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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.

Carly J. Delavau et al.

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We sincerely thank both referees for their thorough reviews and most constructive comments on our manuscript (Reference HESS-2016-539). We fully recognize and appreciate the reviewers' efforts in providing these informative reports on our research and their insights have led to an improved interpretation of our results. We have therefore taken into full consideration all of these comments and have prepared responses to these as well as information on how the paper was revised following the referees' suggestions. Our responses and edits to the paper are provided below in bold following the individual comments requiring action from reviewer 2, Dr. Christian Birkel.

Please do not hesitate to contact us if any of this information is not clear.

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Discussion paper

With kind regards,
Tricia Stadnyk (on behalf of all co-authors)

Referee 2 The manuscript “Examining the impacts of estimated precipitation isotope (18O) inputs on distributed tracer-aided hydrological modelling, Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-539," by Delavau et al. currently under discussion in HESS highlights the importance of the input function and temporal resolution on tracer-aided modelling particularly in remote and data scarce catchments. The evaluation of large scale, spatially-distributed and climate model based isotope products as an alternative or complementary method to ground-based measurements could potentially become a feasible and widely used approach for tracer studies in areas with difficult access and monitoring constraints. I consider this as a novel contribution to the existing literature. The paper is well-written and logically structured. It clearly demonstrates the impact of different isotope input functions on the coupled model and how this analysis contributes to constraining the model uncertainty particularly the internal functioning and how the model generates flows, mixing and the simulated water partitioning. Having said that, I think that the paper could be edited towards more clearly conveying the key points in terms of more generalizable results going beyond the Canadian context and the presented isoWATFLOOD model. I will detail my suggestions further below. Nevertheless, I am convinced that this paper will likely attract a lot of attention across a wide range of readers and beyond the hydrology community.
Thank you kindly for your summary and assessment of our paper, Dr. Birkel, and we agree that we are excited about the implications this manuscript and its comparison of isotope precipitation products may have on the isotope-enabled modelling world. We believe the changes you’ve suggested have greatly improved the quality of this manuscript.

Specific comments: My main point would be that the paper is in parts very much focussed on the particularities of the study site and also the presented model characteristics. However, the results and potential impact of this paper go in my opinion beyond

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this case study and this could be better emphasized to maximize impact particularly in the hydrological modeller community. I therefore, suggest the following:
We also agree that the findings presented in this manuscript go beyond our specific application to the Fort Simpson region and are therefore more general and impactful than we have conveyed them. We have edited the manuscript in a way that conveys our findings in a more general sense, specifically with respect to a range of study sites (particularly those that have seasonality as this one), isotope-enabled models, and modelling applications. Thank you for this feedback.

- Title and Abstract: You could consider substituting the term “estimated” with e.g. “precipitation isotope product” throughout the manuscript to emphasize the different origins of the input functions.
We like this terminology and have adopted it for the revised title *Examining the impacts of precipitation isotope products (δ^{18}O) on distributed tracer-aided hydrological modelling*, as well as throughout the paper. Thank you for the suggestion!

From Line 17 in the abstract, I suggest to revise these sentences, as they do not really reflect the key findings. For example, the statement that the model is only as good as its input function is rather trivial and could be changed to some more specific statement such as which temporal resolution is needed (hourly, daily, weekly. . .) to adequately simulate stream isotope signatures and which product is the best?
Thank you for this suggestion, and we also agree. We have reworded the abstract to instead state “We investigate the impact that choice of precipitation isotope product (δ^{18}O$_{ppt}$) has on model simulations of streamflow, d18O of streamflow, and model parameterization in high-latitude, highly seasonal regions. We assess three precipitation isotope products (i.e., one new, two from the literature) of different spatial and temporal resolutions, and apply them as forcing to the isoWATFLOOD tracer-aided hydrological model in the Fort Simpson, NWT basin.” And

perhaps more importantly, we have revised our discussion and conclusions to comment specifically on the impact that precipitation isotope product resolution has on model output. This has become one of our key take-home messages.

I also suggest to more specifically mention that the coupled simulation of flow and isotopes actually allowed you to constrain the simulations towards a better internal representation of the dominating processes.
We agree and have revised the last sentence in our abstract to state: *Furthermore, the application of a tracer-aided model constrained simulations to achieve a better internal representation of watershed processes, reinforcing that a tracer-aided modelling approach assists with resolving hydrograph component contributions, and works towards diagnosing model equifinality.*

- 2.2, Line 21:. . .is used "to" spatially distribute. . .
Corrected, thank you.

- Page 7, Line 16:. . .based "on"?
Corrected.

- Page 9, Line 14: Would it be feasible to test this for one model configuration and run it over let's say 100K iterations to be able to check for differences compared to 30K runs?
Feasible, absolutely. In the time we have for edits to be submitted for this manuscript – no (we estimate it would take minimum 1 month, perhaps longer). That said, we are in the process of doing 100k runs with (iso)WATFLOOD in another northern basin to look at parameter identifiability with and without the use of isotopes in model calibration and nearing the end of those runs. We are planning to submit this manuscript for peer review within the next couple of months, where we will more definitively tackle the issue of parameter identifiability. Though we think this is a critical issue, it is not the intended focus of this manuscript, but rather follow up work that we now (more clearly) describe in the

new *Future Directions* section of this manuscript.

- Results and discussion: The results could be better linked to the wider literature. E.g. why not include the mean monthly precipitation isoscapes from Bowen and Revenaugh (2003) as a means of evaluation?
This is an interesting suggestion, however, this would only further evaluate KPN43 and REMOiso products and not δ^{18}O$_{sf}$ or other types of simulation output that are our intended focus. Bowen and Revenaugh's 2003 isoscapes are derived from long term average global models that did not include any CNIP data within their formulation, so we aren't convinced this would be a good dataset from which to further validate our REMOiso or KPN43 estimates of δ^{18}O$_{ppt}$ over the Fort Simpson region. It should be pointed out that the KPN models have already been evaluated rigorously in Delavau et al., 2015. REMOiso could definitely use more validation in Canada, but that was already mentioned, we're not confident that the Bowen Revenaugh 2003 isoscape would help with this. We have listed this instead as future work, and it is not currently within the scope of this study to evaluate REMOiso outside of the Fort Simpson study area. That being said, we believe that a comparison (to Bowen Revenaugh's isoscape) may show that KPN43 is a better estimate of δ^{18}O$_{ppt}$ in Canada than the other global models. On a cautionary note, however, this would be comparing oranges to apples because the time scale of the isoscapes would not be the same. And finally.... the static values were derived from actual observations, so there is no need to make a comparison to Bowen there.

I am missing a more concise attempt to generalize the results concerning model uncertainty and the value of tracer data in hydrological modelling.
We agree and have revised the discussion section of the manuscript – and conclusions – extensively to help draw these generalized results into take-home conclusions for the broader tracer-aided modelling community.

- Page 10, Line 1: How is the static approach with a single annual isotope value able

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Discussion paper

to capture seasonal variability?

So the static approach is actually two annual isotope values: one for rainfall and one for snowfall. Therefore, technically speaking, the static approach is capable of capturing some seasonality. This is a point we have much more clearly (and in more detail) described in the manuscript. The fact that the static input captures "sufficient seasonality" is likely more a function of our high-latitude study site than the value of a static input alone. Namely, in high-latitude environments, particularly Fort Simpson, there is no mid-winter freeze/thaw/melt – resulting in snowpack accumulation throughout the entire winter season and one significant freshet in late spring. Similarly, soils freeze up as does any soil moisture that may in other regions contribute to baseflow and/or streamflow throughout the winter. In high-latitude regions, seasonality is more binary than quarterly, therefore the two annual static inputs do a reasonable job of capturing the seasonality.

- Conclusions and recommendations: I suggest to summarize the key points and present them in a numbered order. I also think it would be better to present the outlook as a separate section.

We have taken your suggestion to mean a numbered summary of the key take-home messages, which we have better aligned with the objectives and numbered accordingly in the conclusions section. With regards to "outlook", we assumed you mean future work to be done with the modelling, and have added a "Future Directions" section to this manuscript.

- Would it be possible to include gridded maps of the different mean annual (and seasonal min/max) isotope products over the study area in relation to the observed data for comparison purposes?

Thank you for this suggestion. Though we don't feel another figure is warranted in the manuscript, we see the value in these figures and the presentation of our precipitation isotope products for the modelling community and have decided to add it as a supplement to our manuscript (Figure S-1). To generate the spatially

distributed precipitation isotope products maps, daily isotope in precipitation input used to drive the distributed tracer-aided model was averaged daily across each season (DJF, MAM, JJA, SON) for each source (static, REMOiso, KPN43). Maps were generated using the model grid (10k) and entire modelling domain (includes both Jean-Marie and Blackstone), and isotope compositions were flux-weighted using daily distributed (10 k) precipitation input to WATFLOOD (interpolated Environment Canada station observation, housed in WATFLOODs radcl .r2c files; Kouwen 2014). The resultant maps indicate clear differences in spatial variability among the inputs. Static – not surprisingly – is spatially constant (as it should be!), but seasonally variant resulting from the mixture of rain and snowfall events on the shoulder seasons (MAM and SON). REMOiso has less variability than the KPN43 input, resulting from REMOiso's 55 km grid resolution (i.e., approx.. 5 of the isoWATFLOOD grids shown on our Figure) which would act to smooth topographical and land cover differences in part driving changes in isotopic composition. We've added a brief discussion to the paper and reference to Figure S-1. For your interest and review – we also generated a figure (not included in the manuscript) averaged across the entire study period (1997-1999) for each model input (Figure 1). This confirms the enhanced spatial variability from the KPN43 model, followed by REMOiso (derived from a 55km RCM), and the spatially constant Static input. Because of the high-latitude of the study region, the static input shows that snowfall prevails over rainfall for this site (in terms of isotopic composition), and that the 3-year annual average is more depleted than the temporally (and spatially) variable inputs. KPN43 variability is enhanced in the 3 year average because it is more consistent from grid-to-grid in each year (driven by the KPN43 regionalization) than REMOiso, which would vary temporally and spatially daily and from year to year. We could not generate an observed isotope in precipitation map because we did not have enough observed data to so.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-539, 2016.

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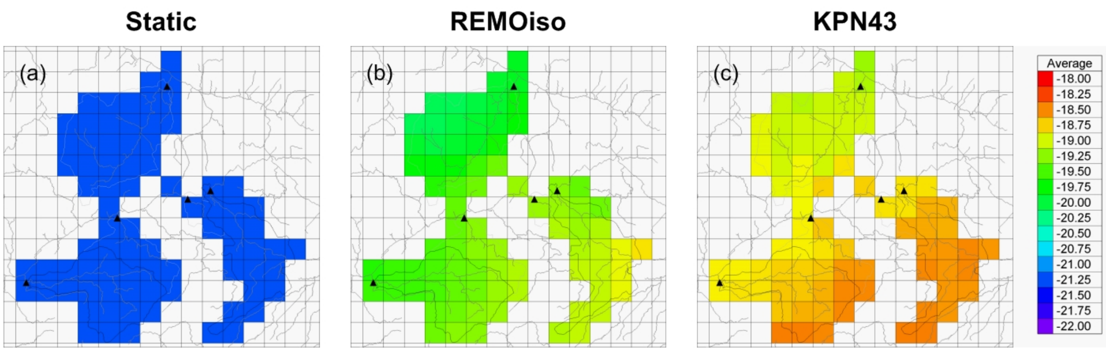

Fig. 1. Spatial distribution of precipitation isotope products averaged across the entire study period (1997-1999)

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