

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2022-87

Anonymous Referee #1

Referee comment on "Impacts of snow assimilation on seasonal snow and meteorological forecasts for the Tibetan Plateau" by Wei Li et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2022-87-RC1>, 2022

Review of "Impacts of snow assimilation on seasonal snow and meteorological forecasts for the Tibetan Plateau" by Wei Li, Jie Chen, Lu Li, Yvan J. Orsolini, Yiheng Xiang, Retish Senan, and Patricia de Rosnay

General comments:

The manuscript describes the impact of assimilating IMS snow cover extent data over the Tibetan Plateau (i.e. over 1500 m in altitude) on the snowpack state as well as on near-surface variables (temperature, precipitation, wind) and upper air variable (600 hPa geopotential height). It is therefore well within the scope of The Cryosphere.

Although the tools, models and datasets used in this paper do not bring novelty to the scientific community, the approach and goal of the study aim to provide substantial progress and answers to the community. Indeed, this study is very promising and the results bring very important information for scientists around the world.

However, the scientific method is not clearly outlined; this manuscript lacks precision leading to multiple gaps with respect to crucial details. For example, no information is given within the manuscript as to how the assimilation of IMS data is performed and very little information is given on the simulation setup leading to important doubts on the nature of the models used in this study. Furthermore, not enough details are given to describe and explain what is in the figures.

Because this study is very interesting but lacks precision, I suggest major revisions on the manuscript.

Specific comments:

Lines 55 to 57: Is SEAS5 a 3D model then? The wording "based on" confuses the reader as to what is the link between SEAS5 and IFS. Please clarify with great precision, as it is crucial for the readers to have a clear idea of the tools and models you use for this study.

Lines 94-96: Could you add more information about the upscaling method you used? Since IMS is a binary product (0 or 1), how do you obtain a snow cover fraction? What assumptions have you made?

Section 3: this section is insufficient, more information is necessary. It should include the method used to assimilate IMS data (assimilation method, analysed variable, snow model used for background, frequency of assimilation, etc.). Please clarify the role of IFS in your set-up.

Line 128: Did you evaluate/compare the two initial states?

Line 158: The reader is not yet aware of an "inherent" model precipitation excess. Please explain and clarify what you mean. Furthermore, IMS only gives information on the presence of snow, not on the state of the snowpack, so this sentence is misleading.

Line 173: Could you explain how snow density is affected by IMS data assimilation?

Line 179: Could you explain this statement? How is albedo affected by the assimilation of IMS data?

Line 188: Too vague! Which variable are you talking about?

Lines 190-191: Reduced in Depth or in Cover Fraction? Please clarify what you mean by

that.

Lines 207-208: How would you explain the decrease in correlation when using DA?

Line 210: More information should be given as to what we see in these figures: what are the + signs, explain why the CC decrease with DA, explain the high median in CC in WTP (vs. ETP), etc.

Figures S1-5 cannot be in supplementary material if you are referring to them in the text. Multiple paragraphs refer to and explain these figures and the manuscript cannot be fully understood without direct access to them. Please insert them in the manuscript.

Line 241: Explain why.

Line 255: lacks precision. Is this cumulative and total (ie. solid + liquid) precipitation? In this case, how do you convert solid precipitation to mm? Is it averaged over the domain? Lots of information missing in the text and in the corresponding figures.

Line 335+: The discussion about the changes in snowpack states should be discussed before their impact on the atmosphere. Please consider reorganising the Discussion.

Line 345 and throughout the manuscript: please clarify whether you are talking about snow albedo or total land albedo (ie. snow-free and snow-covered land as well as vegetation)

269-271: Could you explain why?

Lines 390-391: This is already mentioned just above in point 2.

Conclusions: Point 2 should be snow specific. Point 3 should be specific to the impact of snow DA on the atmosphere.

The English language needs to be improved throughout the manuscript, but more specific corrections are detailed below.

Technical corrections:

- Hectopascal units are to be written hPa (and not hpa), please correct throughout the manuscript and figures.
- Line 20: replace underestimate by underestimating.
- Lines 64-66: please improve wording.
- Lines 138-139: please improve wording.
- Lines 195 and 257: replace 'the' 5-day by 'a' 5-day
- Line 204: is CC defined before?
- Line 285: please improve wording.
- Line 332: "perform not well", please rephrase.
- Line 362: "leads to the more precipitation"
- Line 388: the use of smaller and larger in this sentence is incorrect, please rephrase.