Reply to Referee comment 3

Dear Editor and Reviewers:

We would like to thank the editor and all reviewers for their valuable suggestions and comments on the manuscript. These comments have not only improved the quality of the current manuscript, but also are beneficial to our future research in general. All point-by-point responses are presented as follows and we have carefully revised the manuscript based on these comments. For clarity, all comments are given in the original version, while responses are marked in blue.

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 analyzing two set of reforecast initialised in different ways. One set of reforecasts is initialised with an analysis using IMS snow cover observations for z>1500m in the snow data assimilation system. The other set was initialised without using these observations above the z=1500m 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.

Reply: Thanks for the positive evaluations and comments, all the comments and suggestions have been addressed and incorporated into the revised 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.

Reply: Sorry for the confusions. The "Data" and "Methods" sections have been reorganized as the "Methods and Data" section. All information regarding the model is reported in section 3.1 "Methods". More details on the model setup will be added into the manuscript. The resolutions of atmospheric and ocean model have been stated in more clear terms: The configuration for these experiments was largely similar to the current SEAS5 but with lower atmospheric (~ 0.44°) and ocean (~ 1°) resolution and a newer IFS model cycle (CY45R1).

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 paragraphs 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?

Reply: Thanks for the comments. The Results section has been simplified to improve conciseness and clarity, especially for the discussion of scores in 4.2.1 and 4.3. The discussion about snow density has been moved to the Supplementary as it is not linked with the other variables. The inconsistent change between snow density and forecast albedo is because that the variable "forecast albedo" in the manuscript is referred to land surface albedo rather than snow albedo. Moreover, we have rearranged the text, moving Fig. S1-5 to the Supplementary as they are unnecessary to support the main conclusions.

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?

Reply: Thanks for the comments. The longitude 95°E is used as the boundary of west and east Tibetan plateau (TP) in this study considering the high spatial variability of precipitation and temperature in the TP. The choice of the longitude 95°E is not subjective but according to the climate pattern in the TP, i.e., the climate pattern in the east TP (> 95°E) is usually considered as wet, while it is usually considered as dry in the west TP (< 95°E), and also refers to previous studies (Qian, et al., 2003; Li et al., 2020). Moreover, the only difference in the two reforecasts is whether assimilating IMS snow cover data above 1500 m. The significant differences in snow cover fraction before and after snow assimilation are concentrated around the longitude 95°E in the southeastern TP, leading to the significant changes in precipitation and snow depth at regional scale.

Reference:

Qian, Y. F., Zheng, Y. Q., Zhang, Y., and Miao, M. Q.: Responses of China's summer monsoon climate to snow anomaly over the Tibetan Plateau, International Journal of Climatology, 23, 593-613, 10.1002/joc.901, 2003.

Li, D., Yang, K., Tang, W., Li, X., Zhou, X., and Guo, D.: Characterizing precipitation in high altitudes of the western Tibetan plateau with a focus on major glacier areas, International Journal of Climatology, 40, 5114-5127, https://doi.org/10.1002/joc.6509, 2020.

Specific comments:

Abstract: I would make it clearer that reforecasts are initialised with analysis produced with/without snow assimilation above z=1500m.

Reply: Thanks for the comments. We have made this clear in Line 16-18 in Abstract now: To investigate the impacts of snow assimilation on the forecasting of snow, temperature and precipitation, twin ensemble reforecasts are initialized with and without snow assimilation above 1500 m elevation over the Tibetan Plateau for the spring and summer 2018.

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.

Reply: Sorry for the confusions. We admit that this statement is unsuitable as the surface energy balance is not the focus of this study. This sentence has been rewritten: Overall, the snow assimilation can improve the seasonal forecasts through the interaction between land and atmosphere.

Ln 70: I think more details on the model setup should be provided for people not familiar with the specific model (see main comments).

Reply: Thanks for the comments. More details on the model setup will be added into the manuscript.

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.

Reply: Sorry for the confusion. We have clarified that the dedicated analysis experiments include the analysis of the atmosphere and land in Line 111-113: In this study, we analysed the impacts of snow assimilation over the TP on the snowpack state (snow cover fraction, snow depth and snowfall) as well as on near surface variables (land surface albedo, 2m air temperature, 10m wind and total liquid precipitation) and upper air variable (geopotential height and temperature at 600 hPa).

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 focused on the Tibetan plateau region?

Reply: The orography threshold for using IMS observations in the snow assimilation system was removed specifically on the Tibetan Plateau region and maintained elsewhere. The analysis focused on the Tibetan plateau region. We have added the clarification in the revised manuscript.

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?

Reply: There are enough model spin-ups in the land and atmospheric fields. We will add relative information in the revised manuscript.

Ln 159: What does "inherent" means? Also snow model biases can contribute to snow depth errors.

Reply: Sorry for the confusion. This sentence has been rewritten in the revised manuscript: The positive bias in snow depth is also much reduced in the DA reforecasts, which is consistent with the decreases in snow cover fraction due to the added assimilation of IMS snow cover.

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?

Reply: Thanks for the comments. We agree with that an increase of snowfall in the forecast would be associated with more (new) low-density snow depositing on the ground which has been presented in the manuscript, and a fresher snowpack would be associated with a higher snow albedo. However, the forecast albedo used in the analysis refers to the land surface albedo rather than the snow albedo. Due to the smaller snow cover fraction after the added snow assimilation, the land surface albedo decreases accordingly. The "forecast albedo" has been replaced with the "land surface albedo" throughout the manuscript to avoid confusion.

1: it would be useful to have an indication on where the data assimilation is acting, that is, highlighting the grid points with orography > 1500m (from the current figure it is hard to see).

Reply: Thanks for the comment. We acknowledge that it would be useful to have an indication on where the data assimilation is acting. However, regions where the orography > 1500 m account for 98.7% of the whole study area (Fig. R1). Considering that there are 1013 grid points in total, fewer than 13 grid points are located in regions where the orography <=

1500 m, which are mainly distributed in the southern boundary of the study area and have few impacts on the analyses and conclusions. Highlighting the grid points with orography > 1500m might be unnecessary in this situation. Moreover, we have tried to plot the boundary of the regions with orography > 1500 m, however, it is almost coincident with the boundary of the study area. We have modified Line 75-76 in the text of revised manuscript to state that almost all of the study area is influenced by the added snow assimilation: Regions where the orography > 1500 m account for 98.7% of the whole study area.

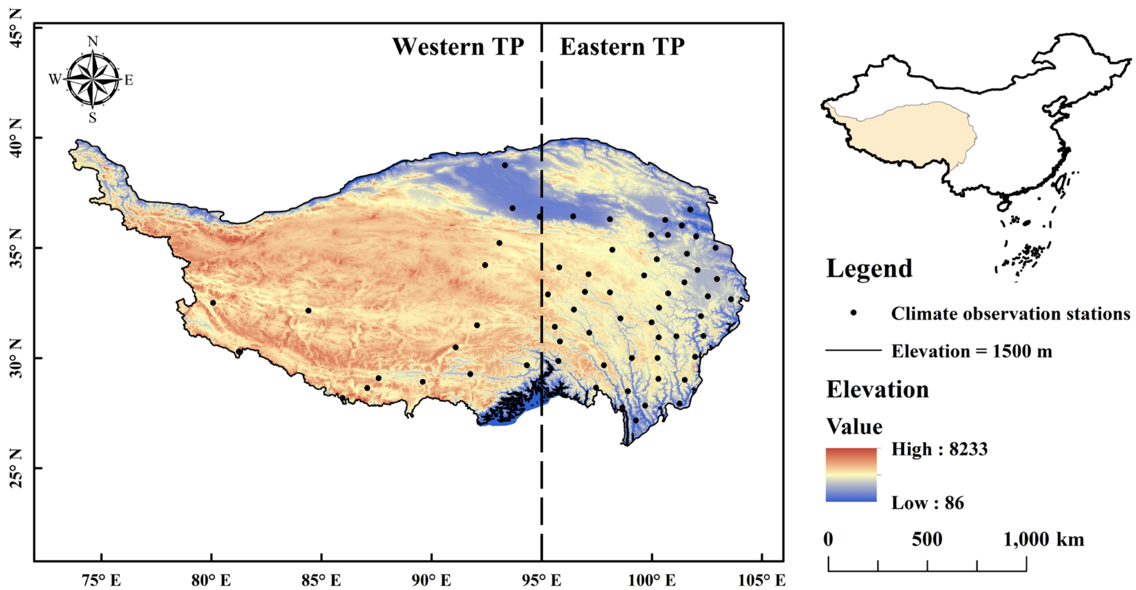

Figure R1: The location and elevation of the Tibetan Plateau (TP) and the location of climate observation stations.

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)?

Reply: We admit that the correlations of temperature reforecasts decrease after snow assimilation. As the data assimilation is performed for snow variables rather than temperature directly, the decrease in correlations of temperature reforecasts might be attributed to the changes in complex regional thermodynamics processes. Moreover, although the correlations of temperature reforecasts decrease after snow assimilation, the added snow assimilation still makes sense as the temperature biases improve.

Ln 233: How would you explain that the correlation against in situ observation gives a different result than the correlation against CN05.1 product?

Reply: The correlations against in situ observations presented in Fig. S2 (now Fig. S3) are spatial correlations, while those against CN05.1 product presented in Fig. 7 are temporal correlations. After calculating the temporal correlations between the two ensemble reforecasts and in situ observations, the results are similar with the temporal correlations against CN05.1 product. We have further clarified in the revised manuscript that the correlations against CN05.1 and GPM products are temporal correlations.

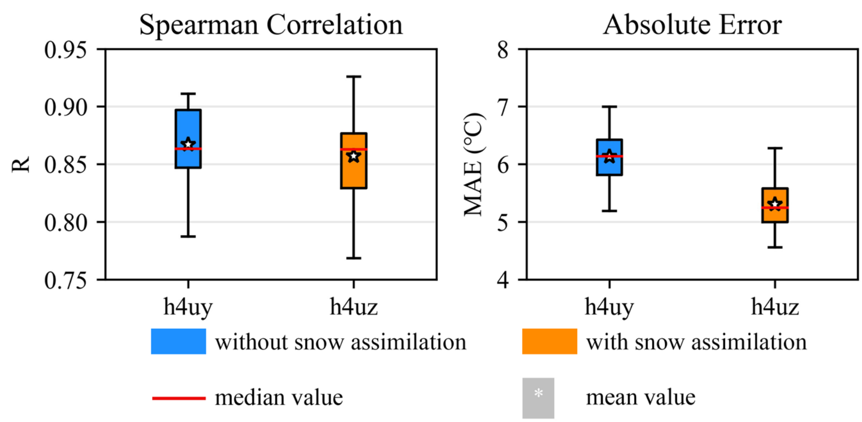

Figure R2: The temporal correlations and mean absolute error between the temperature reforecasts and in-situ observations.

Ln 239: It is not clear what "obvious" mean here. Please rephrase. See also at ln 358.

Reply: Thanks for the comment. Line 238-240 (in the raw manuscript) has been rewritten: With snow assimilation, the wind speed of the DA reforecasts is much larger than that of the control reforecasts in the eastern Tibetan Plateau in either the spring or the whole period. Line 358 (in the raw manuscript) has been rewritten: Therefore, the 10 m wind field is also analysed and the centre of changes in 10 m wind field is observed in the ETP, which is coincident with the centre of changes in snow and temperature in the ETP.

Ln 243: what is the (mean) height above the Tibet plateau of the 600hPa surface?

Reply: We will further check the (mean) height above the Tibet plateau of the 600 hPa surface.

Ln 266: The Spearman's correlation coefficients (CCs) should be defined the first time it is used in the text.

Reply: Done.

Ln 324: it improves in mean error but decrease correlation.

Reply: This sentence has been written: The snow assimilation improves mean error but decreases correlations of the temperature reforecasts when comparing with the CN05.1 data.

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?

Reply: Thanks for the comment. Fig. R3 presents the spatial differences in temperature at 600 hPa between the two reforecasts. It can be seen that in spring and the whole period, the temperature at 600 hPa of the DA reforecasts is higher than that of the control reforecasts for most areas of the TP, especially for the ETP and around the boundary of the WTP and ETP. The spatial differences in temperature at 600 hPa are similar with those in geopotential height at 600 hPa but with reversed changes, i.e., the temperature increases when the geopotential height decreases. Furthermore, the increases in temperature are also consistent with the increases in wind, as the warmer air is advected with the wind. We have added the horizontal temperature maps and relative explanations into the revised manuscript.

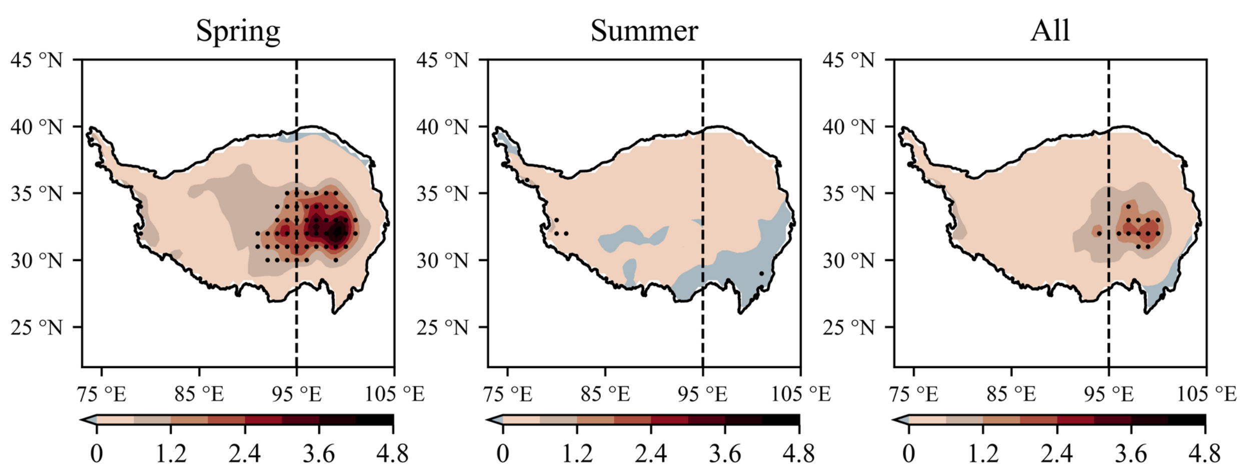

Figure R3: The spatial differences in temperature at 600 hpa (°C) between the two ensemble reforecasts. The stippled regions show the statistical significance of the differences identified by the t-test at a 5% significance level.

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.

Reply: Thanks for the comment. The time series of snow depth from April 1st to July 31st for the two ensemble reforecasts and TPSD are presented in Fig. R4. The snow depth was averaged over the domain (i.e., the WTP and ETP) and the times series were smoothed by a 5-day moving windows. The blue area and line represent the ranges and ensemble-mean of the control reforecasts, respectively; while the orange area and line represent the ranges and ensemble-mean of the DA reforecasts, respectively. The black line represents TPSD data. Both in the WTP and ETP, the ensemble-means of the snow depth of the two reforecasts are higher than those of the TPSD data. However, the snow depth of the DA reforecasts is closer to the TPSD data than that of the control reforecasts. The differences in snow depth between the two reforecasts decrease with time. In the WTP (Fig. R4a), the snow depth of the control reforecasts is higher than that of the DA reforecast for the whole period, while in the ETP, the snow depth of the two ensemble reforecasts is almost the same in the summer. Although the snow depth of the two ensemble reforecasts has an overall downward trend, the snow depth of the DA reforecasts increases around April 15th, which might be contributed to the increases in snowfall in spring after added snow assimilation. The descriptions about the time series of snow depth have been added into the revised manuscript.

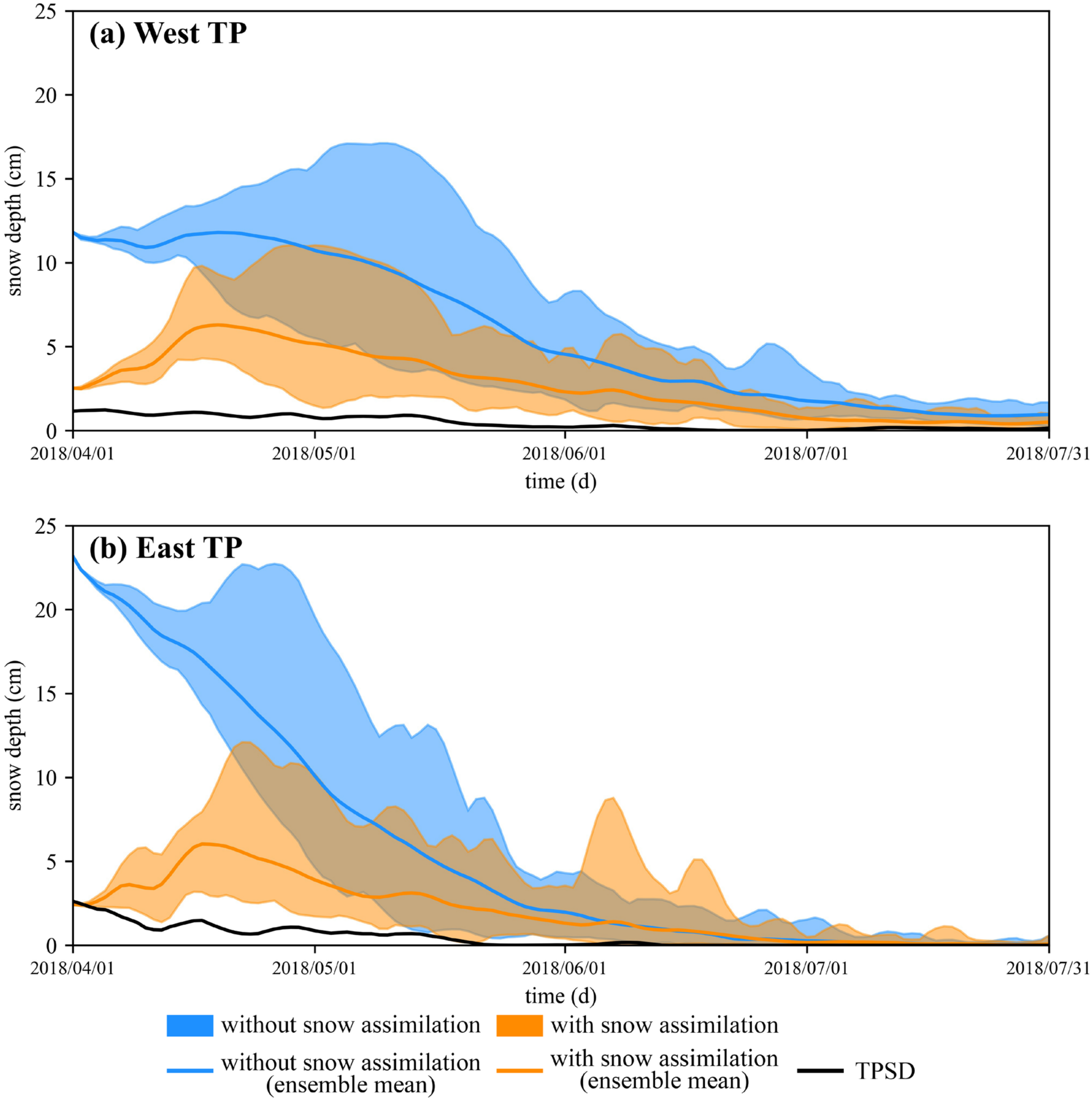

Figure R4: The time series of snow depth averaged over the domain from April 1st to July 31st for the two ensemble reforecasts and TPSD data in the (a) west Tibetan Plateau and (b) east Tibetan Plateau.

Technical comments:

Ln 121: typo, “OCEAN5”.

Reply: Done.

Ln 204: “CC” is not defined in the text.

Reply: The Spearman’s correlation coefficient (CC) is now defined the first time it is used in the text, i.e., in section 4.2.1 “Evaluation of the temperature reforecasts”.

Ln 269: suggestion: I would say “lower”, not “weak”.

Reply: Done.

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)?

Reply: Sorry for the mistake. The caption of Fig. 13 has been corrected: The spatial differences in daily precipitation (mm) between the ensemble reforecasts and GPM data (top and middle rows), and between the two reforecasts (bottom row). The stippled regions show the statistical significance of the differences identified by the t-test at a 5% significance level. The same mistakes in Fig. 3 and Fig. 8 have also been corrected.