The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-88-RC1, 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 #1

Received and published: 17 June 2020

The manuscript by Yu et al. presents a study assessing the role of model complexity of soil physics processes on simulation of vegetation dynamics for a Tibetan meadow site. Three different model versions of the T&C model, with a gradual increase in the complexity of the freezing-thawing treatment, are compared: (1) the original T&C model, lacking soil freezing processes; (2) a modification of T&C in which an ice fraction and freezing/thawing are accounted for; and (3) a coupling of T&C to the soil physics model STEMMUS.

The model versions are parameterized and driven with data from a Tibetan meadow site to evaluate their performance. Differences between results with the three versions

of the model are small for most variables, generally smaller than the model-to-data difference, and the simulated differences are confined to parts of the year where freezing dynamics are likely to play a role. The analysis would benefit from a focus on specific periods of the year where differences arise.

This manuscript is potentially of interest to the readers of The Cryosphere and contains an interesting discussion on the role of freezing-thawing for ecosystem processes in high latitude and high altitude environments, and the importance of model complexity in ecohydrological models. However, in its current form, it contains many inaccuracies in the presentation of the results and ambiguity in the presentation of the model versions, simulation setup and the results, which make it hard to judge the models' qualities. I cannot recommend publication given the current state of the manuscript, but with substantial modifications in the texts and the presentation of the results, I expect that this could become suitable for publication in The Cryosphere. I will discuss my main objections per section below:

Title and introduction

The introduction discusses model improvement in very broad terms, but it does not give a rationale for focusing on soil freezing processes specifically in this study. I would suggest using the introduction instead for a more in-depth discussion of the role of freezing-thawing processes for ecosystem dynamics and the limitations of current models in representing these.

Specific remarks

- Title: The current title is grammatically incorrect. I would suggest removing the question mark at the end of the sentence. Alternatively, "affects" can be replaced by "does affect" (i.e., How does vadose zone mass and energy transfer physics affect the . . .")

- Introduction: The first part of the introduction (L. 33-43) is not very informative for the problem assessed in this manuscript: It describes in general terms the gradual

improvements that have been undertaken in many different types of models, without a clear focus on the research of

- L. 16: consider inserting “those relevant for” after “parameterizations”

- L. 23: “The difference among various complexity. . .”: This sentence is unclear; the meaning of “among various complexity” needs to be specified.

- L. 26: Remove comma; also, I think that “in ecosystem functioning” should read “for ecosystem functioning”.

- L. 58: “there are divergences”. Please explain these divergences.

- L. 69: “The limited knowledge of including or not complex vadose zone processes. . .” Please clarify this sentence.

Methods

The model descriptions in the methods are somewhat unstructured and in part difficult to follow. I see the value of presenting the equations, but I would suggest to introduce the three model setups in the beginning of the methods section, and to describe the processes and equations per setup. For the numerical experiments (section 2.6), please specify which driving variables were used and at which temporal resolution, and how the initial state of the model was determined – without understanding the driving variables of the model, it is hard to evaluate the performance.

Specific remarks

- L. 101: Please specify whether you use remote sensing data for one pixel only, or whether you use it for a spatial analysis, and what it hence is “representative” (L. 102) for.

- Fig. 1b: It is hard to interpret the freezing front data, in particular for winter 2017-18 because of the missing data. Is it possible to mark the times and depths of missing data? Also, smaller symbols in the figure would probably allow to differentiate the

dynamics at the surface better.

- For the subscripts used in the equations, please separate the i used for layering (Eq. 2) from the i used for ice (Eq. 3, L. 196, Eq. 5).

- Eq. 4: Please check this equation: the left-hand side is mass-based, and the right-hand side volume-based. I assume that soil density should be added to the equation.

- L. 235: Convergence of which variables, and which criterion is used to determine if convergence has been achieved?

Results

The energy fluxes displayed in Fig. 3 and 4 are too large, probably by a constant factor (the seasonal dynamics look fine), resulting in fluxes that exceed theoretical limits set by incoming radiation – please check the averaging method. Numbers in Fig. 9 seem more realistic.

In general, the analysis focuses on the entire period of simulation. This is fine for a general overview, but differences between the three model versions tend to be small for most of the period. I would recommend focusing on specific times of the year where the three model versions deviate, and discuss the abilities of the three models for these periods specifically. Also, the authors could consider displaying differences from observations rather than absolute amounts, to make differences between model versions more visible. At the moment, the main conclusion that one draws as reader is that the choice of model version does not matter too much, whereas the differences between the model versions may well provide important insights e.g. in representing fluxes during freezing times.

Specific remarks:

- Can the authors comment on the differences in the models’ abilities to capture LE and H? The model is doing a very good job in capturing LE, but variations in H are poorly captured. Regarding the “overall performance . . . in terms of turbulent flux sim-

ulation", I think this difference between LE and H should be noted. Also, the models simulate consistently a large difference in H between the summer from 2017 and those from 2016 and 2018, whereas observations indicate less variations between summers. What is the reason for the simulated differences between 2016 and 2018 on the one hand and 2017 on the other?

- L. 304 and Fig. 7ab: What causes the pronounced difference in simulated ice content between the two model versions, and is the band of high ice content in unCPLD-FT in the first winter season a model artefact or a real phenomenon? For comparing, it would be preferable to have the same colour scale for plots 7a and 7b.

- L. 326: What causes the difference in onset between unCPLD and CPLD on the one hand and unCPLD-FT on the other? How do the soil physics processes impact GPP?

Discussion and Conclusions

The discussion is generally fine and provides insights in how soil physics processes are expected to affect other parts of the ecohydrological system. It would be nice to see whether conclusions from the authors corroborate existing literature, and where they agree. The conclusion provides a balanced assessment of the advantages and disadvantages of enhanced model complexity for representing the dynamics.

- L. 361: Specify which slope is discussed here (it is clear from the figure, but hard to understand from the text).

General comments

The language would benefit from editing by a native speaker. Also, references should be checked carefully; references seem to be missing from the reference list (Fisher et al. 2014) or need to be specified (Yu et al. 2016a and 2016b in the reference list, but the text refers to Yu et al. 2016).

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