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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Using isotopes to constrain water flux and age estimates in snow-influenced catchments using the STARR (Spatially distributed Tracer-Aided Rainfall-Runoff) model" *by* Pertti Ala-aho et al.

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GENERAL COMMENTS

The manuscript presents an innovative approach to examine the residence time of water in catchments by using a numerical model to simulate the flow and isotopic composition of streams draining small catchments. The model has a relatively simple construction but it captures the integrated effects of spatially distributed sources and reservoirs of water and provides a useful tool for improving our understanding of catchment hydrological processes. This is a nice piece of work and warrants publi-

cation in this journal. However, I have noted a few issues that need to be addressed before the manuscript is considered for publication. Please see my specific comments below.

SPECIFIC COMMENTS

P3, L25-28. I am not sure if these sentences (in Latin?) are meant to be here.

P5, L21-23. Were event-by-event precipitation samples available for both snow and rain? It is straight forward to collect rain samples, but I am not sure how snow samples were collected. Please explain.

P5, L26. Please spell out DCEW at its first appearance.

P5, L33. What is the elevation of Svartberg meteorological station in relation to the catchment outlet?

P6, L1. Where are these meteorological stations? Can you show them in Fig. 1?

P6, L2. How far is this station? What is the elevation?

P6, L6. What is the elevation of the SNOTEL station?

P6, L8. How were these lapse rates determined? These rates may vary between summer and winter. What is the justification for using constant values?

P6, L12. Where was SWE measured? Can you indicate the location in Fig. 1?

P7, L9. What is the difference between "snow storage" and "ground snowpack"?

P7, L10. This equation assumes instantaneous mixing of snow within the snowpack. This may be a reasonable model for a thin snowpack, but its validity is questionable for a thick snowpack typical of mountainous catchment (see SWE graph in Fig. 7). Fractionation associated with sublimation and evaporation occurs from snow near the surface, which is not easily mixed with the rest of the snowpack. Similarly, snowmelt fractionation occurs near the surface, not from the entire snowpack. I see this as

a major deficiency in the snow isotope module of this model. Its effects on model performance need to be discussed more openly and carefully. It is highly desirable to add isotope data for snowpack or snowmelt percolation collected by snow lysimeter (P8, L3) to validate the assumption.

P7, L22. It appears that the model uses a constant value for evaporation fractionation factor, whereas it is expected to vary with meteorological conditions such as relative humidity and wind speed. Please present justification for using a constant value.

P7, L20. Snow loss to the atmosphere occurs by two different processes depending on snow surface temperature; sublimation under 0 C, and evaporation of melt water at 0 C. Resulting isotopic fractionation factors may be different. This is a subtle point, but should be discussed.

P7, L26. What is the reasoning for dividing M_frac by d_melt? Is this purely empirical or does it have a theoretical basis?

P8, L1. I am generally against citing unaccepted "in review" manuscript because there is no guarantee that the manuscript will be published. Please avoid using the in review manuscript, or at least include it in the reference list with the journal name.

P8, L3-4. What kind of algorithm is used for the new snow module? Is it still a degree-day model? I note that radiation is included in the data set (P5, L32)? Please include a brief explanation of the model. Note that in a catchment with rugged topography such as Bogus, slope aspect and angle may have a strong influence on the spatial distribution of snow accumulation and melt.

P8, L6. This is another questionable assumption. Snow accumulates from the bottom to the top during winter without much mixing. Snow melt and evaporation occurs from the top. Therefore, it is questionable to assume complete mixing for water age. Please point this out in texts and discuss potential errors resulting from this assumption.

P8, L8-10. I do not quite understand this sentence. How is FC defined? By calibration?

It is not listed in Table 1.

P8, L11. The need for adaptation became apparent. How?

P8, L13. Depending on the soil thickness, root density, and other complex factors, it is unlikely that evaporation age is equal to average soil water age. Please discuss this issue carefully.

P8, L16. There appear to be 13 parameters listed in Table 1. If three values are used for each of the 13 parameters, there are roughly 1.6 million combinations (3^13) of parameter values. How were the 10,000 combinations selected? Please explain.

P8, L20. Were empirical coefficients used in the snow module (e.g. coefficient for degree-day model)? If so, how were they calibrated? Please explain.

P8, L25. Is this field capacity the same FC as the one described in L8-10 above?

P9, L17. The standard metric for stream flow calibration is Nash-Sutcliff efficiency (NSE), which is familiar to most of the readers of this journal. I do not see a strong justification for using Kling-Gupta efficiency (KGE). I suggest NSE be used instead of KGE. If not, please present a stronger justification for using KGE and include its definition (e.g. equation).

P9, L28 – P10, L3. I read this section several times, and still could not understand the definition of F ( ) and how it was constructed. Please re-write the section more clearly.

P10, L6-9. This section was also very difficult to follow. Please re-write.

P10, L30-31. The model is calibrated for stream isotopic composition, but it is not clear how well the isotopic compositions in groundwater and soil water are simulated. Laudon et al. (2013) describes systematic sampling programs for soil water and groundwater at Krycklan catchment. Please include a comparison of simulated and observed soil water and groundwater data, where available.

P15, L2. It is true that STARR has a spatially distributed model structure, but it uses

uniform values of model parameters for an entire catchment. In reality, the spatial distribution of material properties (e.g. soil type and thickness, bedrock type, vegetation) has an important effect on catchment hydrological responses. That is why many models use hydrological response unit (HRU) approaches to capture catchment heterogeneity. I suggest that the authors discuss the lack of heterogeneity in the current configuration of STARR and its potential implication for model performance and estimated water age.

P16, L10. High sensitivity of E_frac in Fig. 10. This is a bit misleading because the highest sensitivity is observed for Bogus catchment, which had few data points. Fig. 10 indicates low sensitivity for E_frac for Bruntland catchment. If the authors want to showcase the snow isotope model as one of the highlights of this work, then this topic needs to be explored a bit more carefully. Please note my comments on the isotopic fractionation in P7, L10.

P17, L8. I do not think isotope data from Bogus catchment had exceptionally high quality. Please clarify.

P17, L11. I do not think the need to incorporate isotopic evaporative fractionation is convincingly demonstrated. Please see my comment on Fig. 10 above.

Fig. 5. It is impossible to see the difference between red line and pink band in the top figure. Can you use different color combination (e.g. blue and pink) and use the same color for all of Fig. 5, 6, and 7? I do not see a real need for using different colors for different catchments.

Fig. 9. Please include a scale and a north arrow for each catchment.

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