dimes, so this information or evidence in favour of $A$ may well increase your supremum acceptable rate $\underline{P}(A)$ for betting on $A$ [Can this information be modelled as the occurrence of an event?]. On the other hand, you might also know that – I know this sounds silly, but let's accept it for the sake of the argument – I have just given away most of the dimes still in my possession to my brother, who is an avid collector of all things American [Can this information be modelled as the occurrence of an event?]. This provides you with information against $A$, which makes it less likely for you that $A$ will occur, so it will tend to decrease your upper probability $\overline{P}(A)$, which is one minus your supremum rate for betting against $A$. This, and nothing more nor less, is what I meant when I said that more information tends to bring $\underline{P}(A)$ and $\overline{P}(A)$ closer together.

Of course, not all information can be interpreted as evidence that makes either the occurrence of $A$ or that of its complement more likely; often it will "speak out" in favour, or in disfavour, of both. And in that case a subject's lower and upper probabilities for $A$ may be driven apart. Romano Scozzafava rightly draws our attention to such cases, and his example serves to illustrate this phenomenon quite well. Indeed, only if $r_0 = 0$, i.e., if the urn with known composition contains no white balls, the fact that we draw a white ball from the urn selected at random points unequivocally towards the fact that the selected urn is the one with the unknown composition, and in this case we get $P(A|B) = 1$ from Bayes' rule. But in all other cases, the available evidence has many possible "explanations" $x = 0, 1, \ldots, N$. Only one of these ($x = 0$) excludes the occurrence of $A$, leading to $P(A|B, x = 0) = 0$. For the other "explanations", the evidence is divided, and it tends to point more towards $A$ as $x$ increases to $N$, leading to $P(A|B, x = N) = N/N + r_0$. But however interesting this example may be, I don't think it runs counter to what I said above.

About Point 5: I don't think I stated in my paper that "a precise assessment of the probability of an event $E$ carries more [or less] information than an imprecise one". But let's assume that I did say something along these lines; what, then, does this mean? I mean to state in several instances (see also the previous paragraph for a more exact statement) that more information can lead to more precise assessments, because the increase in available evidence or information may lead a subject to engage in bets with higher betting rates. I also claim in various places that a subject with little relevant information about an event $A$ will not have strong dispositions to bet on or against $A$, and will therefore have nearly vacuous lower and upper probabilities. I sometimes turn this way of reasoning around to say that very precise probabilities are not warranted when little relevant information is available. So I would be inclined to say that a precise probability $P(A) = .5$ reflects stronger dispositions than the pair of lower and upper probabilities $\underline{P}(A) = .4$ and $\overline{P}(A) = .6$. And insofar as we can turn around the link from information to dispositions, I would be inclined to say (perhaps with less justification) that the former assessment is based on less relevant information than the latter. But I would never be tempted to compare, on those same grounds, a precise probability .5 with a lower/upper probability pair .8 and .95 (or a precise probability .9 for that matter).

In his Point 6, Romano Scozzafava also claims that the problems involving coins which I use as running examples in the text are too simple to be really interesting, and can be "easily modelled through suitable Beta distributions". Presumably, he means to imply that a linguistic statement of the type "the coin is loaded so heavily that it will almost always fall with the same side up, only I don't know which side" can be modelled by specifying some precise Beta distribution $B(\cdot|\alpha,\beta)$ on the set $[0,1]$ of possible values of the probability $\theta$ of falling 'heads'. This too leads to a hierarchical model, where both the first and the second order models are precise probability measures. Using a standard coherence argument, similar to the ones followed in Section 6 (and similarly in Section 8) of my paper, this hierarchical model can be reduced to a first order one, which will be a precise probability measure too. In fact, for any gamble $X$ on $\{h,t\}$, we have for the induced precise prevision $P(X)$ that

$$P(X) = \int_0^1 P(X|\vartheta)B(\vartheta|\alpha,\beta)\mathrm{d}\vartheta$$

where of course $P(X|\vartheta) = P_\vartheta(X) = \vartheta X(h) + (1-\vartheta)X(t)$, so we eventually get

$$P(X) = \frac{\alpha}{\alpha+\beta}X(h) + \frac{\beta}{\alpha+\beta}X(t).$$

So this model for our information implies that we should be willing to bet on 'heads' at odds $\alpha/\beta$ and on 'tails' at odds $\beta/\alpha$. If we use the symmetry in the available information to let $\alpha = \beta$, it seems we should be willing to bet on 'heads', as well as on 'tails' at even odds, and to act *as if the coin were fair*. This shows that the behavioural implications of this model are very strong, and it seems to me that they are in no way warranted by, or a reflection of, the information that is actually available. On the other hand, the model that I proposed in Examples 3, 7 and 11 of my paper leads to a vacuous first order model, which has no behavioural implications at all. The claim that "situations like this one can be easily modelled through suitable Beta distributions" seems therefore somewhat too facile.

Of course, I cannot be really sure that the way I use Beta distributions here to represent the available information about the coin, is what Dr. Scozzafava has in mind, because using a Beta distribution seems to imply that the notion of 'a probability of a probability' is at least in some (perhaps indirect) way taken seriously, and this is something he seems to reject categorically. But on the other hand, any way of using Beta distributions that eventually results in a precise probability for heads and tails, will have very strong behavioural implications (it will leave no room for any indecision), which I feel are not warranted by the scarce information that is really available about the coin. Indeed, I strongly support the thesis that a lack of relevant information may lead to *indecision*, and that this indecision should be a part of our models. It really doesn't help to consider very complicated example problems when we can't even reach agreement on the simplest of ideas.

Finally, whether the theory expounded in my paper is indeed "a cut and paste of many existing theories" that "lacks necessity", I shall leave for the readers to decide. Let them wield Ockham's razor.

## LEV UTKIN [22]

I tend to agree with most of Dr. Utkin's comments, and I shall therefore limit myself to suggesting ways to answer the two questions asked near the end of his contribution.

The first question concerns how to deal with independence in the framework of possibilistic previsions, and how to construct joint possibilistic previsions from given independent marginals. Let me give a simple example to show that such things are indeed possible. Consider two random variables $X$ and $Y$ assuming values in the respective sets $\mathscr{X}$ and $\mathscr{Y}$. Assume that the information about the value that the random variable $X$ assume in $\mathscr{X}$ is modelled by a full possibilistic prevision $\mathfrak{p}_X$, defined on some subset $\mathscr{K}_X$ of the set $\mathscr{L}(\mathscr{X})$ of all gambles on $\mathscr{X}$. Similarly, for the random variable $Y$, there is a full possibilistic prevision $(\mathscr{Y},\mathscr{K}_Y,\mathfrak{p}_Y)$ modelling the information about the value that $Y$ takes in $\mathscr{Y}$. To make things as easy as possible from a mathematical point of view, I shall assume that $X$ and $Y$ are random variables whose behaviour is governed by the respective true, physical probabilities (or linear previsions) $P_T$ and $Q_T$.[3] The representation $\mathscr{M}(\mathfrak{p}_X)$ of $\mathfrak{p}_X$ is a possibility distribution on the set $\mathbb{P}(\mathscr{X})$ of all linear previsions on $\mathscr{L}(\mathscr{X})$, whose value $\mathscr{M}(\mathfrak{p}_X)\cdot P$ is the upper probability that $P$ is the true linear prevision $P_T$. Similarly for $\mathscr{M}(\mathfrak{p}_Y)$.

We assume the variables to be stochastically independent, so their common behaviour is governed by the product prevision $R_T = P_T \times Q_T$. We now want to find the greatest (most conservative) possibility distribution $\pi$ on the set $\mathbb{P}(\mathscr{X}\times\mathscr{Y})$ of all linear previsions on $\mathscr{L}(\mathscr{X}\times\mathscr{Y})$ satisfying the following requirements:

$$\sup_{\substack{R\in\mathbb{P}(\mathscr{X}\times\mathscr{Y})\\ R_{\mathscr{X}}=P}} \pi(R) = \mathscr{M}(\mathfrak{p}_X)\cdot P \qquad \text{and} \qquad \sup_{\substack{R\in\mathbb{P}(\mathscr{X}\times\mathscr{Y})\\ R_{\mathscr{Y}}=Q}} \pi(R) = \mathscr{M}(\mathfrak{p}_Y)\cdot Q \tag{1}$$

for all $P$ in $\mathbb{P}(\mathscr{X})$ and $Q$ in $\mathbb{P}(\mathscr{Y})$, where $R_{\mathscr{X}} = P$ for instance means that $R$ has $\mathscr{X}$-marginal $P$. This simply expresses that the marginals $P_T$ and $Q_T$ of the true prevision $R_T$ have respective possibility distributions $\mathscr{M}(\mathfrak{p}_X)$ and $\mathscr{M}(\mathfrak{p}_Y)$. Moreover, in order to take into account the stochastic independence of $X$ and $Y$, we require that if $R$ is a linear prevision that is not a product $P\times Q$ of some $P$ in $\mathbb{P}(\mathscr{X})$ and $Q$ in $\mathbb{P}(\mathscr{Y})$, then $\pi(R) = 0$, i.e., $R$ has upper probability zero, because it cannot express this stochastic independence.

We shall denote the greatest such possibility distribution by $\pi^g$, and it is not difficult to verify that

$$\pi^g(R) = \begin{cases} \min\{\mathscr{M}(\mathfrak{p}_X)\cdot P, \mathscr{M}(\mathfrak{p}_Y)\cdot Q\} & \text{if } R = P\times Q \\ 0 & \text{otherwise.} \end{cases} \tag{2}$$

This $\pi^g$ induces a full possibilistic prevision that we shall call the *type-I product* or *strong extension* of $\mathfrak{p}_X$ and $\mathfrak{p}_Y$, and denote as $\mathfrak{p}_X \times_I \mathfrak{p}_Y$. My reason for using this terminology is that for each $\alpha \in (0,1]$, $\pi^g_\alpha = \mathscr{M}(\mathfrak{p}_X)_\alpha \times \mathscr{M}(\mathfrak{p}_Y)_\alpha$, so at each level $\alpha$, the cut lower previsions of $\mathfrak{p}_X \times_I \mathfrak{p}_Y$ are type-I products [1, 23], or strong extensions [2, 3, 4], of the corresponding cut lower previsions of $\mathfrak{p}_X$ and $\mathfrak{p}_Y$. We have, for any gamble $h$ on $\mathscr{X}\times\mathscr{Y}$ and for all $z$ in $\mathbb{R}$, that

$$(\mathfrak{p}_X \times_I \mathfrak{p}_Y)(h)\cdot z = \sup_{(P\times Q)(h)=z} \min\{\mathscr{M}(\mathfrak{p}_X)\cdot P, \mathscr{M}(\mathfrak{p}_Y)\cdot Q\}.$$

In particular, if $f$ is a gamble on $\mathscr{X}$ and $g$ a gamble on $\mathscr{Y}$, then it follows fairly easily that

$$(\mathfrak{p}_X \times_I \mathfrak{p}_Y)(f+g) = \mathfrak{p}_X(f) + \mathfrak{p}_Y(g)$$
$$(\mathfrak{p}_X \times_I \mathfrak{p}_Y)(fg) = \mathfrak{p}_X(f)\cdot \mathfrak{p}_Y(g)$$

and in particular if $A \subseteq \mathscr{X}$ and $B \subseteq \mathscr{Y}$,

$$(\mathfrak{p}_X \times_I \mathfrak{p}_Y)(A\times B) = \mathfrak{p}_X(A)\cdot \mathfrak{p}_Y(B).$$

The '$+$' and the '$\cdot$' in the right hand sides of these expressions refer respectively to the (fuzzy) addition and the (fuzzy) multiplication of fints. It should be mentioned, however, that these simple summation and product rules for the possibilistic previsions do not carry over to their induced first order models. Nevertheless, as I explained in the paper, these first order models can be obtained from the possibilistic previsions by simple integration.

In the concrete example that Lev Utkin suggests, the two random variables $X$ and $Y$ represent the state (either *work* or *fail*) of two components. Let $Z$ represent the state of a

[3] I believe interesting things could be said in other situations as well, using buying functions rather than possibilistic previsions, but we shouldn't take this discussion too far.

series connection of these components, and assume that these components are independent, then it follows for instance from the considerations above that

$$\mathfrak{p}_Z(\{work\}) = \mathfrak{p}_X(\{work\}) \cdot \mathfrak{p}_Y(\{work\}),$$

or in other words the 'fuzzy reliability' of a series of two independent components is the fint product of the fuzzy reliabilities of the components.

The second of Lev Utkin's questions deals with the ordering of fuzzy numbers (or fints). It is only indirectly related to the topic of this paper, because it is a question about fuzzy numbers in general, rather than fuzzy probabilities or previsions in particular. Nevertheless, I shall try and suggest an answer to this question, also because it serves to illustrate one of the points made in Serafin Moral's comments: we could use the behavioural interpretation of possibility measures to suggest well-founded methods for dealing with existing problems in fuzzy set theory.

So how do we compare two fuzzy numbers? Before we can even begin to look for an answer to this question, we must first turn to matters of interpretation, and formulate clearly what the question means. I will describe one type of situation where a clear interpretation, and a solution method suggests itself: assume that the two fuzzy numbers can be interpreted as possibility distributions $\pi_X$ and $\pi_Y$ for two (bounded) real-valued random variables $X$ and $Y$, and where the corresponding necessity measures $\underline{P}_X$ and $\underline{P}_Y$ are interpreted as lower probabilities, with a behavioural (supremum betting rate) interpretation. What does the available information allow us to say about the event that, say, $X > Y$? One (but not the only) way to look at this problem, is by asking the question: if $X$ and $Y$ were uncertain rewards, which of the two rewards would I choose, given the available information present in (or modelled by) the possibility distributions $\pi_X$ and $\pi_Y$?

To answer this last question, let's first look at how this is done in precise probability theory: Suppose that we have two probability densities $f_X$ and $f_Y$, and let $f$ be any joint density function with marginals $f_X$ and $f_Y$. Then we would choose reward $X$ if the expected reward for $X$ exceeds the expected reward for $Y$, i.e., if

$$\int x f(x,y)\mathrm{d}x\mathrm{d}y > \int y f(x,y)\mathrm{d}x\mathrm{d}y \quad \text{or equivalently} \quad \int x f_X(x)\mathrm{d}x > \int y f_Y(y)\mathrm{d}y,$$

meaning that the centre of mass of $f_X$ lies to the right of that of $f_Y$. Observe that this holds for every possible choice of the joint density $f$, so this answer doesn't depend on any additional assumptions about whether $X$ and $Y$ are independent.

If we now go back to the possibility distributions $\pi_X$ and $\pi_Y$, let $\underline{P}$ be any joint coherent lower prevision whose marginals are the corresponding necessity measures $\underline{P}_X$ and $\underline{P}_Y$. Then the uncertain reward $X$ will be preferred to the uncertain reward $Y$ if (see also Example 10 in the paper, and more generally [23, Section 3.9])

$$\underline{P}(X - Y) > 0.$$

The answer will in this case depend on whether $X$ and $Y$ are assumed to be independent or not! Under fairly general independence assumptions (this will hold for epistemic irrelevance [1, 12, 14], epistemic independence [1, 23], strong independence [1], Kuznetsov independence [2, 4]), we have $\underline{P}(X-Y) = \underline{P}(X) - \overline{P}(Y)$, and so we should prefer $X$ to $Y$ if (see also Section 6 of the paper, and [8])

$$\int_0^1 \inf\{x\colon \pi_X(x) \geq \alpha\}\mathrm{d}\alpha > \int_0^1 \sup\{y\colon \pi_Y(y) \geq \alpha\}\mathrm{d}\alpha.$$

In other words, the "area to the left of the left slope of $\pi_X$" should be strictly greater the "area to the left of the right slope of $\pi_Y$". This generally produces only a weak order on fuzzy numbers, so not any two fuzzy numbers will be comparable on this approach. But given that possibility distributions generally represent only weak information states, this should not really be surprising. For instance, if we only know that $X$ belongs to the interval $[a,b]$ and that $Y$ belongs to $[c,d]$, then $\pi_X = I_{[a,b]}$ and $\pi_Y = I_{[c,d]}$, and we can (according to the formula above) only say that $X$ is greater than $Y$ if $d < a$, which is, of course, what we expect. Observe that if both $a < d$ and $c < b$, we are, on the basis of the available information, *undecided* about which of the two rewards we should prefer, and this indecision should be taken seriously, and not be swept under the carpet.

## LOTFI ZADEH [27]

My answer to Dr. Zadeh's somewhat rhetorical central question "Can [my] paper deal effectively with imprecise probabilities [in cases where] everything, and not just probabilities, is imprecise?" is simple: "No!". It was never intended to. And the reason why the theory in my paper cannot, as it is, solve the deceptively simple problems Lotfi Zadeh poses, is nearly as simple: all of these problems involve working with fuzzy events, and I haven't tried to define the (lower, upper, possibilistic) probability of a fuzzy event $A$. Why not? Because in order to do so in a way consistent with the approach and spirit of my paper, we would need to give a behavioural interpretation to these notions, and in particular also to the membership function $\mu_A$ of such a fuzzy event. I don't know how to do that.

I could opt for the easy way out, and follow Dr. Zadeh in defining the probability of a fuzzy event as the expectation of its membership function. If I indeed could be convinced to define the (lower, upper, possibilistic) probability of a fuzzy event $A$ simply as the (lower, upper, possibilistic) prevision of the membership function $\mu_A$, I could probably have a go at solving a number of the problems Lotfi Zadeh poses. But I do not want to do that, because this would amount to interpreting a fuzzy event, or its membership function, as a gamble, and this has a lot of other implications that would have to be explored first. To clarify what I mean here, consider an example: let $\Omega = [0, 100]$, and suppose I ask for a subject's lower probability that the number $\omega$ I will select from $\Omega$ will be large. Suppose we agree that this lower probability is actually his lower prevision $\underline{P}(\mu_A)$, i.e., the subject's supremum price for receiving the uncertain reward $\mu_A(\omega)$, where $\mu_A$ is *some* membership function for the fuzzy subset $A$ of the large real numbers between 0 and 100. A lot of issues then arise. To mention only a few: Whose membership function do we take (mine or the subject's, or yet somebody else's)? How do we make sure that the membership function we choose – with the obvious behavioural implications of this choice – is a reasonably good model for the fuzzy event that will I select a large number. What is the meaning of "being a good model" for a fuzzy event in this particular context? It is clear that a lot of interesting and fundamental questions need to be asked and addressed here. And it's a great pity that most of the literature that I have seen on the probability of fuzzy events, deals with the more technical aspects of the definitions and techniques in Lotfi Zadeh's Generalized Constraint Language, but rarely touches upon the more fundamental matters of interpretation and motivation. Of course I mean no offense: I highly value Lotfi Zadeh's insistence on asking annoying questions about precise and imprecise probability theory, only I need to be convinced of the validity of the answers he suggests himself, with more solid arguments than the mainly intuitive ones I've come across so far. I'd feel more than rewarded if my paper succeeds in drawing more attention to such issues.

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GHENT UNIVERSITY, SYSTEMS RESEARCH GROUP, TECHNOLOGIEPARK – ZWIJNAARDE 914, 9052 ZWIJNAARDE, BELGIUM

*E-mail address*: gert.decooman@ugent.be