

## ***Interactive comment on “Soil Moisture and Hydrology Projections of the Permafrost Region: A Model Intercomparison” by Christian G. Andresen et al.***

### **Anonymous Referee #1**

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In this study, the authors compare eight land surface models in the PCN-MIP for their performance in modeling hydrological processes in the permafrost region. Authors examine the long-term change of surface (0-20 cm) soil moisture, finding a drying trend in most models. Authors attribute the drying trend to moisture infiltration as the active layer deepens. In modeling runoff, models tend to have structural limitations that underestimate runoff volume compared to observations. Authors conclude that this generation of land surface models

This is overall a necessary and useful study in terms of model intercomparison, presenting the capability of the current generation of land surface models in projecting the

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hydrological state of the Arctic, which also gives some insights to the application of models. According to the cover letter, the authors have tuned the scope of this study to a model intercomparison along with many other revisions after the previous rejection. I think, however, some major issues still remain unclear or unaddressed in this manuscript, I, therefore, recommend a major revision.

Major issues:

#### 1. Definition of permafrost in land models

In figure 1, it is unclear that if all soil layers showing here are hydrologically-active or not. I think authors here show all soil layers since for some models the soil layers are as deep as 47 meters, while in the figure caption authors call the figure “soil hydrological column configuration”. As bedrock layers do not involve in hydrological processes, authors should make clear in the figure how many layers for each model are hydrologically active. More importantly, this unclear statement raises a question in the definition of permafrost in this study. In section 2.2 (line 122), the authors define permafrost grid points with ALT less than 3 meters. However, for some models (JULES, TEM, and UWVIC) showing in Figure 1, the deepest soil layer is less than 3 meters deep. Then how permafrost is defined in these three models? Furthermore, comparisons are somewhat unreasonable because of the way authors define permafrost regions in these 8 models. Showing in Figure 3, the permafrost region actually differs substantially from models. ORCHIDEE has probably the biggest permafrost area globally while JULES has the smallest one. Different regions could correspond to different climate zones and climate changes associated with global warming. At least some differences showing in Figure 2 and 4 are originated from such different permafrost regions. Comparison over the overlapped regions with permafrost for all 8 models could be a more reasonable approach.

#### 2. Runoff in SIBCASA and other models

Figure 6 & 7 show that the annual mean runoff in SIBCASA for the period of 1970-1999

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is close to zero with little-to-none inter-annual variability, which is of course fairly biased from gauge station data. But in Table 3, there is also some (although not high) degree of correlation between observation and SIBCASA-simulated runoff. For the Mackenzie Basin, it is not even the lowest. The runoff of SIBCASA is more “flawed” than “low” to me. Authors should give an explanation/speculation of why SIBCASA simulates such abnormal runoff. Is there any systematic error or technical failure? Or the model itself does not involve runoff modeling? If there is systematic error in SIBCASA-simulated runoff, authors should exclude it from correlation coefficient analysis.

Another potential deficiency of this model-observation comparison is the inconsistency of forcing data. Previous studies, which have also mentioned by authors in the discussion section, have suggested that different forcing data, even for reanalysis datasets that are observational-restricted, can cause some substantial biases in modeled variables. Since runoff is largely dependent on precipitation that is directly from the forcing, some difference of inter-annual variabilities of runoff and their difference between gauge data should be attributed to the difference in precipitation forcing.

### 3. Discussion in the uncertainty of soil and hydrology simulations

In the cover letter, authors mentioned that one of the reviewers in the previous submission rejected the manuscript partially because “The manuscript does not provide anything we don’t already know from the literature, i.e. that the model results vary depending on what model and forcing you use”. The authors tried to fix this issue by re-scoping the study and add discussions on the uncertainty of soil moisture and hydrology simulations. In my opinion, however, authors should work more to improve this part, showing how these differences contribute to the differed performances for different models in the result section.

Readers can actually expect all uncertainties authors discussed solely from Table 1 & 2, where different numerical implementations are listed. Of course, models could differ substantially that may cause differences in model output. In section 4.2, authors should

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work more on linking the discussed uncertainty to the intercomparison results. For example, in line 317, the authors mentioned that involving organic matter could enhance drainage and redistribution of water in the soil column. Is there any evidence showing in the model intercomparison? Are models with organic matter involved showing greater drainage? And if not, is the signal covered up by some other more dominating physical processes? Similar discussion/comparison should be addressed as much as possible for all factors the authors mentioned in section 4.2. Otherwise, this part looks more like a literature review than discussion.

Minor issues:

Figure 2: Why the precipitation for UWVIC behaves abnormally in the historical period, which decreases substantially between 1970 and 2000? If without such decrease, the precipitation, P-ET, and runoff for UWVIC would possibly be fairly close to the average. As precipitation data is directly from the forcing data, authors should explain/discuss how the different forcing datasets in the historical period could bring biases to permafrost thermodynamics and hydrology, and if the biases in the historical period influence the simulation in the projected period.

Figure 3: Specifically for JULES, some Arctic coastal regions in Eurasia and Alaska are not defined as permafrost? Is it due to a lack of spatial resolution?

Figure 4: The Y-axis ticks for ORCHIDEE should be changed to the same as other sub-figures.

Table 3: P-values or significance tests should be addressed for these correlation coefficients.

Discussion section: In my opinion, section 4.1 and 4.2 should switch. As section 4.2 is more closely related and more important to the intercomparison results. Section 4.1, on the other hand, discusses the feature most involved models have not supported yet.

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-144>, 2019.

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