

## ***Interactive comment on “Possibilistic uncertainty analysis of a conceptual model of snowmelt runoff” by A. P. Jacquin***

**A P Jacquin**

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First of all, the author would like convey her appreciation to the Editor Dr. Niko Verhoest and the anonymous referees for their constructive criticisms, which have helped to improve the quality of the manuscript. In particular, their comments pointed out the necessity to define basic possibilistic concepts, as well as introducing changes in the description of the method and the presentation of results. The details on how the remarks made by the Editor and the anonymous referees are addressed in the revised paper are given in what follows.

REPLY TO COMMENTS BY Dr. N.E.C. VERHOEST (EDITOR)

C1299

As indicated by the different reviewers, the possibilistic approach for calibrating a hydrologic model is very interesting. However some important issues have been raised by the reviewers, compelling for a major revision of the paper. In the revised version, the author should carefully address the remarks made by the reviewers, with special attention to the following items:

1. The terminology often is not very precise. Several comments were made hereupon. I also suggest that clear definitions of possibilistic concepts are given as possibility theory still is not widely known as alternative to describe uncertainty.

Response: This point was indeed repeatedly raised by the referees. The author acknowledges that a more formal approach to the description of the method and its theoretical basis is required. An effort was made to improve the readability of the revised manuscript avoiding imprecise terminology. As requested by Dr. Verhoest and the anonymous referees, definitions of basic possibilistic concepts are included. In addition to this, most of the references suggested and also references to existing studies on the application of possibility theory and imprecise probabilities in hydrology are incorporated.

2. It is not clear why the possibility distributions for the parameters was chosen to be rectangular (most probably for computational reasons as you then only have one alpha-cut (i.e.  $\alpha=1$ ) to sample): such choice seems very strange with respect to describing the uncertainty: choosing a trapezoidal or triangular possibility distribution functions seems to be more logic and I would suggest that this option is implemented in the revised version.

Response: Even though there is prior knowledge on the ranges where the optimal parameter values are usually found when the model is calibrated, a lot of dispersion exists between catchments. This means that, before model performance is evaluated, it is not known what regions of the parameter space are more likely to be associated with good model performance in the catchment used case study. The author thinks that

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assigning an initial uniform possibility distribution to the parameter vector is consistent with this poorly informed initial situation. If other sources of information on appropriate parameter values for the catchment case study are available (e.g. the plausibility of the simulated glacier mass balance and snow cover, as proposed in the present study), the initial possibility distribution assigned to the parameter vector should reflect this additional information. The author agrees in that this situation is likely to be different in other modelling problems, where the behavioural regions of the parameter vector may be estimated beforehand. In such cases, the use of triangular or trapezoidal possibility distributions would be a better alternative for representing this knowledge.

In any case, using a uniform initial possibility distribution for the parameter vector does not have serious disadvantages with respect to the possibility distributions of discharge estimates that are ultimately obtained. The method uses a conjunctive rule for combining the initial possibility distribution with the possibility distributions derived from the information on the performance of the model realizations. This implies that the combined possibility distribution of the parameter vector is not distorted by the initial possibility distribution, which has a constant value equal to unity inside the feasible space. The possibility distributions of the discharge estimates at each time step are later derived through the application of the Extension Principle to the aggregate possibility distribution of the parameter vector, which bears the information on the goodness of fit of the model realizations.

3. Using a Monte Carlo simulation, which is a typical probabilistic technique, for sampling the parameter-space at an alpha-level, is quite confusing. Sampling all parameters at a certain alpha-level by discretizing the interval in a 'limited' number of points and simulating with all possible combinations on which the extension principle is applied seems to be more logical.

I am looking forward to receiving your revised manuscript.

Yours sincerely,

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Niko Verhoest

Response: Dr. Verhoest is right in that Monte Carlo sampling is normally used in probabilistic analysis. However, the author believes that choosing this sampling strategy in the present study is not a severe problem from a practical point of view, at least with respect to the uncertainty estimates that are ultimately derived. When the methodology applied in this study was first presented, Jacquin and Shamseldin (2007) proposed the use of a Monte Carlo sampling strategy with uniform probability distributions in order to simplify the calculations, even though it was acknowledged that other sampling strategies were also possible. Firstly, as explained by Jacquin and Shamseldin (2007), the results of the method do not directly depend on the probability distribution that is used for generating the sample, as long as whole feasible space of the parameter vector is sufficiently sampled. A systematic sampling strategy (i.e. non-random) would also be useful, as long as the sample size and the spatial distribution of the sample points ensure that all the regions of the parameter space are explored. To sum up, all that is required is a sample that provides a thorough exploration of the parameter space, in order to empirically derive the possibility distribution of the parameter vector by evaluation of the goodness of fit of the model realizations. A comment on this issue is included in Section 3.2 of the revised manuscript.

The sampling strategy proposed by Dr. Verhoest had been considered by the author, but this approach was not further pursued. As Dr. Verhoest pointed out in Comment 2, using a uniform initial possibility distribution implies that there is only one alpha-cut to sample. Thus, the sampling process would reduce to dividing the feasible range of each parameter with a grid and simulating all possible combinations of parameters. The author thinks that choosing such systematic sampling approach would not be more efficient than random sampling in the current situation. As explained in response to Comment 2, however, the case where the existence of sufficient prior information enables the use of trapezoidal or triangular type initial possibility distributions would justify the application of a systematic sampling procedure in the manner suggested by

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Dr. Verhoest.

#### REPLY TO COMMENTS BY REFEREE 1

The paper presents an application of possibility theory to assess the uncertainty associated to a snowmelt runoff model. Various possibility distributions are built according to different information sources (i.e., REP, REVF, MSE), and associated uncertainty bounds are computed for various combinations of these sources.

The application seems interesting, and possibility theory may be a proper choice, especially if uncertainty is due to parameter ill-known values or to model structure. However, there are many technical aspects that are left unspecified, and it is not crystal-clear how possibility theory is used exactly.

Here is a list of the major technical (and, to some extent, philosophical) deficiencies present in the paper:

1. The vocabulary used by the authors is sometimes improper and may be misleading to readers that are non-experts in uncertainty theories. Authors should pay a close attention to the vocabulary and the concepts they explain, and illustrate them with concrete examples. I think it to be important if authors want the readers to be interested. Here are some examples:

Response: The vocabulary use has been improved. Theoretical concepts have been better explained and examples have been included in the revised manuscript.

1.1. It is often unclear (see introduction, start of page 2057) whether the authors speak about possibility distribution values (i.e., possibility values on singletons or elementary sets) or possibility measures induced by the distribution.

Response: The cases where possibility distribution values and possibility measures are being referred to have been clarified.

1.2. Authors should be clear about what they consider as epistemic and subjective, as

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they are two different concepts. Uncertainty can be epistemic and not subjective (e.g., imprecision due to sensor).

Response: The author understands the point made by Referee 1, even though it is often found that the expressions “epistemic uncertainty” and “subjective uncertainty” are used interchangeably in engineering literature (Ferson and Ginzburg, 1996; Bae et al., 2006; Ross et al., 2009). The expression “subjective uncertainty” has been eliminated and “epistemic uncertainty” is used throughout in the revised manuscript.

1.3. While authors speak about updating (referring to a prior and a posterior), what they really use in my opinion is an information fusion tool aiming at fusing different information source.

Response: Referee 1 is right in that the word “updating” was used in reference to the combination of different information sources. This word was chosen because the possibility distribution of the parameter vector is repeatedly modified as new aspects of the agreement between observed and estimated discharge are assessed. However, the author agrees in that using the words “updating”, “prior” and “posterior” in the sense explained above may be problematic, as these are often found in applications of Bayesian methods. In the revised manuscript, these words are no longer used. The word “prior”, in reference to the possibility distribution assigned to the parameter vector before model performance is assessed, is changed to “initial”. The “posterior” possibility distribution is referred to as “aggregate” possibility distribution. Similarly, the word “updating” is changed to “combining”.

2. It is quite surprising that authors, in their introduction and short bibliographical review, do not speak about recent developments of uncertainty theories such as imprecise probability theory (especially since they speak about subjective probability theory).

Response: As requested by Referee 1, the bibliographical review on uncertainty analysis methods used in hydrology has been expanded, including the use of imprecise probabilities (Hall, 2006; Ghosh and Mujumdar, 2009; Nijssen et al., 2009) and previ-

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ous applications of possibility theory (Verhoest et al., 2007; Mujumdar et al., 2008) in this field.

3. There is a need to formally introduce different basic concepts of possibility, in addition to the (sometimes difficult to follow) explanation of the authors. In particular, the authors never introduce possibility and necessity measures, and the notion of alpha-cuts (page 2059, first lines) is only informally described. A proper notation would help. This is also true for other equations or concepts. For instance, what are the 16 parameters the authors talk about? Are they encompassed in Theta (which in this case is a vector)? Are the model Q and the extension principle ever used (Authors only speak of Theta afterward)?

Response: Basic concepts of possibility theory are introduced in the revised manuscript, including possibility measures and necessity measures. The notion of alpha-cuts is formally defined in Section 2.4 of the revised manuscript.

The parameter vector Theta includes the 16 parameters of the model, as explained in Section 3.2 of the revised manuscript.

The fact that the Extension Principle was used in order to obtain the possibility distribution of the estimated discharge from the possibility distribution of the parameter vector was stated in page 2058, Equation 2, of the original manuscript. This is now explained in Section 3.4 of the revised manuscript.

4. How simulations are performed should be explained clearly, as I did not understand whether Monte-Carlo simulations were performed on the parameter space to evaluate the possibility distributions (propagated through extension principle?) or to possibility distributions (simulating intervals) to achieve the extension principle level-wise?

Response: It is expected that this procedure is better explained in the revised manuscript, but a short explanation is given in what follows. Monte-Carlo simulations are performed on the parameter space, in order to obtain a sample of the parameter

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vector Theta. The goodness of fit of each model realization is assessed and the possibility value of each parameter vector in the sample is subsequently derived (Equations 4, 5 and 7 of the original manuscript). The possibility values associated with different measures of model performance are combined, obtaining aggregate possibility values for the parameter vectors. The Extension Principle is subsequently applied, in order to empirically derive a possibility distribution of the discharge estimates at each time step (Equation 2 of the original manuscript).

5. In general, more figures/algorithms to illustrate the mathematical details could help, especially since the intended audience are non-experts. For example, a figure picturing a possibility distribution and an alpha-cut could be helpful.

Response: New figures have been included in the manuscript. In particular, a figure showing an example of a possibility distribution and some alpha-cuts has been incorporated.

6. The author should also pay attention to the way they write sentences. The English used is sometimes cryptic and the meaning of some sentences difficult to catch.

Response: An effort was made to improve the clarity of the revised manuscript.

7. I now give comments regarding more particular aspects:

7.1. Introduction

- P2054, L-3: I am not sure that "certitude" is a proper English word

Response: It has been checked that certitude is an English word (The Shorter Oxford English Dictionary, 5th Edition).

- P2056, L8: parenthesis for Jacquin and Shamdeldin, 2007 are only around the year. This happens at other places and for other references, please check.

Response: This referencing format is permitted in this journal.

C1306

## 7.2. Possibilistic method for uncertainty analysis

- P2057, L1: a possibility distribution is not an indication of credibility, but rather of plausibility. A necessity measure (that is not introduced) would be an indication of credibility.

Response: As pointed out by Referee 1, the possibility degree of an event  $S$  indicates the extent to which the occurrence of  $S$  is plausible, whereas the necessity degree of this event, indicates the degree of certainty in the occurrence of  $S$ . In the epistemic view of plausibility, the plausibility of an event can be seen as the degree to which its occurrence would not be surprising, in the sense that the potential surprise assigned to the event reflects the extent to which the evidence available to the observer is in contradiction with its occurrence (Dubois and Prade, 1998). In the original manuscript, the expression "model credibility" was used in the sense of "model plausibility", understood as the extent to which the evidence available fails to refute the model representation. The word credibility was preferred because the author thought that non-experts readers would understand it more easily than plausibility. However, the author agrees in that using the word "credibility" may be misleading. Accordingly, "credibility" is removed from the revised manuscript and "plausibility" is used throughout.

- P2057, L4-5: please define clearly (formally) what is meant by alpha possibility bounds. If they correspond to alpha-cut bounds, why not say so?

Response: The alpha possibility bounds correspond to the upper and the lower bound of the strong alpha-cut of the possibility distribution of the discharge estimates at each time step. At each time step, the alpha possibility bounds necessarily enclose all the discharge estimations having possibility values strictly higher than alpha; but, the alpha possibility bounds may additionally enclose some discharge estimations with smaller possibility values. Thus, the alpha possibility bounds are not necessarily alpha-cut bounds. This distinction is explained in Section 3.4 of the revised manuscript, but a short discussion is given in what follows.

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The strong alpha-cut of the possibility distribution of the discharge estimates at each time step is the set of all discharge estimates  $q^*$  with possibility values strictly greater than alpha. If the possibility distribution of the discharge estimates is unimodal, then each strong alpha-cut is an open interval. In this case, the set of discharge estimates enclosed by the alpha possibility bounds (i.e. the lower and the upper bound of this open interval) is exactly the same as the strong alpha-cut. However, if the possibility distribution of the discharge estimates is not unimodal, the set of discharge estimates enclosed by the alpha possibility bounds and the discharge estimates inside the strong alpha-cut are not the same at all possibility levels alpha. In this situation, only possibility levels alpha that are higher than all local maxima different from the global maximum, and possibility levels that are lower than all local minima, define strong alpha-cuts that are open intervals. For a non-unimodal possibility distribution, a possibility level alpha that does not fulfil these requirements defines a strong alpha-cut that is a collection of open intervals instead of a single open interval. In this case, the alpha possibility bounds enclose a range of discharge estimates whose possibility values are not all greater than alpha.

- P2057, L6-7: in fact, alpha -cuts can be given a frequentist flavour (as an interval in which the true value may fall with a lower probability of  $1 - \alpha$ ), by considering a possibility measure as an upper probability (see the paper of Dubois-Prade, "when upper probabilities are possibility measures").

Response: Referee 1 is right in that possibilities can be related to frequencies by considering possibility degrees as upper probabilities. There are methods for deriving a possibility distribution from the probability distribution induced by a histogram of observations, and methods for transforming a possibility distribution into a probability distribution with a frequentist interpretation (see e.g. Dubois and Prade, 1986; 1993; 1998; Civanlar and Trussel, 1986). In the present study, however, possibilities are interpreted as plausibility degrees and no attempt is made to establish a relationship between possibility and frequency values. At each time step, the possibility distribution of the

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estimated discharge is derived from the information on the goodness of fit of the model representations and the author believes that there is no reason to expect that the alpha possibility bounds will enclose a fixed fraction alpha of the observations. Links to studies dealing with probability-possibility transformation have been added to Section 2.6 of the revised manuscript.

- P2058, L1: what are performance criteria? Do they correspond to different modelling of the snowmelt?

Response: A "measure of model performance" is a statistic of the model errors that evaluates the model's ability to provide satisfactory discharge estimates. Different measures of model performance evaluate different aspects of the model's fitness (see e.g. Martinec and Rango, 1989; Legates and McCabe, 1999). This concept is not explained in the revised manuscript, because it is widely used in hydrological literature.

-P2058, L11: again, I'm not convinced by the use of posterior/prior here, as in my opinion what is performed is information fusion here, not the counterpart of a statistical inference scheme.

Response: As explained in response to comment 1.3 by Referee 1, the words "prior" and "posterior" are no longer used in the revised manuscript.

- P2059, L5: I did not fully understand the sentence.

Response: The issue was discussed in the response given to the comment 7.2 on P2057 by Referee 1. An explanation (including an example) is given in the revised manuscript (Sections 2.4 and 3.4).

### 7.3. Model description

- P2059, L15: it is claimed that the 16 variables are independent of each others. How is it taken into account in the propagation?

Response: The model evaluated has a total of 16 independent parameters, all of which

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may be subjected to calibration. In the present study, a Monte Carlo sample of the parameter vector is generated using a uniform random number generator, by varying all 16 parameters simultaneously and independently. For further details on the process of uncertainty propagation, please refer to the response given to Comment 4 by Referee 1.

- Perhaps Section 4 and section 3 could be blend together, as both are quite short?

Response: Section 3 and section 4 are combined, as suggested by the referee.

### 7.4. Methodology

- P2062, Eq. 4: where is Theta in the right-hand part of the equation??? Same remark for the other equations.

Response: Due to an involuntary mistake, Theta is missing in the right-hand part of these equations. This problem is fixed in the revised manuscript.

- It is not clear at all how the extension principle is applied to these possibility distributions (all the same for the 16 parameters Theta?), or is it already applied somehow?

Response: Please refer to the response given to Comment 4 by Referee 1. An effort has been made to explain the application of the Extension Principle more clearly in the revised manuscript (Section 3.4).

### 7.5. Results

- The shown results seem good, however it is really difficult to judge their relevance given the fact that the process through which they are obtained (i.e., the final possibility distributions) remain quite cryptic.

Response: An effort has been made to improve the readability of manuscript. The author hopes that the methodology followed in the study is now more clearly explained.

- Are 0 percent possibility bounds equal to the alpha-cut of level 0 (corresponding to a

C1310

confidence level of 1)? I think that for usual readers it may be counter-intuitive to see that intervals decrease in size as "confidence" percentage increase, hence the need to really introduce necessity measures of alpha-cuts.

Response: Although necessity measures are introduced in the revised manuscript, the author would like to use the terminology "alpha possibility bounds", as this is the manner in which the uncertainty bounds were named when the possibilistic method applied herein was first introduced by Jacquin and Shamseldin (2007). Introducing this change may be confusing to readers that already know the method.

- P2069, L19: possibility

Response: This mistake is corrected in the revised manuscript.

8. I would suggest to the authors to read the two following papers (and some reference therein): -Oberguggenberger, M., King, J., and Schmelzer, B. 2009. Classical and imprecise probability methods for sensitivity analysis in engineering: A case study. *Int. J. Approx. Reasoning* 50, 4. - Cedric Baudrit, Didier Dubois: Comparing Methods for Joint Objective and Subjective Uncertainty Propagation with an example in a risk assessment. *ISIPTA 2005*: 31-40.

Response: The second reference, and several others, have been incorporated in the revised manuscript. The first reference is not included, because imprecise probabilities are used in an example application from aerospace engineering. References on the use of imprecise probabilities in hydrology have been included instead.

#### REPLY TO COMMENTS BY REFEREE 2

The paper considers the problem of snowmelt runoff model parameter determination by using a possibility theory based statistical inference scheme.

The application issue is interesting and its characteristics concerning incomplete and imprecise knowledge justify in a general sense the consideration of possibility theory. However the possibility statistical inference considered raises many questions. The

C1311

presentation is quite good in general but a few important aspects are not completely clear.

Hereafter more specific comments:

1. A sub-section introducing shortly the basic notions of possibility theory (possibility and necessity measures, possibility distribution, alpha cuts, Extension Principle) has to be added at the beginning of section 2. Links with plausibility and credibility measure of the Dempster-Shafer theory have to be mentioned as well as likelihood semantics and confidence interval semantics of possibility distributions (D. Dubois, S. Moral, H. Prade, A semantics for possibility theory based on likelihoods. *J. of Mathematical Analysis and Applications*, 205, 1997, pp.359-380; D. Dubois, L. Foulloy, G. Mauris, H. Prade, Probability-possibility transformations, triangular fuzzy sets, and probabilistic inequalities, *Reliable Computing*, 10, 2004, pp. 273-297).

Response: As suggested by Referee 2, a section introducing basic notions of possibility theory has been incorporated in Section 2 of the revised manuscript. Links to Dempster-Shafer theory of evidence, as well as likelihood semantics and the existence of a relationship with confidence intervals, have been mentioned. The references provided have also been incorporated.

2. The equation 2 which is the heart of the knowledge integration process looks like a kind of generalized Bayes theorem. However the conditioning on some events or observations does not appear in the possibility distribution considered. In particular the parameter is not written in the right part of the equations 4, 5, 7. I am not sure the author intent is to really make a conditioning but merely to fuse the different possibility distributions associated with performance measures. This crucial point has to be clarified in order to justify the origin of equation 2.

Response: The author believes that the referee is discussing Equation 1 rather than Equation 2 of the original manuscript. In that case, the referee is right in that Equation 1 is used for combining possibility distributions originating from different information

C1312

sources (i.e. different measures of model performance) and that it represents a data fusion process rather than a Bayesian revision of possibility distributions. The author thinks that the names “prior” and “posterior” are the reason for this confusion. As explained in response to Comment 1.3 by Referee 1, these names are no longer used in the revised manuscript. The parameter vector is wrongly missing in the right-hand side of Equations 4, 5 and 7 of the original manuscript, but this has been corrected.

3. Concerning the result evaluation of uncertainty bounds, the choice of the alpha level (0 percent, 50 percent, 75 percent) is quite arbitrary. I think that building a possibility distribution of the observations and then compare (in terms of inclusion or intersection) this one with the prediction possibility distribution would be more founded.

Response: The author believes that the line of work suggested by Reviewer 2 is very promising. Recent studies dealing with the quantification and propagation of discharge data uncertainty have explicitly recognized that uncertainty in the rating curve that is used for the estimation of discharge from stage observations is a major source of uncertainty in discharge measurements (e.g. Liu et al., 2009; McMillan et al., 2010). Unfortunately, site information on the stage-discharge measurements that were used to derive the rating curves is not available to the author, which prevents a direct estimation of the magnitude of discharge measurement errors. Consequently, a possibility distribution of the “real” discharge at the catchment’s outlet cannot be estimated from objective site-specific information. In further studies, the author intends to investigate how possibility distributions of the “real” discharge could be derived from “soft” information provided by experts and/or from recommendations found in hydrological literature.

The author agrees in that the choice of the alpha level is subjective. The full description of predictive uncertainty can only be provided by the possibility distribution of discharge estimates at each time step. Nevertheless, the alpha possibility bounds presented in the manuscript were chosen as examples of how the uncertainty bounds change as the alpha level increases. In addition this, an example plot of the possibility distribution of the discharge estimates is presented in the revised manuscript, showing the possibility

C1313

bounds at several alpha levels together with the value of the observed discharge.

#### REPLY TO COMMENTS BY REFEREE 3

General comment: This manuscript uses the concept of possibility distributions to deal with epistemic uncertainty in the context of a conceptual hydrological model. In my opinion, this is an interesting approach to deal with this type of uncertainty. However, I still have some remarks about the methods and terms used in this paper.

1. The main formula (Equation 1, p4), which is the basis of the methodology is used similarly as the probabilistic Bayes’ rule in order to update prior possibility distributions. Nevertheless, it is my opinion that the problem at hand in essence is a data fusion problem. Also, why did the author choose for the use of the product in Equation 1, which is very restrictive and in the case of total conflicting information between two sources, one would end up with a zero possibility degree and poorly reliable results can be obtained (see the paper of Destercke et al, 2009 for further information, see below this comment for the reference).

Response: Referee 3 is right in that Equation 1 of the original manuscript represents a data fusion process and not a Bayesian revision of possibility distributions. The author thinks that the names “prior” and “posterior” are the reason for this confusion. Accordingly, as explained in response to Comment 1.3 by Referee 1, these expressions are no longer used in the revised manuscript.

The author agrees in that using a conjunctive operator may be restrictive, and also inapplicable if there was total conflict between the information sources being combined. However, although the author agrees in that there will always be a level of conflict between different performance criteria, it is unlikely that a total conflict exists if the model structure of the watershed model at hand is indeed appropriate for modelling the runoff generation process of the catchment. If this is the case, it is expected that several parameter vectors can be found that are able to produce estimated discharge hydrographs that approximately fit the observations. In these situations, multi-criteria

C1314



calibration strategies are indeed aimed at finding compromise solutions with respect to all of the aspects of model fitness that are being evaluated (see e.g. Gupta et al., 1998; Seibert and McDonnell, 2002; Engeland et al., 2006).

With these issues in mind, the author would like to explain the choice of a conjunction as a data fusion operator, as originally proposed by Jacquin and Shamseldin (2007) when the method applied in the present study was presented. As explained by Dubois and Prade (1994; 1998), a conjunctive combination of the possibility distributions is justifiable if all sources of information are seen as equally reliable, as assumed in the study by Jacquin and Shamseldin (2007) and also in the present study. The normalized product operator is chosen, because it allows a reinforcement of possibility degrees and it is also associative, which are advantages with respect to the normalized minimum operator (Dubois and Prade, 1994; 1998). Adaptive combination rules (Dubois and Prade, 1994; Destercke et al. 2009), which are useful when there is a level of conflict between sources of information, may also be appropriate, but they have not been tested. This subject is discussed in Section 3.3 of the revised manuscript.

The application of adaptive combination rules would imply a major modification to the method proposed by Jacquin and Shamseldin (2007). This method has been applied in very few cases (Jacquin and Shamseldin, 2007, 2009) and the present author believes that it would be convenient that more experience with the method exists before important changes are introduced. In particular, the present study is the first attempt to investigate the applicability of the method in snowmelt runoff modelling. This is the main research gap that the present study is intended to address.

2. On page 7 (lines 22- 23), the author mentions that "the feasible ranges for the model parameters are defined so that they are wider than the ranges of optimal parameter values found in previous applications of the model." Why did the author then choose to use uniform possibility distributions instead of trapezoidal possibility distributions with the optimal parameter range as the core of the distribution?

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Response: Please refer to the response given to Comment 2 by the Editor.

3. Furthermore, I do not adhere the use of the Monte Carlo sampling strategy, originating from a probabilistic framework, in a possibilistic framework. How well are the 80000 parameter values, which is very small given the 16 dimensional parameter space, distributed in the sampling space?

Response: The referee is right in that Monte Carlo sampling is more usually found as part of probabilistic frameworks, but the author believes that choosing such a sampling strategy in the present study is not a severe problem from the point of view of the uncertainty estimates that are ultimately derived, as explained in response to Comment 3 by the Editor. All that is required is a sample that provides a thorough exploration of the parameter space, in order to empirically derive performance-based possibility distributions of the parameter vector. A comment on this issue is included in Section 3.2 of the revised manuscript.

In this study, the sample was produced using independent uniform probability distributions for each parameter, implying that the 80000 parameter vectors in the sample are uniformly distributed in the parameter space. Preliminary experiments with varying sample sizes were performed, with the aim of establishing what sample size is appropriate for the model and the catchment case study. This sample size was selected because it was observed that further increases in the number of parameter vectors in the sample did not produce significant changes in the uncertainty bounds (nor in the possibility distributions of discharge estimates). A comment is included in Section 5.1 of the revised manuscript. An example showing a possibility distribution of discharge estimates, together with some of its uncertainty bounds, is also incorporated in section 6.

Even though other sampling strategies would be possible, the author would like to use the method of Jacquin and Shamseldin (2007) in the same manner that it was originally proposed. As explained before, this method has only been tested few cases

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and the applicability of the method to model structures that include a snowmelt runoff component had not been explored until now. This is the main research gap that the present study is intended to address. Although it is certainly true that more efficient sampling strategies should be developed, the author feels that this issue could be better dealt with in further studies.

4. Throughout the manuscript, the author uses the term possibility bounds, I suppose that the author refers to alpha-cuts, which is the usual term. Furthermore, the author indicates possibility levels or bounds in percentages (see e.g. p 12 line 14, table 1, etc.). Please note that the meaning of a possibility level is not a frequency, and percentages are hence not used.

Response: In order to avoid repetition, the author would like to kindly refer to the response given to comment 7.2 about P2057 L4-5 by Referee 1. With respect to percentages, these are no longer used in the revised manuscript, as requested by Referee 3.

5. Other comments:

-P3, line 26: "the possibility of a discharge prediction" this should probably be "the possibility degree" (also on lines 28, 29).

Response: The referee is right. This should be "possibility degree".

-Line 27: the symbol alpha is used, however, conform the usual notation this should be pi.

Response: This sentence has been removed from the revised manuscript.

-P10-11: Alternative possibility distributions are used on the basis of prior knowledge. How do these distributions look like? Are these uniform possibility distributions albeit narrower than the ones described in the previous sections?

Response: An alternative definition of the prior possibility distribution (called "initial"

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possibility distribution in the revised manuscript) that evaluates the plausibility of the simulated glacier mass balance and snow cover at the end of the calibration period is considered. This possibility distribution assigns a possibility value of unity if the simulated glacier mass balance and snow cover fulfil the constraints, and a null possibility value otherwise. This possibility distribution has a patchy appearance rather than being uniform, because the model structure is affected by strong interactions between some of the model parameters (Jacquin and Sánchez, 2009).

-P11: In my opinion, figures 2 and 3 are quite redundant, conclusions drawn from these figures are obvious.

Response: Figures 2 and 3 were included because it was thought that they could help illustrate (more clearly than a table) how the total number of simulations retained decreases as the possibility level increases for the different possibility distributions included in the analysis. Following the referee's suggestion, figure 3 is removed from the revised manuscript and only figure 2 is left for illustrating the points made in the discussion.

-P12: the author introduces the Nash and Sutcliffe performance index, although already three indices were used. What is the added value of this new performance index, which isn't used in the methodology to obtain the "posterior" distributions, only MSEBC, REV F and REP.

Response: The efficiency criterion of Nash and Sutcliffe (1970), based on the mean squared error between model estimated and observed discharges, has several deficiencies that include a high sensitivity to outliers. For this reason, it is not used for the derivation of a possibility distribution of the parameter vector in this study. In spite of these deficiencies, it is also the case that the efficiency criterion of is one of the most widely used performance measure in hydrological modelling. The author believes that the reported efficiency values can be easily interpreted by the intended reader, an advantage that is not shared by the more robust (but less widely accepted) performance

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measure based on the mean squared error of the Box-Cox transformed discharge.

-In my opinion, figures 6 and 7 can be combined in one figure, as it is quite obvious that narrower intervals will be obtained as the possibility degree rises.

Response: As suggested by the reviewer, the plots showing uncertainty bounds at different possibility levels are combined.

-Figures 8 and 9, the different linestyles are not clear to distinguish. Furthermore, these figures show the width of the prediction bounds for the verification period, which is smaller than one year (from Julian Day 250-350, if I understood this well). From figures 6 and 7, one can see that the discharge does not show a high frequency of peaks and baseflow, only one discharge peak is observed a year. Furthermore, a large uncertainty is observed for high discharge values (as stated on p13, lines 15-16). Hence, I do not understand this rapid change in the width of the intervals at different possibility degrees? Why is this behaviour so different than the one that can be deduced from figures 6 and 7?

Response: Simulations in this study are performed at monthly time steps. The calibration period has a total of 21 years, while the verification period has a total of 8 years. The plots in figures 8 and 9 show the width of the uncertainty bounds from month 250 ( $21 \times 12 + 1$ ) up to month 348 ( $29 \times 12$ ) of the simulations. This is why these plots show several "peaks" on the width of the uncertainty bounds, as pointed out by the reviewer. In order to avoid confusion, years are used in the revised versions of figure 8 and 9. With respect to the presentation of these figures, linestyles have been changed as suggested by the referee.

reference: S. Destercke and D. Dubois, 2009, Possibilistic Information Fusion Using Maximal Coherent Subsets, IEEE Transactions on Fuzzy Systems, vol 17, no. 1.

Response: This reference was incorporated in the revised manuscript.

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