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Discussions

Interactive comment on "Examining the impacts of estimated precipitation isotope (δ^{18}O) inputs on distributed tracer-aided hydrological modelling" *by* Carly J. Delavau et al.

Anonymous Referee #1

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Review of hess-2016-539: Examining the impacts of estimated precipitation isotope (delta oxygen-18) inputs on distributed tracer-aided hydrological modelling by Delavau et al.

General comments: The authors present an interesting case study about using differing precipitation stable isotope input datasets for distributed hydrological modeling in the Northwest Territories of Canada. Based on three different precipitation stable isotope datasets, three different calibrations of the model isoWATFLOOD were identified based on a Monte Carlo random sampling approach. The results show that modeled streamflow was relatively similar for each of the three used stable isotope input datasets. Whereas the differences in the modeled stable isotope signature in streamflow and the

internal apportionment were much more pronounced. However, the study is lacking some important and critical explanation of the presented model outputs. Please find a detailed description of specific comments and technical notes below. The focus of the presented study is in the scope of HESS. Furthermore I will highlight at this point that the paper is very well written, understandable and the sections are mostly very well structured. However, based on my review below I recommend major revisions prior to a publication in HESS.

Specific comments: The authors should rethink the use of the word "estimated" in the title as well as throughout the whole manuscript. It suggests that the input data was generated specifically for the presented study. It should be clear that (2 of 3) available precipitation isotope product were used to the study. Which is actually an asset for the study and with respect to future studies in other basins.

The first sentence of the abstract is "…increasingly popular tools as they have documented utility in constraining model parameter space during calibration, reducing model uncertainty, and assisting with selection of appropriate model structures.". However, there is no evidence for that statement. Please include additional information to the introduction section or revise the first sentence of the abstract.

The authors highlight the importance of snowmelt in the study region. The stable isotope signature of the snow pack and its melt water is a very challenging topic. Please handle this point very carefully in your publication. On page 5, Line 17 for example you mention that the default method for oxygen-18 input is annual average rainfall and snowfall. In your static approach, however, you used average measurements of rainfall and snowpack from the GEWEX campaign. Please provide the values of snow pack stable isotope signature in figure 5 by the way. Especially during the ablation season the isotopic evolution of the snowpack progresses due to percolating rain water and fractionation caused by processes like melting and sublimation (Zhou et al., 2008; Unnikrishna et al., 2002; Dietermann and Weiler, 2013; Lee et al., 2010). This leads to an increase of heavy isotopes in melt water throughout the freshet period (Taylor et al., 2001,2002; Unnikrishna et al., 2002). Which is correctly represented by the shown model results. Taylor et al. (2001 and 2002) point out that for hydrological applications (in their case isotope based hydrograph separation) a correct representation oft he snow pack melt water is absolutely crucial.

REMOiso is a distributed dataset and the precipitation amounts are also available spatially distributed over the study area. Why was the precipitation amount weighting only conducted at one location and not spatially distributed?

The authors mention that "several changes and improvements" (Page 7, Line 16) were carried out in the model version used for the study. In the following only one modification (proportion of bog an fen split) is mentioned. Are there any other modifications? If so, please mention them here.

The first two paragraphs of section 4 (Results and discussion) should definitively be revised. There is a lot of content that can be mentioned later in the conclusions section (the last sentence on Line 12-14 for example).

In section 4.2 (Modelling streamflow) please explain the model results as well as the observed streamflow in much more detail. The three different inputs (and three different calibrations) provide very similar results for the simulated streamflow (Page 15, Lines 5-8). Those results should be discussed in more detail. Is there really no discharge in winter (Figure 2 and 3)? What are the influences of groundwater on the hydrology of the region? The same holds for section 4.3. Explain the results in more details. There is especially the time of the spring freshet that needs much more carefully discussed. The model results show a sharp drop of streamflow stable isotope signature, while the observed values are getting more and more enriched at that time. This completely opposed development may be related that the contribution of snowmelt water to total streamflow during that time is too high (or the signature of the snow melt signal is wrong, please remind here my suggestions above) and the contribution of baseflow too low. The contributions of baseflow (groundwater) to total streamflow during the

post-freshet are especially for Jean Marie River much lower in the present model study compared to the results of St Amour et al. (2005). Furthermore, please provide the stable isotope signature of groundwater. And compare those observed values with the values generated by the models in the groundwater routine after the spin-up period.

Please check the manuscript for repetitive information. The sentence on Page 13, 34+35 for example appears almost identical on the next page again (Page 14, Lines 20-22). This would be an excellent take-home sentence for the conclusions section by the way.

In general, I am missing some distinct conclusions in the conclusions section of the submitted manuscript. There are a lot of recommendations and speculations but no clear take-home messages.

Technical notes:

Page 1, Line 18: "...to capture both the variability and seasonality". There it would be better to write "spatial variability and seasonality" or "spatial and temporal variability", since the seasonality is also a variability (temporal).

Page 1, Line 31: (e.g. Beven and Binley, 1992; Kirchner....)

Page 3, Line 22 and Line 29: Please provide size and elevation characteristics of the basins here.

Page 3, Line 27: "...is selected based ON data availability."

Page 4, Line 20: The study region is not a high elevation region. Please mention correctly why the approach is suitable for the study region.

Page 4, Line 21: "...is used TO spatially..."

Page 5, Line 9: Why are they not adequate for model forcing? This is the input data used and referred STATIC in the study, right? Please revise this sentence

Page 5, Line 12: “such that” appears twice.

Page 5, Line 24: KP43 instead of KPN43.

Page 6, Line 4: From my point of view the section 2.4.1 is a description of methods and should therefore be moved in the appropriate section.

Page 6, Line 16: Please mention that Snare Rapids is a CNIP station for clarity.

Page 6, Line 7: IAEA (2014) this citation is listed in the references section as IAEA/WMO (2014). Please adapt.

Page 7, Line 16: based on instead of based off.

Page 8, Line 5: The authors should reconsider the terms “bahavioural” and “non-behavioural” for the model outputs of streamflow and stable isotope signature of streamflow. From my point of view those terms are not appropriate in this context. Reliable and non-reliable are terms coming to my mind here.

Page 8, Line 13: KGE. This abbreviation is introduced later (Line 23). Would be nice to have the explanation earlier.

Page 8, Line 30: Please mention for completeness that the other circa 52 % were sampled during the summer months.

Page 10, Line 11-18: Please explain clearer that you are talking about the average streamflow simulations of the three calibrations used in this paragraph. The reader will otherwise think you are talking about an average streamflow simulation (Line 12) of all model runs. Furthermore please precise which model you are talking about at the end of line 12 and beginning of line 13 (“The model also has…”).

Page 10, Line 13: difficulty instead of difficultly

Page 10, Line 20-27: Please explain shortly why you have compared REMOiso vs. static and KNP43 vs. static for calculating the Kendall’s tau coefficient.

Page 10, Line 29: Please revise the title of section 4.3 to Modelling delta oxygen-18 in streamflow).

Page 11, Line 14-16: Please check the literature and provide a reference here.

Page 11, Line 16: functions or function(s)?

Page 11, Line 21+22: Please provide some values (and percentages related to total annual precipitation) from mean annual precipitation for the mentioned periods (summer and fall, winter and spring).

Page 14, Lines 29-31: This sentence is a bit confusing. Please revise.

Page 14, Line 31: isoWATFLOOD or WATFLOOD?

Page 15, Line 4: isoWATFLOOD or WATFLOOD?

Page 15, Line 10: isoWATFLOOD or WATFLOOD?

Page 16, Line 13: kpn or KPN43?

Please check the citations carefully. Pietroniro et al. (1996) and Töyra et al. (1997) are listed in the references section (Page 18, Line 46 and Page 19, Line 34) but appear not in the manuscript itself.

In general, I liked the style and the coloring of the figures. However, figure 2 and 3 are a bit unclear. It is a real asset to show the uncertainty bounds of the different calibrations. The authors should rethink the presentation of this data, especially the streamflow results (panel b). Furthermore I would suggest indicating periods with snowfall and rainfall, if possible. At this point it would also make sense to combine the two times-series (static-rainfall and static-snowfall) to one static-precipitation input time-serie.

Figure 6: Are here shown the mean or median values (circle symbols)?

Figure 7: Please refer to Table 6 (were the parameters are explained) in the figure caption. The order of the table numbering in the text is sometimes were confused

(Page 4, Line 4: Table 1; Page 4, Line 32: Table 4, for example). Please order them correctly.

In general, I suggest reducing the amount of tables. Table 3 for example is not needed. The applied average correction values (and the range) can be mentioned in the text.

Table 5 is also unnecessary. You can mention the values in the text. However, it would be very relevant to explain in more detail how these values were selected.

Table 8 is also unnecessary from my point of view.

References: Dietermann, N. and Weiler, M.: Spatial distribution of stable water isotopes in alpine snow cover, Hydrology and Earth System Sciences, 17, 2657-2668, doi:10.5194/hess-17-2657-2013, 2013.

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