

The Cryosphere Discuss., referee comment RC2  
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## **Comments on Kouki et al. (tc-2021-195)**

Anonymous Referee #2

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Referee comment on "Evaluation of Northern Hemisphere snow water equivalent in CMIP6 models during 1982–2014" by Kerttu Kouki et al., The Cryosphere Discuss.,  
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### **General comments**

In this work, Kouki et al. use the SnowCCI data (passive microwave + point-based snow depth measurements) to evaluate CMIP6 model-based SWE products and to address dominant factors causing SWE discrepancies using a linear regression approach. The study presents results quantifying the discrepancies between CMIP6 and SnowCCI SWE and the relative contributions of precipitation and temperature to the differences for winter and spring seasons, respectively. The paper is generally written well, and the presentation quality of the figures is great. However, the current manuscript needs to be expanded upon before publication is warranted. Major concerns are given below. I am going to recommend that this paper be returned for major revisions and specifically the inclusions of more extensive literature reviews, additional analysis, and reorganizing the structure of this study for the Cryosphere community.

### **Major comments**

- Introduction

The current section has extremely limited information about the previous studies for climate model-driven snow products in the Introduction section (such as the general performance of SWE products from earth system models within the CMIP, and what are the previous findings of the differences in CMIP6 as compared to CMIP5 snow products, etc). I would strongly recommend including a further description about climate model-

driven snow products and comparison studies (CMIP5 & 6, and statistical or physically downscaled products e.g. CORDEX) with its reliability and uncertainties in Introduction section. Also, the authors should provide much more sufficient information about a recent progress of the SnowCCI products from Luo et al., (2021) and Pulliainen et al. (2020) [this manuscript should provide that information as a standalone work]. I'm sure this will draw potential readers' attention to the necessity of this study.

- Non-mountainous regions

The authors clearly stated that a main differentiation of the current study from one previous study comparing SWE in CMIP6 models (Mudryk et al., 2020) is to consider both temperature and precipitation to explain the differences in SWE. However, I would note that, unlike Mudryk et al. (2020), this study was conducted only for non-mountainous regions because of the unavailability of the SnowCCI SWE product over complex topography. This is crucial for SWE because a large portion of the seasonal snow exists on mountains (for example, 40 to 60% in North America; Wrzesien et al., 2018; Kim et al., 2021). To achieve the comprehensive results across the NH, thus, I strongly suggest that the authors would consider adapting the weight-based blending approach used in Mudryk et al. (2020) with one or more additional reliable SWE products to include mountainous regions in this study. They used this approach to overcome the unavailability of the GlobSnow SWE in complex terrains. The approach allowed them to merge multiple observations and reanalysis products to be able to evaluate CMIP6 SWE over the entire NH domain (not just non-mountainous areas). As the authors may know, the method is that a weight given to the GlobSnow data is linearly reduced with increasing the fraction of mountainous terrain, reaching zero for grid cells containing only mountainous terrain. Regarding dominant portions of the seasonal snow in NH exist in mountain regions, this will surely strengthen the results. Otherwise, it should be clearly stated that this study focuses on non-mountainous regions.

- Wrzesien, M. L., Durand, M. T., Pavelsky, T. M., Kapnick, S. B., Zhang, Y., Guo, J., and Shum, C. K.: A new estimate of North American mountain snow accumulation from regional climate model simulations, *Geophys. Res. Lett.*, 45, 1423–1432, 2018.
- Kim, R. S., Kumar, S., Vuyovich, C., Houser, P., Lundquist, J., Mudryk, L., Durand, M., Barros, A., Kim, E. J., Forman, B. A., Gutmann, E. D., Wrzesien, M. L., Garneau, C., Sandells, M., Marshall, H.-P., Cristea, N., Pflug, J. M., Johnston, J., Cao, Y., Mocko, D., and Wang, S.: Snow Ensemble Uncertainty Project (SEUP): quantification of snow water equivalent uncertainty across North America via ensemble land surface modeling, *The Cryosphere*, 15, 771–791, <https://doi.org/10.5194/tc-15-771-2021>, 2021.
- Mudryk, L., Santolaria-Otín, M., Krinner, G., Ménégoz, M., Derksen, C., Brutel-Vuilmet, C., ... & Essery, R. (2020). Historical Northern Hemisphere snow cover trends and projected changes in the CMIP6 multi-model ensemble. *The Cryosphere*, 14(7), 2495-2514.

- Forested areas

I am not fully sure about the reliability of the SnowCCI product is enough as a single reference dataset to evaluate the CMIP6 SWE product to achieve a general conclusion, particularly in not only mountainous areas (which were already masked), but also vegetated (or forested) areas in this study. There are well-known limitations of satellite-based passive microwave (PMW) sensors for snow remote sensing which have been used to develop the GlobSnow product as the main component. Numerous previous studies have found that the passive microwave SWE products are problematic due to many issues (e.g. deep snow "saturation effect", wet snow, forest canopy, terrain heterogeneity, etc.; Dong et al., 2005; Derksen et al., 2010). I believe many readers may also concern about the issues regarding the reliability of the SnowCCI product, particularly in snow hydrology community (Larue et al., 2017).

To address the issue of the product in forested areas, ideally, employing a model/reanalysis SWE product could mitigate it (such as MERRA2 or ERA5; Colleen et al., 2019). Also, it might be helpful to discuss about recent findings in the Introduction or Discussion sections. For example, a recent study from an independent group found that there were better performances of the GlobSnow SWE product as compared to the passive microwave alone SWE retrievals, particularly in maritime and warm forest environments (Cho et al., 2020; this study used the previous version; GlobSnow v2). I strongly recommend providing clear descriptions how (not) to deal with the issues with sufficient literatures.

- Dong, J.P. Walker, P.R. Houser, Factors affecting remotely sensed snow water equivalent uncertainty, *Remote Sens. Environ.*, 97 (1) (2005), pp. 68-82
  - Derksen, P. Toose, A. Rees, L. Wang, M. English, A. Walker, M. Sturm Development of a tundra-specific snow water equivalent retrieval algorithm for satellite passive microwave data, *Remote Sens. Environ.*, 114 (8) (2010), pp. 1699-1709
  - Larue, F., Royer, A., De Sève, D., Langlois, A., Roy, A., & Brucker, L. (2017). Validation of GlobSnow-2 snow water equivalent over Eastern Canada. *Remote sensing of environment*, 194, 264-277.
  - Cho, E., Jacobs, J. M., & Vuyovich, C. M. (2020). The value of long-term (40 years) airborne gamma radiation SWE record for evaluating three observation-based gridded SWE data sets by seasonal snow and land cover classifications. *Water resources research*, 56(1).
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- Reorganization of the structure of the manuscript

I think the current manuscript should be re-organized. There exist many statements in Discussion section which are supposed to be in "Result" section (or already mentioned here). There is a limited discussion in the current manuscript which should have been here such as "comparison to previous findings and why they are similar/different", "Limitations in the methods and results", and "future perspectives". To make a more structured manuscript, I would recommend separating Data and Method and making subsections within "Data" section such as "SnowCCI", "MERRA-2 temperature", "GPCP precipitation", and "CMIP6". Also for "Discussion" section, I suggest separating the current form into subsections based on the major findings such as "CMIP6 performance", "Relative

contribution of P and T to SWE”, and “Limitations and future perspectives”, something like them. This would help readers explicitly find and understand this work.

- The residual term

There are many parts that just speculated the reasons of the residual term without supporting explanation based on previous findings or sensitivity analysis (e.g. L254-255, L413-414), even though the portion of the term was considerable. (1) Please provide reasonable rationales to support the author’s statements. Regarding this, I think land characteristics such as forest fraction and/or spatial heterogeneity also can impact on generating the residual. To examine this, (2) I would suggest that the authors conduct some sensitivity analysis to provide useful information to be able to explain regional differences in residual from Figures 7 and 12.

### **Specific comments**

L13 Specify in-situ “snow depth”

L54 Even though a satellite remote sensing technique is the only option for “observing” SWE at continental scale, state-of-the-art model/reanalysis SWE products have been successfully estimated, and they have been widely used for hydrological and climate research rather than satellite-based approach (mostly passive microwave) probably due to its limitations above. I would suggest rewriting this part covering not only remote sensing approach but also model/reanalysis products for NH SWE.

- Huning, L. S., & AghaKouchak, A. (2020). Global snow drought hot spots and characteristics. *Proceedings of the National Academy of Sciences*, 117(33), 19753-19759.

L69 They have used four model/reanalysis and satellite SWE datasets and combined them using a blend approach, not just satellite-based data.

L87 – 89 I think presenting the results from the brief analysis (even in supplementary info) should be helpful for keen reader. Also please provide the detailed description of how the difference among the ensemble members are quantitatively smaller than that of models.

L91-92 Is the GlobSnow v3.0 the same product as SnowCCI used in this study? If not, please add the differences.

L100 – 102 Even though the GlobSnow retrieval was improved by combining in-situ snow depth observations as compared to a satellite-only retrieval SWE, there was still large uncertainties for moderate and deep SWE range (about > 150 mm) which is probably due to the “saturation effect” of the volume scattering approach (Derksen et al., 2010; Cho et al., 2020). Was the SnowCCI improve these limitations as compared to the previous version of the GlobSnow? Based on the SWE assessment in Luojus et al. (2021), the overall RMSE for all samples and for shallow to moderate snow conditions only (SWE below 150 mm) is 52.6 mm and 32.7 mm, respectively.

L109-110 What percentage of the seasonal snow-covered area is non-mountainous area over NH? It would be helpful for reader to get the conclusion from this study within non-mountainous areas (if the authors adhere to non-mountainous area).

L112-113, L361-362 Overall, I felt that the paper is overvaluing the accuracy of the SnowCCI product as reference dataset. Please tone down.

L254 What does “model structural factors” mean? Be specific.

L254-255 This is speculation for me. Please provide rationale based on literatures related to this statement.

L259 I do not think  $R^2$  is a “parameter” of linear regression.

Figure 11 To me, the residual terms overwhelmed the contribution of P and T. In this case, are the contributions of P and T still statistically significant?

L337 Please add further discussion “other factors” particularly in spring season. Do you think mismatching of the spatial resolution among the data sets can be one of the reasons? If so, please add some discussion about this. Regarding this, how do you think of the resampling method (nearest neighbor)?

Figure S6 There are areas where the  $R^2$  values are extremely low. I think it would be good to show the  $\beta_P$  and  $\beta_T$  for regions only where there are statistically significant. Please consider applying this throughout all figures.

L342 Be consistent either "Fig" or "Figure"

L360-361 This sentence is redundant as the authors already mentioned. I would suggest rephrasing something like "while ..., our study focuses on analyzing the CMIP6 SWE responses to both temperature and precipitation"

L362-364 I am not sure if the statements are needed here, which were already mentioned several times.

L373 Figs.

L360-364 & 376-380 To me, it seems like the summary, not discussion. I would strongly recommend using here for the detailed discussion, such as what are similar/different and what are new findings from this study as compared to previous studies?

L388 Figs. If you refer more than two figures, please use Fig"s"

L430 I suggest providing much more details of the limitations/uncertainties from the SnowCCI and others to provide sufficient information for those who would use the data sets for their own research, particularly for the issues that I provided in the major comment (such as uncertainties in forested areas which have been challenging areas in snow community). What would the authors expect potential uncertainties in GPCC? Please add discussion sufficiently.