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Interactive comment on "Moving beyond the cost-loss ratio: Economic assessment of streamflow forecasts for a risk-averse decision maker" *by* Simon Matte et al.

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We thank you very much for your comments and suggestions which will help us to improve the manuscript. Some of those comments and suggestions rejoin some concerns also expressed by Reviewer 1. In the following, we address each comment and suggestion. In some cases, we refer to our (rather lengthy) response to Reviewer 1.

Abstract

- No abbreviations should be used in the abstract without explanations.

- A sentence summarizing the main conclusions of the work should be added.

We will replace CARA by "Constant Absolute Risk Aversion" in the abstract, according to your suggestion. As for the addition of " A sentence summarising the main conclusion of the work", it was precisely the goal of the last sentence of the abstract: "It is found that the economic value of a forecast for a risk-averse decision maker is closely linked to the forecast reliability in predicting the upper tail of the streamflow distribution." We will also add a sentence clarifying the impact of this finding for the design of future forecasts.

Introduction

- **Again, abbreviations are not well explained. Please provide the full term when the abbreviation is used for the first time. Please check the whole paper.**
  We verified the entire manuscript for acronyms and obtained the following list (the pages and line numbers refer to version of the manuscript initially submitted):
  - CARA: Constant Absolute Risk Aversion. Appears for the first time in the abstract and **will be defined there in the revised version of the manuscript**.
  - HEPEX: Hydrological Ensemble Prediction EXperiment. Appears for the first time at line 1 of the introduction and **will be defined there in the revised version of the manuscript**.

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- DEH: Direction de l'Expertise Hydrique. Appears for the first time at line 3 of page 3 and is already defined there.
- vNM: von Neumann and Morgenstern. Appears for the first time at line 32 of page 2 and is already defined there. Typo in the "Neumann" pointed out by Rev. 1 was corrected in the revised version of the manuscript.
- CRPS: Continuous Ranked Probability Score. Appears for the first time at line 31 of page 11 and is already defined there.
- INRS: Institut National de la Recherche Scientifique. Appears for the first time at line 17 of page 5 and is already defined there.
- RHHU: Relatively Homogenous Hydrological Unit. Appears for the first time at line 21 of page 5 and is already defined there.
- SWE: Snow Water Equivalent. Appears only once, on page 6 line 2 and is defined there. **We might remove the acronym since we are not using it later**.
- BV3C: *Bilan Vertical en 3 Couches*. Appears only once at line 28 of page5. **Will be defined.** It is the name of a subroutine of HYDROTEL
- TIGGE: THORPEX Interactive Grand Global Ensemble. THORPEX is itself an acronym, which stands for "The Observing system Research and Predictability Experiment". Appears for the first time at page 6, line 31. The acronym TIGGE is already defined there, but not THORPEX. **The definition of THORPEX will be added.**
- ECMWF: European Center for Medium Range Weather Forecasting. Appears for the first time at line 32 of page 7 and is already defined there.
- MSC: Meteorological Service of Canada. Appears for the first time at line 33 of page 6 and is already defined there.
- EnKF: Ensemble Kalman Filter. Appears for the first time at line 21 of page 7 and is already definied there.

- USACE: United States Army Corps of Engineers. Appears only at line 30 of page 10 and in the list of reference. **It will be modified so that the full name appears instead of the acronym, both in the text and in the list of references**.

- **p. 2, line 7/8: What does this mean? Could you provide examples?** According to a comment by Reviewer 1, all references to analog forecasting systems have been removed from the revised version of the manuscript.

- **In general, a more structured review of the literature on uncertainty is missing. For example, different types of uncertainty (epistemic versus aleatoty/natural uncertainty) could be distinguished since they may have different effects on decisions and decision makers because epistemic uncertainty can be reduced by better data or models while aleatory uncertainty cannot. Later in the paper, this should also be discussed in the context of the study.**

  Identifying the different types of uncertainty can certainly help guiding the improvement of forecasts. This indeed has been the focus of many papers (e.g. Juston et al., 2013; Beven, 2016). The later reference includes a section about the level of *confidence* that one can have in the forecasts and how this level of confidence (potentially affected by disinformation and uncertainty) can impact decision-making. This is briefly discussed on page 17, line 30 of the current version of the manuscript. In the present study, which is only the first step toward a more realistic assessment of the value of forecasts, we assume that "the decision maker's trust of the forecasting system is absolute" (p.17 line 31-32). Considering the level of confidence of decision-maker has toward the forecasting system, and how this level of confidence can vary according to his/her ongoing experience with the forecasts, requires further significant modifications of the decision model. As mentioned on p. 17 line 32-33, this will be the object of future work.

Now, under this assumption that "the decision maker's trust of the forecasting system is absolute", the identification and reduction of different sources of uncertainty is somewhat distinct from the decision maker's problem. For a given forecast, the decision maker's spending decision does not depend on the *type* of uncertainty. The decision maker takes the forecast as given, and does not directly take part in its elaboration. This is also linked to Reviewer 1's comment C (Page 8, line 16 of the original manuscript) regarding the "cost of the forecasting system". Any decision that may affect the quality of the forecasts (implementation of new forecasts, reduction of uncertainty...) has been taken before the decision maker has to decide how much to spend. At this point, he or she takes the forecast as such and makes the best possible decision given the information available at that time.

Given your comment, as well as Reviewer 1's, we will adjust the discussion in order to avoid such confusion. We will also include the above mentioned references regarding the types of uncertainty in the revised version of the manuscript.

- **The von Neumann and Morgenstern utility function should already be briefly explained in the introduction (p. 2, line 31/32)** The vNM utility functions will be intuitively described in the introduction of the revised version of the manuscript. We will also add many details in section 2. See our detailed answer to comment A from Reviewer 1.

- **p. 3, line 2: delete "forecast" once.**: Will do. Thank you.

Section 2

- **If you use a section 2.1 there should also be a section 2.2. One subheading does not make sence. Consider to delete the headline.** The headline of section 2.1 will be deleted according to your suggestion.

- **The economic model and the utility functions should be better explained. The content of the chapter referenced in line 30 (p. 3) should be briefly summarized.** Utility theory and the economic model will indeed be better explained in the revised version of the manuscript. Please see our answer to Comment A by Reviewer 1. He/she also had many questions and comments regarding those topics and asked for additional references.

- **A paragraph that bridges this section to the next should be added** A paragraph (or rather a sentence) that bridges this section to the next will be added according to your suggestion.

- **Starting on p. 4: Check the numbering of the equations; add numbers to all equations on p.4,9 and 12.** All equations will be numbered in the revised version of the manuscript.

Section 3

- **Typo in line 20 (p. 4)** Will be corrected, thank you.

- **p. 4, line 28/29: consider rephrasing, check logic** Those lines currently read: "The response time of the watershed is rapid (12 hours). The return period of damaging floods is also short. This makes emergency evacuation and flood damage a common occurrence for riverside". We would like more precisions on what to clarify. First sentence means that floods appear rapidly. Second sentence means that floods happen often. This is why it is important to have flood forecasts and an emergency plan for this particular watershed.

- **In Table 1, the potential damage should be added fpr each return period.** According to our answer to Reviewer 1's comment B (second item), the revised

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version of the manuscript will include histograms reporting the number of events observed in each category of damages, for each forecasting system. We believe that this information will be more in line with the general framework of the paper. In addition, we are worried that displaying the values derived from the flow-damage curve provided in Leclerc et al. (2000), which would be gross approximations, could lead the reader to put too much confidence in those estimates.

Note also that Leclerc et al. (2000)'s report (though in French) is freely accessible on the Internet.

- **p. 5, line 8/9 consider rephrasing ("cause" is used twice in this short sentence)** The sentence will be rephrased to read: "an early spring thaw caused by extreme temperatures **induced** a flood resulting in the evacuation of 25 households."

- **This is unclear. The calibration performed by the DEH should be explained (as well as the meaning of DEH - see my comment on the use of abbreviations)** The meaning of "DEH" is already defined at the first use of this acronym (please see our answer to your comments about the introduction). The DEH is a section of our provincial government (province of Quebec) that is responsible for hydrology and hydraulics (all aspects: operational flood forecasting, data collection and dissemination, dam safety control, etc.).

  The calibration of model's parameters was performed using the Shuffle Complex Evolution algorithm of the University of Arizona (SCE-UA, Duan et al., 1994). The objective function to maximize is the Nash-Sutcliffe Efficiency criterion. We propose to mention briefly SCE-UA, Nash-Sutcliffe efficiency criterion and global calibration strategy in the revised version of the manuscript. We assume SCE-UA and the Nash-Sutcliffe efficiency to be well understood by most readers.

- **Again, there shouldn't be a section 3.3.1 only. Please reorganize the text.**

We agree and section 3.3.1 will be removed. The content will simply be merged with 3.3.

Section 4

- **p. 8, line 14: The use of 12 categories should be justified or better explained.** First, we wanted to separate streamflow values into different categories because the result of a 450 m3/s flood shouldn't be much different than for a 460 m3/s flood, especially since those values are subject to many uncertainties. Second, regarding this precise choice of **12** categories, it is based on a previous hydraulic study of the sector to establish inundation maps (Leclerc and Secretan, 2012). They produced 11 maps, for streamflow varying from 550 to 1050 m3/s and separated by an increment of 50 m3/s. We adopted this increment of 50 m3/s, but included lower streamflow values. We will add this explanation in the revised version of the manuscript.
- **p. 8, line 19-21: The content and use of the data for the 2014 flood is unclear. Please add some information.** Indeed, we will add more information. Specifically, for 2014, we know the streamflow value (and therefore the associated damage), as well as the amount spent. This allows us to calibrate β_w. This is also explained in more details in section 4.2 (p.9, lines 13-16). Unfortunately, if we can share the value of the streamflow (825 m3/s), our confidentiality agreement with the civil security prevent us to communicate the amount spent.
- **p.9, line 4-6: The basis/source of the mentioned losses is unclear. Please explain how these values were derived. In line 27, a damage curve of Leclerc et al. (2001) is mentioned. This comes too late and too vague. Explain how the curve looks like, whether it is applicable in the catchment under study or/and whether and how is was adpated to your case study.**

Indeed, the damages are taken directly from Leclerc et al. (2001). It is based on a survey regarding the types of houses in the sector (1-2 stories, with/without basement...) and their value obtained from municipal evaluation. Level of submersion for different streamflow values are obtained through hydraulic simulation. Damage is deduced from this level of submersion using Gompertz law Gompertz (1825). We will add these informations in the revised version of the manuscript, but as mentioned above, we choose not to replicate the curve since we do not want to put too much emphasis on its precise values. Since it is available online, the interested reader can also easily access it.

- **p.9, line 4 and line 10: consider using "losses" instead of "damages"** We choose to use “damage” instead of “loss” in order to distinguish from the usual use of the term “loss”, as in “cost-loss ratio”. As described in Appendix A, under risk aversion the two are not necessarily equivalent. We prefer using “damage” representing the actual, incurred, damages.
- **p. 10, line 3 to 15: Most of this should be shifted to the dicsussion section.** We agree, we will move this part to the discussion section.
- **In general, the section 4.3 is somewhat unclear and contains too many issues for discussion. Consider to shorten it to the main point that are necessary for the model application.** This is related to your previous comment. We agree that some items should be moved to the discussion. It will be done in the revised version of the manuscript.

Section 5

- **p. 12, line 3: discuss how the true distribution of streamflow could be determined or whether it is possible to check the validity of the used distri-**

bution. This comment is similar to Reviewer 1's comment B (second paragraph of the section, page C3 of his/her review). It is actually not possible to determine the true distribution of streamflow. One can only approach it by using the available historical record. It is expected that a longer record will provide a better empirical estimate of the true streamflow distribution. However, there can also be various sources of non-stationarity affecting the observed streamflow values over time (e.g. changing the measurement apparatus, climate change, land-use change, etc) that can contradict the previous sentence. Regarding the "validity of the used distribution", in our study, we did not fit any parametric distribution. As for the validity of the available empirical distribution, to the best of our knowledge, no, there is no way that its validity could be verified with certainty. We will modify the sentence "Note that the history under consideration must... distribution of streamflow" for "Note that, *strictly speaking*, the history under consideration *should*... distribution of streamflow". Then we will add a brief discussion about (1) non-stationarity effects/limits in the availability of data and (2) the impossibility to compare with the "true" distribution of streamflow.

Section 6

- **p.13, line 22: What do you mean by "sharpness"? Accuracy?** Sharpness is a desired attribute of ensemble and probabilistic forecasts (e.g. Gneiting and Raftery, 2007) and does not correspond to accuracy. In a sharp forecast, all members are very close to one another. They are not necessarily accurate, though, as they could all be wrong. Deterministic forecasts are, by definition, infinitely sharp (as a Dirac function). We will add a precision on that point in the revised version of the manuscript.
- **What do you refer to when you mention "relatively rare and comparatively**

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small flood events"? we mean that the "usual" flood events for the Montmorency River are much less dramatic than the predicted ones (looking at the upper tail of the predictive distribution).

Then, for a relatively risk-averse decision maker, but small level of immaterial damages (say $A = 0.005$ and $\psi \leq 10$), having no forecast is better than having ensemble forecasts (see Figures 8 and 9). This happens because the ensemble forecasts predict huge streamflow value, which are never realized. However, those dramatic predictions lead the decision maker to spend immense amounts of money.

You are right that our statement was imprecise. We will add more details in the revised version of the manuscript.

Section 8

- **p.17, line 19: typo "AND in terms..."** Thank you for pointing the typo on p. 17 line 19. It has been corrected in the revised version of the manuscript.
- **Figure 3, 4 and 10: Explain the abbreviations in the figure caption** We would really prefer to leave the abbreviation in the figures for two reasons (1) The acronyms (EnKF and CRPS) are already defined in the text, before the figures (please see our answer to your comments about the introduction) and (2) The use of acronyms in figure titles allows for those titles to remain relatively short, which in our opinion is better for ease of reading.

References

Beven, K.: Facets of uncertainty: epistemic uncertainty, non- stationarity, likelihood, hypothesis testing, and communication, Hydrological Sciences Journal, 61, 1652–1665, 2016.

Duan, Q., Sorroshian, S., and Gupta, V.: Optimal use of the SCE-UA global optimization method for calibrating watershed models, Journal of Hydrology, 158, 265–284, 1994.

Gneiting, T. and Raftery, A.: Strictly Proper Scoring Rules, Prediction, and Estimation, Journal of the American Statistical Association, 102, 359–378, 2007.

Gompertz, B.: On the Nature of the Function Expressive of the Law of Human Mortality, and on a New Mode of Determining the Value of Life Contingencies, Philosophical Transactions of the Royal Society of London, 115, 513–583, 1825.

Juston, J., Kauffeldt, A., Montano, B., Seibert, J., Beven, K., and Westerberg, I.: Smiling in the rain: Seven reasons to be positive about uncertainty in hydrological modelling, Hydrological Processes, 27, 1117–1122, 2013.

Leclerc, M. and Secretan, Y.: Reconstruction de la prise d'eau de l'Arrondissement Charlesbourg – Simulation hydrodynamique du secteur Canteloup, des Îlets, Trois-Saults de la rivière Montmorency, Tech. Rep. R1416, INRS-Eau and Laval University, Quebec, 2012.

Leclerc, M., Heniche, M., Secretan, Y., and Ouarda, T.: Travaux d'atténuation des risques de crue à l'eau libre de la rivière Montmorency dans le secteur des îlets – PHASE 2. Mise à jour de l'analyse hydrologique, idmensionnement des travuax d'atténuation et analyse de l'impact sur les risques résiduels de dommage aux résidences., Tech. Rep. R555, INRS-Eau, Quebec, 2000.