**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.

**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*)

P2 L56 : Luce and Turbonton → Tarboton

P3 L 61 : SWE → bassin-wide SWE

P3 L83 : I am not a statistician expert, but I would argue that the landing location is affected by micro-topography 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 ?

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" 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)

P5 L 142 : Hydrology → hydrology

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 ?

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).

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 ; Fig 3 entails iso-altitude lines but we don't know their altitude spacing ; 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.

P8 Sect 3.2 : the spatial resolution of the GPR data should be specified for comparison with other monitoring methods

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 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 (?)

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

**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*.