

## ***Interactive comment on “Evaluation of snow depth and snow-cover over the Tibetan Plateau in global reanalyses using in-situ and satellite remote sensing observations” by Yvan Orsolini et al.***

### **Anonymous Referee #1**

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General: The authors have presented results on intercomparing snow depth (SD) and snow cover fraction (SCF) estimates in different reanalyses and observational data sources over the TP. This is an important topic with relatively rare previously documented literature, due to the difficulty in understanding snow conditions over the TP using both observational and modeling techniques, as well as the importance of TP snow to climate prediction. In addition to data intercomparison, authors also implemented a simple parameterization of blown snow sublimation in the ERA5 LSM to further show some problems are caused by blowing snow events previously missing in models, which offers potential explanations on the snow positive bias problem. While I found this paper interesting to the readership of The Cryosphere, below are some

C1

specific comments for the authors to address before it can move further.

1. Around P5L10: change “didn’t” to “did not”
2. P5L10: it is a bit ambiguous “they incorporate snow observations over the TP (ERA-I, JRA-55)”. (1) Do they incorporate TP-exclusive observations, or do they incorporate global observations that covers the TP? (2) Any reference?
3. Around P5L10: why 1cm is used for 100% SCF? Any reference for that? Would 1cm be too small of a value for 100% SCF?
4. Major-ish: P6L10: what type of uncertainties? How would they potentially impact the results? Authors need to be more specific about this.
5. Around P6L15: change to “IMS data is not used above 1500 m in ERA-5, i.e., xxx, while it was still used in ERA-I”.
6. Around P6L20: change “specificity” to “special conditions”
7. Around P6L25: change to “a horizontal resolution of 0.25 degree” or “a grid cell size of 0.25 by 0.25 degree”
8. Around P7L10: what does “on a non-zero accumulation throughout winter” mean?
9. P7L10: change “by a factor 5 to 10” to “by a factor of 5 to 10”
10. P7L12: “remaining SD” does not make sense? Consider revise it
11. In presenting the SD evaluation results: instead of mentioning the MW data as: “The temporal correlation with the in-situ data is poorer than for the re-analyses though (0.32), but is established over two years only.”, I think authors need to more objectively state that the MW data has some problem capturing the SD variability. To me, this level of low correlation (even only with two years of data) already can help make this conclusion
12. P7L18: what does “the MW data is able to represent shallow layers of the order of

C2

5 cm or less.”? Authors need to revise it with a clearer statement, e.g., “the MW data is able to estimate the SD as being smaller than 5 cm, which is significantly closer to the in-situ observations than re-analyses other than ERA-I”

13. P7L23: Is IMS SCF assimilation the only difference between ERA-I and ERA5? If not, how can the conclusion be reached on “The tendency to reduce or remove the snowpack provided by the IMS observational constraint during assimilation appears to play a major role in bringing ERA-I SDs significantly closer to the in-situ observations.”? This seems to be a conjecture not well supported.

14. P8L5: add “(see Appendix)” after “a thin 2 cm layer is equivalent to 100% SCF”

15. Fig. 3: the grey shading for MW data cannot be clearly seen. Can the opacity be changed lower and the shading be put behind the black line?

16. P8L12: add “(see Appendix)” after referring to MERRA-2’s high threshold. In addition, can the authors provide an estimate of threshold value in SD, using the 26 SWE threshold and an estimate of snow density? This way, readers will be better informed about the threshold problems used in different reanalyses in a consistent way.

17. The Discussion part reads very interesting and reasonable to pinpoint to possible reasons for reanalyses bias problems. Suggestions on P9L20: add some references after “... for this region”. For example, Su et al. (2013; JC) suggested possible snow problems over the TP, which may explain the GCM cold biases there: <https://journals.ametsoc.org/doi/10.1175/JCLI-D-12-00321.1>.

18. P10L31: change “incl.” to “including”

19. Major-ish: About the parameterization of blowing snow sublimation scheme: while I think results in this section are interesting and provide possible reasons on the snow bias problem, the three parameters (A, B, and gamma) used in the parameterization were derived from Gordon et al. (2006) as best fit to their locations, and hence are

C3

highly empirical. How would they potentially influence the results? What if the positive snow bias in TP is not caused by the unaccounted blowing snow sublimation, but it only saw reduced error because of the parameters used to compensate other sources of errors? It will be good for the authors to provide some discussions or sensitivity analyses on these issues. This is a major conclusion of this paper so it may warrant more scrutiny.

20. Major-ish: Figure 8: this part of discussion is interesting. But should SCF a more related variable to Tmax than SD? I saw SD biases are more consistently aligned with Tmin&Tmax biases (Fig. 8d). But in Fig. 8e, the SCF biases for MERRA-2 seem not supporting the Tmax cold bias. Any explanations on that?

21. Major-ish: Table 1: I see less of a need to mention atmospheric model layers/resolutions, than to mention what type of observational data were assimilated? The authors provided some information in the main texts, but not in the table. I believe such information needs to be more clearly summarized here.

22. Major-ish: for climate predictions at the seasonal time scale, the eastern TP snow will be an insignificant source of predictability due to its short memory (snow vanishes in a few days, as the authors observed). However, only one station is on western TP (where snow memory may be longer), and therefore the TP estimates accuracy over western TP is still largely unknown. How can this study inform medium- to seasonal-time scale climate predictions with the results being skewed towards those shallow snow regions? It would be good for the authors to discuss these issues.

23. The English of this paper may still benefit from corrections from native speakers.

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C4