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Discussions

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

Christian G. Andresen et al.

candresen@wisc.edu

Dear Reviewer, I sincerely appreciate taking the time to review this paper and provide very helpful comments and suggestions that significantly improved the clarity, flow and message of the manuscript. I addressed every comment you had and responses are below. Tracked changes are in the supplement pdf file. FYI- modified figures in text will have both versions where top figures will be the old version and bottom figures will be the new, corrected version. On behalf of all authors, Thank you. Christian Andresen

Reviewer #1 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 au-

thors here show all soil layers since for some models the soil layers areas 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.

Authors response: These are important points that needed clarification in the manuscript. Thank you for highlighting them. Changes: -We modified the footnote of Figure 1 to clarify the hydrology layers of the models: "Figure 1. Soil hydrological-active column configuration for each participating model. Numbers and arrows indicate full soil configuration of models of non-hydrologically active bedrock layers. Colors represent the number of layers. " -We also clarified the permafrost estimation for the top 3m soil column which is slightly different among models due to its soil configuration layers ranging from 2-3m. Line 123 now reads: "we define a grid cell as containing near-surface permafrost if the annual monthly maximum active layer thickness (ALT) is at or less than the 3m depth layer depending on the model soil configuration (Figure 1)" -Regarding differences in permafrost extents across models, we decided to compare the full permafrost extent for each model rather than a subset to be representative for each model. The temperature (forcing) differences from the models, and thus, different

areas, are shown in figure 2a and did not raised main concerns. However, we added clarification and highlighted this in the first paragraph of methods section 2.2: L121-123 "This qualitative hydrology comparison was based on the full permafrost domain in each model rather than a common subset among models in order to fully portray the overall changes in permafrost hydrology for participating models."

2. Runoff in SIBCASA and other models Figure 6 & 7 show that the annual mean runoff in SIBCASA for the period of 1970-1999 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.

Authors response: We clarified the issues of low runoff in SIBCASA in the results and excluded it from Figures 5, 6 & 7 to avoid confusion and make the paper clearer. We kept SIBCASA in the correlation coefficient table 3 but highlighted that the analysis was for surface runoff only Changes: -Table 3 header: "Correlation coefficients between simulated annual total runoff and gauge mean annual discharge 1970 to 1999. SIBCASA correlations are for surface runoff." -We also added the following explanatory

statement to the results: L239- "SIBCASA horizontal subsurface runoff was disabled on the simulation because it tended to drain the active layer completely, resulting in very low and unrealistic soil moisture. Therefore, SIBCASA runoff values shown in this study are only for surface runoff."

3. Discussion in the uncertainty of soil and hydrology simulations. In the cover letter, authors mentioned that one of the reviewers in the previous sub-mission 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 work more on linking the discussed uncertainty to the inter comparison results. For ex-ample, 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.

Authors response: These are certainly important points for this study. Particularly, linking the uncertainty to differences processes will be very helpful for the science community. It is important to note that this study is a qualitative analysis (i.e. wetting vs drying) and does not focus on the details of magnitude and spatial patterns of the models signatures. Nonetheless, the manuscript originally addressed some of the

uncertainty sources (e.g. organic matter, runoff, etc) for each model with the help of the modeling groups as "potential" causes of performance. However, the first review of the manuscript was discontent with these speculations and the lack of evidence to support them. Pinpointing these processes directly was difficult and required additional simulations. Therefore, we removed these from the manuscript and we focused on the main modelling challenges (e.g. ALT, soil thermal dynamics, ET, etc.) and supported the statements with literature. Changes: To clarify and remind the reader the focus of the paper, we added the following sentence in the first paragraph of discussion L279-281: "It is important to note that this study is more qualitative in nature and does not focus on the detail of magnitude or spatial patterns of model signatures." Minor issues:

Figure 2: Why the precipitation for UWVIC behaves abnormally in the historical period, which decreases substantially between 1970 and 2000? 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. Authors response: Discussion of how different forcing datasets influence projections it is certainly an important topic. However, in this manuscript we only focused on the overall trend of drying or wetting in these models rather than focusing in the detail at the magnitude and/or spatial patterns of the model signatures. No changes made

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?

Authors response: JULES is missing these cells in the future projections and thus, not added to the figure. No changes made Figure 4: The Y-axis ticks for ORCHIDEE should be changed to the same as other sub-figures. Authors response: Thanks for pointing that out, now all axes in figure 4 are identical Table 3: P-values or significance tests should be addressed for these correlation coefficients. Authors response: We did ran significant tests for these correlations but did not added them. Changes: We added

the stats to the figure and included the following statement in the footnote: "Pearson correlations (r) significant at *p<0.01 and **p<2e-16. "

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.

Authors response: We kept the "Permafrost degradation and drying" section first and details of uncertainty second given that this manuscript is a qualitative analysis of the trends and the causes of the trends (i.e. permafrost thaw and drying across all models). No changes were made.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-144/tc-2019-144-AC1-supplement.pdf

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