



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-237**

Matthieu Lafaysse (Referee)

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Referee comment on "Using snow depth observations to provide insight into the quality of snowpack simulations for regional-scale avalanche forecasting" by Simon Horton and Pascal Haegeli, EGU sphere, <https://doi.org/10.5194/egusphere-2022-237-RC2>, 2022

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### **General comment**

In this paper, Simon Horton and Pascal Haegeli address the necessary but challenging task of evaluation of a snow modelling system in support of avalanche hazard forecasting. To address this question, they transform local snow depth observations in a regional scale assessment of snow depth at treeline before the comparisons with numerical simulations. They also compare the predictability of a simple statistical model of avalanche hazard using predictors from either the model or the observations. They finally illustrate the impact of precipitation errors on the simulated stratigraphies. The paper is well written and well structured, with interesting results supporting the discussion. Some methodological choices are unusual, which is of course interesting and probably the main added value of the paper for the community, but these choices would have sometimes required, to my mind, a better justification.

Mainly, the inconsistent spatial scale between snow observations and model simulations is a very well-known problem in spatialized snow modelling evaluation. A strong choice of the methodology of this paper is to adapt observations towards the modelling geometry as described in Section 3.1 rather than adapting model output to observation locations or more simply filtering data with too much spatial discrepancies. Although all methods have advantages and disadvantages, the approach used here is not common compared to previous literature which often evaluate models directly with raw observations without any interpolation or spatial aggregation of observations. There are probably good reasons for using such a specific approach here (specificities of non-conventional observations ? scale of interest for avalanche forecasters ?), but I would have expected a better justification

and discussion of this choice in the paper. Why interpolating observations rather than model outputs ? How this can affect the conclusions ? Does it not amplify our perception of observation uncertainty rather than model uncertainty ? Indeed, all the correction factors in Section 3.1 are very likely to add a significant level of uncertainty rather than considering a snow depth observation as it is, i.e. only representative of the point where it's done. Perhaps, it would also help to introduce this challenge of spatial scale in a more explicit way in the introduction. My feeling is that a significant part of what the authors identify here as « uncertainty of observations » would have been considered in common model evaluations as « unresolved spatial variability » of the simulations. This can be obviously debated, but I think the introduction of the challenge and the discussion of the pros and cons of the methodology compared to existing literature could be improved in the paper.

### **Detailed comments**

L26 Indeed these references assess the ability of Crocus to simulate optical reference but it also worths mentioning that optical satellite observations are also often reduced to a simple Snow Cover Fraction, which is a common evaluation variable in snow modelling (many available references in the snow hydrology community).

L30-32 A number of the stations used in the mentioned references also provide real-time observations and are used in real-time monitoring of snow modelling systems.

L38-42 Although I acknowledge that observation uncertainties and spatial representativeness must be accounted for in model evaluations, at the current state of the art of snow modelling, I honestly think it is more than optimistic to consider than snow simulations can outperform the accuracy of snow observations at the local scale. The last sentence of the paragraph is definitely very far from the perception of snow modelling by French avalanche forecasters ! I would recommend to be more specific on the contexts, and especially to limit the spatial scale for which this statement applies.

L43-48 It is true than precipitation forcing is always found as the main source of uncertainty of snow modelling, but other uncertainties can not be ignored. Especially snow depth simulations are also known to be especially sensitive to the accuracy of longwave incident radiations (Raleigh et al, 2015 ; Sauter and Obleitner, 2015 ; Quéno et al. 2020). They can also be affected by very uncertain parameterizations of new snow density (Helfricht et al., 2018). Therefore, it should be more clear than the evaluations performed in this study assess the ability of the whole system to simulate snow depth (including all forcing errors and snow modelling errors, but not reduced to precipitation errors).

L59-60 The limitation of data to the end of March has a strong impact on the scope of the study, which should be better emphasized. Indeed, it is rather clear from Figure 6 that this paper only focuses on the snow accumulation period and that the melting period is excluded from the analysis.

L83-90 I understand the choice to sample simulation points to reduce numerical costs, but indeed in that case as mentioned by the previous reviewer, it is questionable to select only the closest point rather than smoothing NWP output among different points of the 10 km grid cell, especially in the context where these simulations are going to be compared to spatially smoothed observations.

L109 I don't understand the choice of summing hourly variations of HS to obtain 24h height of new snow. Indeed, the definition of height of new snow in the International Classification does include the impact of settlement of new snow, melting, or any other process modifying the snow depth during the 24 hours, as the reference measurement of this variable is a snow board where all these processes occur. When using HS to derive HN, the problem of settlement below the new snow also exists, but it is not solved by the sum of hourly values. Can you better justify this choice or maybe redefine the evaluated variable if too different with the standard concept of height of new snow ? Note that daily snow depth variations is also a useful evaluated variable (Quéno et al., 2016 ; Vionnet et al., 2019).

L181 Does this variance really represent the uncertainty of observations or does it simply represent the small scale spatial variability of snow depth which is known to be very high ? Maybe another way to consider the question is should we consider your regional assessment of snow depth at treeline as an observation considering the complex and uncertain protocol necessary for this assessment ?

L228-229 The bias correction method used here is probably sufficient to investigate the sensitivity of snow profiles to precipitation errors. However, I recommend to emphasize here that (1) the assumption behind this method is that snow depth errors are entirely explained by precipitation errors, which is a very strong simplification (see my comment about L43-48), and (2) that this correction method is not the state-of-the-art way to assimilate snow depth observations in a snow model (Largeron et al. 2020, Cluzet et al., 2022, I give references from my team but of course feel free to use other ones as many teams work on that topic).

L246 Again, I am wondering if the word uncertainty is appropriate as it might be associated with measurement errors when it is actually mainly refers to subgrid spatial variability.

L272-274 Does it really make sense to compute a spatial correlation between simulations and interpolated observations when the number of real observations for some subregions is only 1 or 2 stations ? I think it means this metric just reflects the ability of the interpolation method itself to explain the simulated variability of snow depth but it is poorly related to the ability of simulations to explain an observed spatial variability. The same question applies for regions where only a very low number of simulated grid points ( $\leq 3$ ) are considered.

L287 Unfortunately, it is not possible to identify in the maps the position of this transect

as (1) the transect is not materialized in any map, and (2) the maps do not provide the geographical coordinates. Could you improve this ?

Figure 8 The legend for grain types colors is really tiny. Could you add a common and larger legend bar below the Figure ?

L317 I agree it helps to have a correct snow depth, but this is not sufficient to guarantee an appropriate stratigraphy, and this should be remind for readers unfamiliar with detailed snow modelling.

L364 Note that surface precipitation from rain gauges are almost never assimilated in the assimilation cycles of NWP systems, even for rainfall in low lands, so this is not specific to snow observations from avalanche networks. I generally agree with this discussion, but maybe you could limit this comment to the developement of analysis products and evaluation of NWP, but remove the reference to data assimilation in NWP.

Also, it could be mentioned that in some countries (France), the density of snow observation networks and of precipitation observations are unfortunately correlated, which limits the potential added value of incorporating snow observations in analyses products (because they are available only where the precipitation network is already sufficiently dense). This is especially emphasized in Cluzet et al., 2022. This is not the case in Switzerland, where a very dense snow observation network almost everywhere has on the contrary a strong positive impact on precipitation analyses.

L374-378 This discussion raises again the same ambiguity as mentioned before. The point is that observations should definitely be preferred as ground truth compared to numerical simulations, as long as they are considered at their appropriate spatial scale (local and not regional). The uncertainty of interpolation observation products may indeed be higher than uncertainty of numerical models, in their ability to estimate regional snow depth. But I really think it is important to not mix up observations and interpolation of observations,

and not mix up local scale and regional scale. Therefore, too general sentences as « *observations should not be treated as absolute ground truth* » are to my mind inappropriate.

L394-395 I think that the correction method used in this study was fine to illustrate the impact of these errors on snow stratigraphies. However, even with high quality observations, I don't think that this method should be recommended for an operational system as more advanced data assimilation techniques exist to avoid the strong assumptions of (1) temporally and spatially homogeneous precipitation errors and (2) seeing precipitation errors as the unique source of snow modelling errors.

L445 quality or density ?

Despite these comments, the necessity of this paper is obvious in the context of the development of a new snow modelling system for Western Canada, and I like the idea to not only consider the classical metrics to compare simulations and observations but also to compare their ability to predict avalanche hazard.

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