

The Cryosphere Discuss., referee comment RC3
<https://doi.org/10.5194/tc-2022-87-RC3>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on tc-2022-87

Anonymous Referee #3

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-RC3>, 2022

This work describes the impact of the usage of snow cover data assimilation above 1500m in the ECMWF seasonal prediction system over the Tibetan Plateau, by means of analysing two set of reforecast initialised in different ways. One set of reforecasts is initialised with an analysis using IMS snow cover observations for $z > 1500\text{m}$ in the snow data assimilation system. The other set was initialised without using these observations above the $z = 1500\text{m}$ orography threshold.

The experimental setup gives the opportunity to study the impact of snow initial conditions on seasonal time-scales over the Tibetan plateau. Results are interesting and worth publishing. However, I have some comments that I would like the authors to address before the work is published, that are reported below. For these reasons, I suggest major revisions on the manuscript.

General comments

- I found the description of the model setup not clear enough, missing important details or reported in a confusing way. I suggest the authors to reorganise the "Data" and "Methods" sections. All information regarding the model (horizontal resolution, number of vertical levels in atmospheric and ocean model etc.) should be reported in one section and meaning of acronyms clearly explained (e.g. Ln. 70, "ORCA1_Z75"). For instance, Ln. 90 says that "The reforecasts have a spatial resolution of 0.5° "; however, Ln. 70 and Ln. 120 says that a TCo grid is used.
- The Results section is in many places descriptive and can be shortened, improving conciseness and clarity. For instance, the discussion of scores (CC, MAE) in 4.2.1 and 4.3 could be simplified. Another example is the discussion about snow density, which is not linked with the other variables; the underlying physical mechanism for which density is lower in the DA experiment is not clear from the text. Also, there is large usage of "supplementary" figures, in particular in Sect. 4.3, two paragraph of

discussion of "Supplementary" material. If those figures are important for the discussion maybe the authors can think of moving those in the main text? Otherwise, I would suggest the authors to rearrange the text, moving for instance details that are unnecessary to support the main conclusions to an appendix?

- I acknowledge that the proposed methodology was developed with the climatology differences between West and East Tibetan plateau in mind. However, it looks to me that the main differences in precipitation (Fig. 13), or snow depth (Fig.3), are in a south-located region on the edge of the (arbitrary?) 95° line chosen by the authors. How results are sensitive to the choice of this longitude value?

Specific comments

- Abstract: I would make it clearer that reforecasts are initialised with analysis produced with/without snow assimilation above $z=1500\text{m}$.
- I found the last sentence of the abstract rather vague. Can you be more specific, e.g. which component of the surface energy balance? A plot showing which surface flux is mostly affected would be important to support this last statement.
- Ln 70: I think more details on the model setup should be provided for people not familiar with the specific model (see main comments).
- From the "Methods" section, is not clear if the dedicated analysis experiments are land-surface analysis only or include the analysis of the entire atmosphere+land. Please clarify in the text.
- Was the orography threshold for using IMS observations in the snow assimilation system only removed for the Tibetan plateau region? Or was removed globally, and then the analysis focussed on the Tibetan plateau region?
- For how long the dedicated analysis were run? Are there possible model spin-ups in the land or atmospheric fields that should be taken into account?
- Ln 159: What does "inherent" means? Also snow model biases can contribute to snow depth errors.
- Sect 4.1: The mechanism linking the change in snowfall, snow density and albedo is not clear from this section. An increase of snowfall in the forecast would be associated with more (new) low-density snow depositing on the ground. A fresher snowpack would be associated with a higher albedo. However, the authors found that the albedo decreases in the DA simulation. Why?
- 1: it would be useful to have an indication on where the data assimilation is acting, that is, highlighting the grid points with orography $> 1500\text{m}$ (from the current figure it is hard to see).
- Ln 204-213: Does the fact that CC is lower in the reforecast with snow DA mean that the temperature variability is worsened, but the temperature biases compared to CN05.1 are improved (as shown by the reduced MAE)?
- Ln 233: How would you explain that the correlation against in situ observation gives a different result than the correlation against CN05.1 product?
- Ln 239: It is not clear what "obvious" mean here. Please rephrase. See also at Ln 358.
- Ln 243: what is the (mean) height above the Tibet plateau of the 600hPa surface?
- Ln 266: The Spearman's correlation coefficients (CCs) should be defined the first time it is used in the text.
- Ln 324: it improves in mean error but decrease correlation.
- I found the argument of "horizontal heat transport" a bit speculative. The authors should also show horizontal temperature maps to clearly see if warmer air is advected with the wind. For instance, could the "convergence zone" cause colder temperature from surrounding snow area (or higher mountains) to be advected over the region?

- It would be useful to have a time series of snow depth, similarly to what it is provided for air temperature and precipitation. It would enable understanding if the increased snowfall in the snow DA reforecast compensates to some extent differences due to the initialisation. It would also clarify differences in summertime snow melt in the two reforecasts.

Technical comments

- Ln 121: typo, "OCEAN5".
- Ln 204: "CC" is not defined in the text.
- Ln 269: suggestion: I would say "lower", not "weak".
- Fig. 13 legend looks wrong to me. Is it not the "spatial differences in daily precipitation (mm) between the ensemble reforecasts and GPM data" **the top and middle row** (not column)?