Our responses are denoted in blue color below.

Review of preprint manuscript : https://doi.org/10.5194/egusphere-2024-395 by Ohara et al. **June 2024.**

General comment

I thank the authors for the nice and easy-to-read manuscript. In my opinion the research topic is of high interest at the area of blooming remote-sensing products for snow. The data and methods are well described (except a few minor missing informations that I detailed below) and the analyses are sound.

Besides some minor formal suggestions that you will find below, my main comments are related to the implications of the research carried out, that could imo be better described or enlarged, for the benefit of the impact of the paper and appropriation of its findings by a wider snow research community. The implications for the subgrid parameterizations of snow depth / SWE for snow or hydrological models are stated but could be described in more details (what are the current assumptions prevailing in models for this, are there different below forest vs in open areas, how do the paper's findings impact on them ?) In general the described applications of the paper's finding should be described more in-depth. I think there could be also implications related to the assimilation of station snow depth data within operational hydrological models. This remark pertains both to the Introduction and to the Discussion or conclusion parts.

Overall my appreciation of the paper is positive and I encourage its publication provided the above main comment and the following minor comments are addressed.

Thank you for your positive evaluation.

We agree that the discussion on detailed applications is useful. We added following paragraph in the end of Discussion section (Line 419-430):

"It is encouraging that snow depth and SWE distributions are generally well approximated by the Gaussian or weak non-Gaussian distribution, which is a fundamental assumption for statistical characterization of sub-gird variability used in snowpack estimation by remote sensing. The non-Gaussianity found in the partially snow-covered areas may also be modeled by truncated normal distribution although it must be tested further. Moreover, weak non-Gaussian distribution would enable asymptotic methods including the Edgeworth expansion method proposed by Pires and Perdigão (2007). For instance, the non-Gaussian asymptotic method or information metric can effectively determine the saddle point approximation of the joint probability density functions (PDF) through maximizing the Shannon entropy between the remotely sensed signal and the SWE. Thus, the quantification of non-Gaussiany in snow depth/SWE distributions would be an important milestone toward accurate snow water quantification using remote sensing techniques as well as grid-based snow and earth surface models."

Minor comments

P2 L44 : you could cite here a bit more literature in support of this statement and extend it to regional climate modelling (for instance citing *Rudisill et al 2024, Lalande et al 2023*)

Thank you. They are nice publications to cite here.

P2 L56 : Luce and Turbonton → Tarboton

Corrected. Thank you.

P3 L 61 : SWE → basin-wide SWE

Thank you.

P3 L83 : I am not a statistician expert, but I would argue that the landing location is affected by microtopography and meteorological effects at the micro-scale (e.g preferential deposition downwind of a crest); is this compatible with "identically distributed"; isn't there a scale effect or spatial aspect to consider?

The phrase, "without microtopography and meteorological effects" was added for accuracy here. Thank you.

P3 L 87-89 : " This implies the presence of both systematic (non-Gaussian) and random (Gaussian) mechanisms in snow accumulation and ablation processes.". I don't see the implication link with the previous sentences. Or rather : I see it, but I think that the meaning of "in theory"

The sentence was revised as follows:

"He, Ohara, and Miller (2019) reported non-Gaussian snow distribution in open areas as well as Gaussian snow distributions in the forested, fully snow-covered areas during the peak snow season using airborne Light Detection and Ranging (LiDAR) observations in the Snowy Range, Wyoming."

L82 should be clarified to make this paragraph clearer (If I understand correctly, all the micro-scale/topographic effects of my previous comment are excluded from the initial "in theory" of the paragraph, but this should be explicitly stated)

I think that the revision above adheres to the context of the paragraph.

P5 L 142 : Hydrology → hydrology

Corrected. Thank you.

P5 L 148 : I understand the interest of having statistics of SWE instead of snow depth for hydrological purposes, but are there other motivations behind the use of SWE instead of Snow depth from ASO data ?

Water resources evaluation purpose was the reason to use SWE data. Snow depth data includes more ineffective records in the dataset.

P5 Fig 1 : the equivalent of Fig 1 for the non-Arctic sites would be great, as well as a table with a short description of the different sites (or sub-sites)'characteristics : extent of the data collection zone, spatial resolution of the data, estimated accuracy, date, collection method (GPR, magnaprobe, etc…), vegetation cover/variability, landform(s).

Table 1 was added. Thank you for nice suggestion.

P6 and further : Some study sites lack a detailed description of topography. For instance in subsection 3.1 too little info is given on this aspect ;
Following sentence was added on Line 176-178:
The topography of these grids in the ACP are very flat with elevation variation of less than 1 meter while accurate absolute elevation data are hard to compare due to the spatial inaccuracy of the magnaprobe.

Fig 3 entails iso-altitude lines but we don't know their altitude spacing ;
They are 1 meter interval contour lines. The caption now includes, "… superimposed over the land cover map with 1 meter interval contour lines."

line 241 the polygons are mentioned but we learn only at the very end of the manuscript that there are both low-centered and high-centered.
Thank you. A phrase "found in lower and higher center parts in the left panel of Figure 5" was added on Line 255.

P8 Sect 3.2 : the spatial resolution of the GPR data should be specified for comparison with other monitoring methods
The GPR data are collected based on the frequency of the pulse, which yields very fine or nearly continuous snow depth data. We used the resampled depth data at 0.5-meter resolution. New Table 1 includes the specifications of the datasets.

Sect 3.4 : In general in this section, the effect of forest vegetation on the Gaussianity could be better highlighted by providing explicitly SWE distributions on forest-covered areas vs on other areas. Also in this section 3.4 and further in the discussion and conclusion, the **effect of scales** should be more emphasized : snow depth/SWE on the forest floor may be quite Gaussian when looked at the spatial scale of over a few meters, but at decimetric or centimetric scales this is likely not true.

L 370 : much stricter → less stricter seems more accurate to me (?)
Stricter is correct.

L377 : I very much like this way of synthesizing your findings.
Thank you!

References

Lalande, M., Ménégoz, M., Krinner, G., Ottlé, C., & Cheruy, F. (2023). Improving climate model skill over High Mountain Asia by adapting snow cover parameterization to complex-topography areas. *The Cryosphere*, *17*(12), 5095-5130.

Rudisill, W., Rhoades, A., Xu, Z., & Feldman, D. R. (2024). Are atmospheric models too cold in the mountains? The state of science and insights from the SAIL field campaign. *Bulletin of the American Meteorological Society*.