Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-84-AC2, 2019

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Assessing the influence of soil freeze-thaw cycles on catchment water storage – flux – age interactions using a tracer-aided ecohydrological model" *by* Aaron A. Smith et al.

Aaron A. Smith et al.

smith@igb-berlin.de

Received and published: 29 May 2019

General Comments of Reviewer 2

The paper by Smith et al., seeks to use a previously developed ecohydrological model (EcH2O-iso) to further understand the partitioning, water storage, flux and age interactions, particularly in the context of cold, northern catchments. This novelty of this contribution is that they have adapted the model to include soil freezing, and the impact of soil freezing on water ages. As the authors note, most model estimations of

storage-flux interactions oversimplify vegetation-soil-water interactions, while EcH2O-iso provides a generic and relatively simplistic (in some parts) modeling approach to evaluate storage and water ages in cold environments. The model of course has limitations related to the process physics and the assumption of complete isotope mixing within each compartment, which may not hold true. However, the authors are transparent as to its shortcomings in most places, and it is of little value to be overly picky with regards to the choices that are made. The manuscript is well written, and the figures are clear and of high quality. I would like the authors to consider the comments below and I believe the manuscript is suitable for publication after minor revisions. The main conclusion of the work is that soil frost had an early season influence on the ages of transpiration, with less of an influence on water ages of evaporation. Second, that the new module can simulate soil frost dynamics. While I do not dispute this, it is unsurprising that the Stefan-type of equations can simulate frost well, this approach has been used for ages and ages and while perhaps not always a physical realistic representation of ground freezing, it simply works well (as it does here). It would be good for the authors to indicate whey they did not use a more complex thermal scheme, or reference ones. Obviously, one would need more soil layers and computational resources would go through the roof, but a bit more on the 'why' this method was used is good. I would like to focus my comments around the central conclusion re: soil frost and water ages. It would be useful to outline how evaporation and transpiration are partitioned as this would help the reader (although it is likely presented elsewhere) and goes to the central conclusion.

Response to General Comments of Reviewer 2

The authors thank reviewer 2 for their constructive comments for the manuscript. The choice here for using the relatively simple Stefan's equation to solve for the soil frost depth because the authors were trying to minimize the changes to the model structure of EcH2O. The authors had explored more comprehensive thermal schemes, the current model structure of EcH2O resolves the energy balance at the surface through an iterative approach and would not allow for a simple adaptation of the model structure. These changes would result in significant changes to how the energy balance of the surface (and also the canopy) is conducted. This would be an interesting development but would need additional testing in both the winter and summer conditions to ensure that there is not a significant error with a different energy balance estimation. The authors will separately consider how the evaporation and transpiration are estimations in the model description.

Major Comments

R2C1: Equation 1 simulates the depth of the freezing front, but not the soil temperature. I am curious as to how the model simulates soil temperature. I THINK I understand how the surface temperature is driven, and the authors acknowledge that the thermal routine of the snowpack is simple for various reasons. What I'm trying to get at is: does the model simulate a soil temperature and how does this relate to the position of the zero-degree isotherm. Yes, soils will be identified as frozen or unfrozen base on Eq1, yet is there a modeled soil temperature that simply has no freezing routine? More clarity is needed.

Response to R2C1: The model does estimate a soil temperature; however, the implementation of soil temperature was derived for warm climates where there are not discontinuities with the thermal conductivity or heat capacity (due to ice conditions). These discontinuities influence the depth that the soil temperature is recorded as there is no soil temperature profile estimated within the model framework so it is not currently possible to identify the zero-degree isotherm in the model. There is additional work to be conducted with the model to account for the ground heat flux under the snowpack, which may require a more robust thermal diffusion through the snowpack to properly estimate the soil temperatures. The authors will clarify these points in the manuscript when introducing the energy balance of EcH2O.

R2C2: The central conclusion that soil freezing affects transpiration is fine, but is it

simply because the plants are not 'on' when the soil is frozen and soil evaporation is impeded (it certainly would be). When the module is off, plants can transpire, and soils evaporate? Is it this simple? I'm just not sure. More clarity on what drives the transpiration would be helpful as I'm unsure if there can be no transpiration when the rooting zone is frozen – how does this all work?

Response to R2C2: The authors will clarify the effect of frost on transpiration and evaporation estimation in the discussion section. The soil frost does not turn 'on' or 'off' the transpiration or soil evaporation, rather, the soil frost restricts the water available for transpiration. As transpiration is a function of the water available, the transpiration is reduced. When the soil frost routine is 'off', more water is available for the transpiration and thereby isn't restricted to the same degree.

R2C3: Is there sublimation in the model? I see that latent heat is set at 0 when there is snow – why? What impact does this have when snow is melting and sublimation maybe important.

Response to R2C3: There is not currently a sublimation module from the snowpack surface within EcH2O (latent heat set to 0). However, there is some sublimation from the canopy interception, though there is not currently a phase dependent interception within the model (SWE and rainfall depths have the same interception capacity in vegetation). This may have some effect on the water volumes during the spring months and increase the dominance of the soil frost conditions. A brief discussion will be included about the influence of sublimation.

R2C4: For Equation 7, what is the basis of the amplification factor C. Does equation 7 pre-serve an isotope mass balance throughout all time steps (I'm assuming so – but it should be stated).

Response to R2C4: The meaning of C will be added to the manuscript. While S is representative of the shape of the snowmelt fractionation curve (i.e. timing of the melt), the amplification factor is representative of the atmospheric effect on the fractionation (i.e. RH and temperature effect). To minimize “moving parts” in the model, this was held constant and calibrated. The manuscript will be adjusted to better reflect what this term means and that Eq 7 serves as the isotope mass for the snowpack of the model.

R2C5: The authors use ERA-Interim data to drive the radiative component of the model. For a few years, there was overlap. Did they investigate the bias of the ERA data and correct? I’m assuming ERA-I would work well in this location of Europe, but it’s good to check as it can have biases which will propagate through the energy balance calculations. The underestimation in net radiation is a bit concerning – and latent heat as well. So after all this, my question is that if latent heat is, in fact, greater than simulated, what influence would this have on the age estimates (if any?). I assume some and this should be noted.

Response to R2C5: The authors will discuss the influence of the use of the ERA-data within the study and how the use of different radiation forcing data may impact the results of water ages. The authors did examine the difference between the ERA data to the on-site data and there is no noticeable bias with the ERA data at the site.

R2C6: On line 79, I’m not sure that the CRHM reference is correct and the Xie and Gough paper describes the thermal routine that is later incorporated into CRHM (see papers by Krogh for example). The XG method is in CRHM, but this is just slightly incorrect referencing.

Response to R2C6: The authors will revise this reference.

R2C7: The discussion after line 85 is a bit selective and there are dozens of possible reasons for model errors in turbulent fluxes. First, the authors state sensible heat fluxes are underestimated but only show latent fluxes so the discussion should be there or sensible heat data should be provided. Another reason not stated (and noted above) is the nature of the ERA-I data. I’m also unsure as to how snow processes are incorporated into the canopy module re: unloading, albedo change, etc. All I’m saying is that there are many many reasons here where the model could be improved with

physics, and avoid suggesting ‘direct calibration’ is the best way to improve simulations.

Response to R2C7: The sensible and latent heat fluxes (with the net radiation) are all provided in Figure 5 (rather than just latent heat). It was not the intention of the authors to state that “direct calibration” was the only means to improve the estimation of the heat flux estimation, rather than some of the model parameters may be sensitive to the heat flux that was not included in the calibration because the heat flux (evaporation or transpiration either for that matter) were not calibrated. Thereby, any inclusion of those parameters would yield no significant posterior distribution. The authors will modify this section to indicate that there are multiple processes (some of which are not yet included in the model) that may influence the energy balance while stating that the potential reasons are examples of influences.

R2C8: Figures that highlight the differences between soil moisture at depth would be helpful.

Response to R2C8: The authors chose not to include the soil moisture at depth since the model was not directly calibrated to different soil moisture depths. The authors believe that showing this dataset may result in confusion that the model was calibrated to distinct soil depths. This calibration was not directly possible due to the calibration of soil layer depths 1 and 2 which limited the comparability of the calibration with variable depths. The soil moisture at different depths will be added to the appendix for additional information for the readers.

Specific Comments

R2C9: Line 80: under different vegetation communities (forest vs mire). 2) To examine the influence of soil frost on the dynamics and age of water (Comma instead of a period after(forest vs mire)

Response to R2C9: The authors will revise this typo.

R2C10: Line 54: qin: subscript needs to be added

Response to R2C10: The authors will add the subscripts.

R2C11: Line 73: comma needed within coordinates

Response to R2C11: The authors will revise the statement to include the comma.

R2C12: Line 95: "Stable isotopes determinations were carried out" : Fix wording

Response to R2C12: The authors will revise this wording.

R2C13: Table 1: Units of precipitation say m/s → should be moved to wind speed. Units need to be added to other dat. "30 min for Sensible Heat says " 30 in" . Column heading needs to say "Time Period" for top row.

Response to R2C13: The authors will revise this table to include units for all input and calibration /validation data.

R2C14: Line 69: stream isotopes tended to retain a slight "memory" effect from the more enriched late summer..."contributions"? "water"? I think a word is needed here?

Response to R2C14: The authors will revise this statement.

R2C15: Beginning Line 95: While some work has been conducted on assessing the transit or residence times of ecohydrologic fluxes or their partitioning in northern (e.g. Sprenger et al., 2018a); however, few studies have included the influence of frozen conditions on the water movement, which may be significant for the effective transit times during the spring freshet period (Tetzlaff et al., 2018) and flow path modelling in "cold" regions(Laudon et al., 2007; Sterte et al., 2018).

Response to R2C15: The authors will revise this statement to improve clarity.

R2C16: Line 99: Traditionally, water ages in stream water at catchment outlets have been the primary metrics for assessing the transport of tracers. Should this read: Traditionally, isotopic tracers in stream water at catchment outlets have been the primary metrics for assessing water ages.

Response to R2C16: The authors will revise this statement with the reviewer's suggestion.

R2C17: Line 29: snow and early spring snowmelt), and snowpack is the amount "weighed" age of solid precipitation (*Should this be weighted)

Response to R2C17: The authors will revise this statement with the reviewer's suggestion.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-84, 2019.