The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-88-RC2, 2020

The Cryosphere
Discussions

Interactive comment on "How vadose zone mass and energy transfer physics affects the ecohydrological dynamics of a Tibetan meadow?" *by* Lianyu Yu et al.

Anonymous Referee #2

Received and published: 22 June 2020

"How vadose zone mass and energy transfer physics affects the ecohydrological dynamics of a Tibetan meadow?" by Yu et al. addresses the importance of including freezing and thawing processes and coupling water and energy balance equations of vadose zone models to simulate ecohydrological dynamics. The authors use data from a high-elevation meadow of the Tibetan plateau to evaluate the outcomes of their different simulations. From their results, the authors claim that adding model complexity improves the model estimates of soil temperature and moisture but may not be necessary to model vegetation dynamics in these regions.

The manuscript is generally well written and understandable. While the presentation

and interpretation of the results makes sense, it sometimes lacks clarifications and could benefit from a deeper analysis. I think the model, even fully coupled, does a poor job at reproducing ice content and thermal conditions of deeper layers. As pointed by the authors, some data collected to evaluate the different simulations seem wrong, while others are simply missing, which means it can be barely used to validate any of the simulations. However, the conclusions of this manuscript remain valid and of considerable interest and is valuable to anyone wondering about the importance of including dynamic freezing and thawing processes in their models. Overall, this manuscript has the potential to be an interesting and useful contribution to The Cryosphere, but some critical issues, mostly in the methodology section, need to be addressed before considering publication.

I also think this manuscript does not reach its full potential. It could use various level of complexity in their approach to include freezing and thawing processes. For example, the CPLD simulation could be divided into different simulations: one that only considers latent heat, one that only considers the effect of ice content on the hydraulic conductivity, etc. This would show modelers which component of the freezing/thawing processes is important to consider, and which one is potentially not. However, I acknowledge that this suggestion would involve substantial additional work, so I understand if the authors would rather not make this change.

Here are my detailed comments.

Introduction: Well written and introduce well the topic and the problematic. However, I think the authors could add a few lines about the efforts done so far to model freezing and thawing processes in coupled water & energy models for cold regions. There are numerous subsurface and surface models already doing that. They do not all necessarily simulate ecological dynamics, but some studies have provided useful information about the impact of neglecting freezing dynamics.

L56: Unclear what is meant by "changes of frozen ground". Maybe the authors meant something like “variations in seasonally frozen ground thickness”?

L58: What are those divergences? Please provide a few examples.

L70: I am not sure “complexity” is the best word here. According to the authors, they are testing their models with or without freezing dynamics, and with or without water and energy coupling. As I understand it, there are no different “complexities” in the way frozen soil is represented or in the way the coupling is achieved.

Section 2.1: It is unclear until the reader reaches the results section if the experimental site is underlain by permafrost or not. Please add somewhere in this section that the site only has seasonally frozen ground.

L93: It is written here that the SMST profiles are measuring temperature and soil moisture at a depth of 80 cm. However, figures from the results section are never showing data at 80 cm, only at 60 cm (which is not listed in line 93). Please fix or clarify.

Figure 1b: If the figure is indeed showing both freezing and thawing fronts, I would recommend using different colors. It is unclear which zone is thawed and which one is frozen (mostly in 2017-2018). I also think the y-axis should not reach 100 cm. Based on the text, there is no sensor deeper than 80 cm. If I understood correctly, the graph is plotting information the authors do not have. Please fix or clarify in the text. Furthermore, is the data here interpolated from the 5 sensors described in line 93?

L124: Please provide more information about the missing data. When and where? This should also be shown in Figure 1b. For example, the authors can use a greyed area to show where data are missing. This is crucial considering the authors are using this data or comparison purposes in Figure 6.

Equation 1: Check units. Are the units of S really s-1 or rather kg m-3 s-1?

Equation 2: Undefined variables: dz, rH, rL, nc

Equation 4: Csoil is presented as the specific heat capacity of the bulk soil, but I think

it is the volumetric heat capacity. The units do not match otherwise. The same issue arises in equations 6, 7 and 8. Equation 8 is from Hansson et al. (2004), where Csoil is defined as the volumetric heat capacity.

Equation 8: Please define which phase is represented by dθ. It should be liquid water.

Section 2.4.4: The authors describe in detail the equations of the two models but do not explain the critical components of the added freezing/thawing processes, except for latent heat. They do provide references but, considering that this is supposedly an important aspect of this manuscript, they should at least describe in more details the different equations used. For example, the calculation of the hydraulic conductivity for a frozen medium is quite important. The authors refer here to Hansson et al. (2004), which uses an impedance factor. This method is widely used, but the impedance factor is an arbitrary number that is likely to change based on the type of soil. The authors never write which impedance factor is used. I am also concerned that the authors are using the method from Dall’Amico et al. (2011) for the soil freezing characteristic curve and the method from Hansson et al. (2004) for the apparent heat capacity. These two papers both use a form of the Clausius-Clapeyron equation with the van Genuchten model, but with different approaches. I could be wrong but, depending on how they have been used, these two methods may not be compatible with each other. More information is required here to make sure the freezing model used by the authors is valid.

Section 2.6: I think this section lacks some important clarifications. First, I think the differences between the models are more complicated than coupled/uncoupled. The vadose zone equations of T&C model are not coupled, because the water and heat equations are independent from each other. On the other end, the heat equation of STEMMUS needs to be coupled to the mass transfer equation because of the consideration of different processes or constituents such as heat advection. However, when adding the freezing/thawing processes, the heat and water equations of T&C becomes somehow coupled (at least one-way) due to the temperature dependency of the hydraulic conductivity. It is unclear if the authors used the names unCPLD, unCPLD-FT and CPLD for their models to characterize the way the heat and water equations are solved or to characterize how the two components (T&C and STEMMUS) are used. If it is the former, I suggest that they authors rename their simulations as T&C, T&C-FT and T&C-STEMMUS. In any case, the coupling characteristics of the different simulations should be further explained. Secondly, it is unclear which processes/parameters are considered in each simulation. For example, the authors state in the discussion that "unCPLD-FT simulation accounting for soil-freezing in a simplified way in comparison to STEMMUS (e.g., the CPLD simulation)" (line 418-419). However, to my knowledge, the difference in the way STEMMUS accounts for soil-freezing processes is never explained. A paragraph describing the different processes that each simulation is accounting for is necessary in this section. This can also take the form of a table.

Figure 3: The grey line is hard to distinguish. I suggest making it slightly darker or thicker.

Figures 5 and 6: It looks like deeper soil moisture and temperature is not well reproduced by any of the models, even though it is not discussed in the text. This has the consequence of poorly representing ice content (Figure 7), mostly in 2017-2018. It is understandable considering the model has not been calibrated and the goal of this manuscript, which focuses more on the growing season, is not necessarily to validate the models. However, I think this poor fit with field measurement should be discussed in the text. There are many reasons that could explain this, such as the presence of heterogeneity in the soil or of freezing-point depression due to increased salinity.

L303: I think they authors meant "unCPLD-FT" instead of "unCPLD".

L307-309: I do not agree that the CPLD model shows a good match with field measurements of ice content, at least not as currently showed in winter 2017-2018.

Figure 8: Please define acronyms in the caption (e.g., "(a) Gross Primary Production" instead of "(a) GPP")

Figure 9: The authors use R^2 here and R in Figure 8. I suggest them to choose one and be consistent.

Figure 10: Please define acronyms in the figure caption.

L381-382: Confusing sentence. Should we compare to unCPLD or unCPLD-FT?

L383-384: What could explain cooler late winter temperatures in unCPLD-FT? Latent heat slowing down the thawing? Lower bulk heat capacity of frozen soil? Please provide hypotheses.

L396-400: This analysis could be improved. The coupling is not the only difference between CPLD and unCPLD-FT. STEMMUS is simulating some subsurface processes that T&C does not (e.g., heat advection, air flow, vapor flow). I recommend providing a more detailed analysis then simply justifying the differences by the coupling. Also, how is ice content and hydraulic conductivity being simulated differently in CPLD than in unCPLD-FT?

L407-410: There are two requirements to experience heat advection: water flow and difference in temperature. While the former is shown in Figure 7, there is no evidence shown for the latter. It would be interesting if the authors could provide some evidence (can be with numbers or words) that heat advection (or convective heat) is mostly relevant during the frozen period.

L413: I think Figure 8 should be referred here instead of Figure 9.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-88, 2020.