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Geoscientific Model Development Discussions

Interactive comment on "EcH$_2$O-iso 1.0: Water isotopes and age tracking in a process-based, distributed ecohydrological model" *by* Sylvain Kuppel et al.

Anonymous Referee #2

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Kuppel et al. presents a physically-based ecohydrological model EcH2O-iso that can track water isotopic tracers (2H and 18O) and age. The EcH2O-iso is an extension of the EcH2O model (Maneta and Silverman, 2013). The EcH2O-iso model was evaluated at the Bruntland Burn catchment in the Scottish Highlands, and the simulation results show reasonable agreements with the isotopic measurements.

The paper is well written and structured, and it could be a potentially useful contribution to the literature. However, the authors used very general terms in many parts of their model evaluation, which makes it difficult to assess the reliability of their results. For example, no statistics were shown on any of the time series plots, so there is no way

that the readers can examine the model performance. Therefore, a major revision is suggested to improve the presentation of the current manuscript.

Specific comments:

Pg2, L9-12: The statement provided here seems not directly related to the paragraph above and below it. It is not clear what was the authors' attempt to deliver here. Also, what is "simplistic" meant by the authors with regard to the hydrology in land surface models?

Pg3, L28-31: It is not clear how these questions being addressed in the paper. It would be very helpful if the authors could add more details about the experimental design to illustrate how these questions were linked to the results.

Pg4, L4: It might be better to change "climate" to "microclimate" since the spatial and temporal scales used in the model is relatively small than the scales used in climate science.

Pg4, L11: What is the temperature threshold for the partitioning between liquid and snow components? How does the model quantify snowpack depth for a given amount of precipitating snow?

Pg4, L12: Canopy conductance is a key factor determining the amount of canopy transpiration. How is canopy conductance represented in the model? Is it simulated at each model time step?

Pg6, L3: Δt is redundant here as it has been defined right above eqn (2).

Pg8, L13: Could the authors provide any reference for the amount of PET estimated at the study site?

Pg11, L27-29: Were there any missing data during the measurement period? If so, what was the gap-filling treatment for the meteorological observations? Also, what was the temporal resolution of the meteorological observations?

Pg 12, L 13: How did the authors determine the transient dynamics has been removed after a 3-year spin up period?

Pg 12, L21: Why did the authors set the depth of the first soil layer to 0.001 m? How sensitive does the model respond to the changes in the depth of the first soil layer?

Pg 13, L11: It should be Eq. 20 instead of Eq. 19.

Pg 19, L 26-27: How is the seasonal change of vegetation represented in the model? Was the increase of ecosystem transpiration resulted from the increase of vegetation leaf area or the increase of canopy conductance? Did the authors check the water loss from canopy evaporation? How much of difference did the model simulate between canopy evaporation and soil evaporation?

Pg29, L23: Please change “T he” to The.

Pg30, L13-14: This is a very general statement. It would be very helpful if the authors could revise it with more specific terms so the readers can catch up easily.

Pg30, L14-15: Again, it is difficult for the readers to understand why this wound indicate the model is correct in both energy celerity and flow velocity viewpoints. It might be useful to explain what exactly are the energy celerity and flow velocity viewpoints meant by the authors.

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