

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

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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 pdf 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

Major points Reviewer #2 Abstract. The last sentence is quite general and states things that are very well known already. Could the abstract instead finish with a more inter-

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esting statement pointing out specific knowledge gaps or recommended directions of research? Authors response: We agree and rewrote the last sentence of the abstract following your suggestion. Sentence now reads: “Coordinated efforts to address the ongoing challenges presented in this study will help reduce uncertainty in our capability to predict the future Arctic hydrological state and associated land-atmosphere biogeochemical processes across spatial and temporal scales”.

106 Although method specifics can (hopefully) be obtained in the cited papers, I'd like a few more details here, for clarity. For one thing, it's not clear when the break point between historical, model-specific climate forcing and the common forcing took place. Was this at 1960 or at 2006? Authors response: We clarified the methods as suggested to include this detail: L107 “simulations were conducted from 1960 to 2299, partitioned by an historic (1960-2009) and future simulation (2010-2299)”. 114-117 Along the same lines, for clarity here: On what timescale did the historical CCSM4 climate forcing repeat? Authors response: That was specific for each modeling group and addressed in McGuire et al 2018 (cited in manuscript). 134 Just to be clear, specify what years of model simulations were used for the comparison with 1970-1999 observations. I assume this is also long-term but is it the exact same period, or some other length? Authors response: We used the same years of simulations for comparison and highlighted it in the footnote of Figure 6 and 7. Changes: Figure 6. Runoff anomaly comparison between gauge data and models simulations for the period 1970-1999 mean. 157 Here the authors refer to the “permafrost domain”, but this is not clearly defined in methods. Please clarify in the methods sections whether the study domain is, for each model, all cells with near-surface permafrost above 45 degrees N, as suggested on lines 121-123, or something else. Authors response: This certainly needed clarification in the manuscript and we added/modified the following statements: Changes: In the first paragraph of methods section 2.2 we added: 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.” We also clarified the permafrost esti-

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mation for the top 3m soil column which is slightly different among models due to its soil configuration layers ranging from 2-3m. Line 123-125 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)”

168-171 I am a bit dubious as to whether these patterns hold over longer-term analysis. If this statement is supported by the comparison of 10-year averages shown in Figure 3, I am unconvinced. See comment on that below. Figure 3. Here the authors use a ten-year period to illustrate long-term spatial changes. This is way too short as decadal variability is clearly substantial for some models (Figure 2c). This should be a 30-year period. Authors response: We agree, a 30-year period comparison will be more representative and strengthen the paper. Changes: We changed the analyses to 30 year averages and modified the figure 3 and 4 as suggested. No major changes were observed.

191-193 I think this statement is not supported enough by the data. Either there is a relationship or not, and it would be easier to determine the likelihood of that with a simple x-y plot of the data rather than these box plots. As the authors note, the UWVIC model is not useful at all for this question due to its resolution. But for the box plots shown, I think the SIBCASA model clearly shows no tendency for more drying with ALT increase, which is not acknowledged. The statement should be modified to moderate this claim somewhat. Also, I am wondering at the use of short time periods again here, and would prefer a 30-year period comparison. Authors response: We acknowledged that this is an important point that needed work and clarification. We are aware that these relationships are not straight forward and we highlighted it in the original text after our claim (L191-192) for fairness. Original text reads: L192-195 “However, there is a large spread between soil moisture and ALT changes (Figure 4) which may be influenced by many interacting factors that can be difficult to assess directly and are out of the scope of this study.” In addition, the reason why we did not

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use simple x-y plots was because boxplots were a clearer way to portray this trends and better shows the distribution of these points (compared to a scatterplot of 10,000 points). Changes: Following your suggestion, we strengthen the analysis by running the comparison analysis for a 30-year period and showed the correlation statistics for these relationships to support our statement.

222-223 According to the text, JULES exhibits the highest runoff increase with 0.8mm/day, but Figure 2g shows ORCHIDEE runoff increasing by 1.2 mm/day. Which is correct? Authors response: The statement only tries to convey that JULES has a high precipitation trend but does not imply it has the highest precipitation. No changes made

Minor and language points Authors response: We made all the changes to the document following your suggestions and edits below. 110 The degree symbol seems to have been replaced by a 0 (zero character), at least on my computer. 161 Add “long-term” or “for the period after 2100” or similar to clarify that it’s only after 2100 that most models stay on the drying side for soil moisture – up till then, about half of the models are close to zero change or wetting. I guess this is implicit with the talking of 2299 in the preceding sentence but still, just to be clear. 303 Change “large-scales” to “large scales”. 392 Change “Study” to “The study”. Fig 1. The figure seems to show depths to 3.5 m but the caption says 3 m. Fig 2. The caption says “Figures d, e, f, and g are represented as relative change from 1960 values”. I think “relative change” implies a normalization which is not done here, so I suggest dropping “relative” from the above sentence. Fig 7. At least in the pdf on my computer, the tick labels on the horizontal axis are misaligned and show up inside the plot instead of outside. Please check.

Please also note the supplement to this comment:
<https://www.the-cryosphere-discuss.net/tc-2019-144/tc-2019-144-AC2-supplement.pdf>

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