Reply to Referee comment 1

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.

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.

Reply: We would like to appreciate the reviewer's valuable comments on this paper. However, we beg to differ with the reviewer about the novelty of the simulations: while the underlying coupled model is based on an operational seasonal forecast model, we present unique, dedicated simulations which had not been carried out before. More information about how the assimilation of IMS data is performed and the simulation setup has been added in the manuscript, and more details about the figures have been described and explained. All the comments have been addressed and incorporated into the revised manuscript.

Specific comments:

Lines 55 to 57: Is SEAS5 a 3D model then? The wording "based on" confuses the readers 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.

Reply: SEAS5 is a forecast model configuration of ECMWF Integrated Forecasting System (IFS) comprising the IFS atmosphere model coupled to the NEMO 3.4 ocean model and LIM2 sea-ice model. The redundant information and reference to IFS model cycle of SEAS5 has been removed and the above information on the relation between SEAS5 and IFS is included in the text when SEAS5 is first described. Note also that a comprehensive description of SEAS5 is provided by Johnson et al (2019).

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?

Reply: Thanks for the comment. The raw IMS snow data used in this study has a resolution of 4 km while the resolution of the reforecasts is 0.5°. The raw IMS snow data is post-processed as following steps to get the IMS snow cover fractions with the same grids of the reforecasts. Firstly, the raw IMS snow data is resampled to a resolution of 0.005° (1/100 of the resolution of the reforecasts) based on the nearest cell. Secondly, a fishnet which has a resolution of 0.5° and is coincidence with the grids of the reforecasts is produced. In each grid of the fishnet, there are 10,000 cells of the IMS snow data as the resolution of the IMS snow data after resampling is one-hundredth of that of the fishnet. The number of cells which are covered by snow is counted and then divided by 10,000 to get the ratio of the snow cover cells in each grid. Finally, the ratios of the snow cover cells in every grid of the fishnet are calculated to obtain the IMS snow cover fractions with the same grids of the reforecasts.

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.

Reply: This paper is not a data assimilation paper per se; it relies on previous studies, cited in the paper, to describe the data assimilation set-up. However, we acknowledge that providing minimum information in the data assimilation approach is useful for the reader. So, the section was updated to provide more information on the IMS snow cover assimilation method, link to the relevant documentation, and details on the analysis experiments and analyzed variables: The IMS snow data assimilation method relies on a two-dimensional optimal interpolation approach which is used to analyse the IFS land surface model (HTESSEL, Balsamo et al., 2009; Dutra et al., 2010) snow depth, with adjustment of snow density when fresh snow is added by positive increments. Full details on the snow data assimilation method are provided in the IFS documentation CY45R1, Chapter 9 (IFS CY45R1, 2018).

References:

Balsamo, G., Viterbo, P., Beljaars, A., van den Hurk, B., Hirschi, M., Betts, A. K., and Scipal, K.: A Revised Hydrology for the ECMWF Model: Verification from Field Site to Terrestrial Water Storage and Impact in the Integrated Forecast System, Journal of Hydrometeorology, 10, 623-643, https://doi.org/10.1175/2008jhm1068.1, 2009.

Dutra, E., Balsamo, G., Viterbo, P., Miranda, P. M. A., Beljaars, A., Schär, C., and Elder, K.: An Improved Snow Scheme for the ECMWF Land Surface Model: Description and Offline Validation, Journal of Hydrometeorology, 11, 899-916, https://doi.org/10.1175/2010jhm1249.1, 2010.

IFS Documentation CY45R1 - Part II : Data assimilation, in: IFS Documentation CY45R1, IFS Documentation, ECMWF, https://doi.org/10.21957/a3ri44ig4, 2018.

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

Reply: The impact of IMS snow cover assimilation was extensively evaluated in other studies such as Orsolini et al. 2019.

Reference: Orsolini, Y., Wegmann, M., Dutra, E., Liu, B., Balsamo, G., Yang, K., de Rosnay, P., Zhu, C., Wang, W., Senan, R., and Arduini, G.: Evaluation of snow depth and snow cover over the Tibetan Plateau in global reanalyses using in situ and satellite remote sensing observations, The Cryosphere, 13, 2221-2239, https://doi.org/10.5194/tc-13-2221-2019, 2019.

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.

Reply: Thanks for the comments. This sentence has been rewritten: 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.

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

Reply: With the added snow assimilation, the snowfall of the DA reforecasts is more than that of the control reforecasts for the ETP and around the boundary of the WTP and ETP in the southern TP. An increase of snowfall will lead to more (new) low-density snow depositing on the ground. Analysis about snow density has been moved to Supplementary as it is unnecessary to support the main conclusions.

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

Reply: With the added snow assimilation, the snow cover fraction changes a lot, especially for the ETP and around the boundary of the WTP and ETP. Typically, snow and ice have high reflectivity with albedo values of 0.8 and above, and land has intermediate values between about 0.1 and 0.4. Because of the changes in snow cover fraction, the land surface albedo also changes after snow assimilation. We have modified this sentence in the text as: Since the changes of snow cover leads to changes in land surface albedo after snow assimilation, Figure 5 presents the spatial differences in land surface albedo between the two ensemble reforecasts.

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

Reply: Sorry for the confusion. This sentence has been rewritten: the main points are that snow assimilation reduces the positive biases of snow cover fraction and snow depth in spring over most areas of the Tibetan Plateau.

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

Reply: This sentence has been rewritten: The reduced snow cover fraction leads to a diminished surface albedo.

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

Reply: Thanks for the comment. 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. We have incorporated the explanations into the Discussions section.

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.

Reply: Thanks for the comments. The + represents the member which pasts the first and third quartiles when calculating the metrics. The reason why the CCs decrease with DA has been explained and incorporated into the Discussions section: 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. As for the high median in CC in WTP (vs. ETP), the topography is more rugged in the ETP than in the WTP, leading to the large temperature variability in the ETP which makes the temperature simulation more difficult and finally causes the lower correlations in the ETP than in the WTP.

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.

Reply: Thanks for the comment. We have moved the descriptions of Fig. S1-5 to Supplementary. This is because there have been already many figures in the manuscript and the descriptions of Fig. S1-5 are unnecessary to support our main conclusions.

Line 241: Explain why.

Reply: The snow assimilation above 1500 m over the Tibetan Plateau mainly reduces the positive biases in snow cover fraction and snow depth in spring, while in summer, the impact of added snow assimilation on the snowpack state is quite little. Therefore, the changes in 10 m wind are also small in summer. This sentence has been rewritten in the revised manuscript: However, the added snow assimilation has little impact on the 10 m wind field in summer as the snowpack state changes little at the meantime.

Line 255: lacks precision. Is this cumulative and total (i.e., 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.

Reply: Sorry for the confusion. The precipitation here refers to daily and total liquid precipitation (rainfall + snowfall) which has been averaged over the domain (i.e., the western and eastern Tibetan Plateau). Actually, the units of the raw precipitation outputted from model are depth in meters of water equivalent. It is the depth the water would have if it were spread evenly over the grid box. We have further explained and described the figures in the revised manuscript.

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

Reply: Thanks for the comment and the Discussions section have been reorganized. The discussion about the changes in snowpack states is firstly presented in the revised manuscript.

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

Reply: Sorry for the confusion. The forecast albedo in the manuscript refers to land surface albedo. We have replaced "forecast albedo" with "land surface albedo" throughout the manuscript.

269-271: Could you explain why?

Reply: The smaller correlations and larger biases of the precipitation reforecasts after snow assimilation may be partly caused by the uncertainties in observations. The bulk of the precipitation over the TP falls as snow in winter and spring, but the GPM products tend to underestimate snowfall which may result in underestimation of total precipitation. However, the snowfall reforecasts become larger after snow assimilation, especially in the eastern Tibetan Plateau and around the boundary of the western and eastern Tibetan Plateau, which may further lead to the smaller correlations and larger biases between the precipitation reforecasts and GPM precipitation. Relative explanations have been added into the Discussions of revised manuscript.

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.

Reply: Thanks for the comment. The Conclusions have been rewritten:
(1) The snow cover fraction and snow depth of the two ensemble reforecasts are larger than the observations for most places of the TP. With the snow assimilation, the snow cover fraction and snow depth of the reforecasts are closer to the observations. With snow assimilation, the snow cover fraction and snow depth are less for the ETP and around the boundary of the WTP and ETP than that from the control reforecasts, and the land surface albedo of the DA reforecasts is also smaller than that of the control reforecasts for the regions where the snow cover fraction reduces. However, the snowfall of the DA reforecasts is more than that of the control reforecasts for the ETP and around the boundary of the WTP and ETP in the southern TP.
(2) When using the CN05.1 temperature as benchmark, the two ensemble reforecasts can capture the seasonal tendencies of the observed temperature. However, the reforecasts tend to underestimate daily temperature. The added snow assimilation improves mean error but decreases correlations of the temperature reforecasts when comparing with the CN05.1 data. The temperature of the DA reforecasts is significantly higher than that of the control reforecasts for the ETP and around the boundary of the WTP and ETP due to the decreased snowpack and smaller land surface albedo after snow assimilation. Moreover, the wind (at 10 m) transports more heat from surrounding regions to the centre in the ETP after snow assimilation, which further leads to a higher temperature.
(3) When using the GPM precipitation as benchmark, the precipitation reforecasts perform better in the WTP than in the ETP. With the snow assimilation, the biases between the precipitation reforecasts and GPM precipitation becomes larger in the

ETP while smaller in the WTP, which may be partly because of the uncertainty from the GPM observations. The precipitation of the DA reforecasts is significantly more than that of the control reforecasts for the ETP and around the boundary of the WTP and ETP as the higher temperature in these regions enables more moisture to be carried to the atmosphere. Moreover, most of the increased precipitation is in the form of rainfall.

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

Reply: Thanks for the comments. We have improved the English language throughout the manuscript by native speakers and modified the manuscript and figures according to all the specific corrections.

Technical corrections:

Hectopascal units are to be written hPa (and not hpa), please correct throughout the manuscript and figures.

Reply: Thanks for the comment. Hectopascal units have been corrected as hPa throughout the manuscript and figures.

Line 20: replace underestimate by underestimating.

Reply: Done.

Lines 64-66: please improve wording.

Reply: Thanks for the comment. This sentence has been rewritten: However, assimilating the IMS snow data but only below 1500 m elevation might influence the forecasting ability over the TP, and inclusion of IMS above 1500 m elevation are probably beneficial to seasonal forecasts at the regional scale.

Lines 138-139: please improve wording.

Reply: Thank for the comment. This sentence has been rewritten: Considering that the only difference between the twin experiments is whether assimilating IMS above 1500 m over the TP, the snow cover is firstly analysed to evaluate the effects of the snow assimilation.

Lines 195 and 257: replace ‘the’ 5-day by ‘a’ 5-day

Reply: Done.

Line 204: is CC defined before?

Reply: Thanks for the comment. 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".

Line 285: please improve wording.

Reply: Thanks for the comment. This sentence is unnecessary and has been removed.

Line 332: "perform not well", please rephrase.

Reply: "the GPM products usually perform not well in detecting snowfall" has been replaced with "the GPM products tend to underestimate snowfall".

Line 362: "leads to the more precipitation"

Reply: This sentence has been removed since the Discussions have been reorganized.

Line 388: the use of smaller and larger in this sentence is incorrect, please rephrase.

Reply: This sentence has been removed since the Conclusions have been reorganized. We have carefully checked it throughout the manuscript.