Author's response to RC1

This manuscript applies a recently published method assimilating local snow depth observations and able to propagate information in space to build a new 10-year reanalysis of snow cover based on the OSHD system over Switzerland. This dataset offers an unprecedented resolution and spatio-temporal coverage and is based on state-of-the-art components in terms of meteorological forcing and physical snow model. The dataset will be highly useful for the community to study the spatio-temporal variability of snow over the country and is going to become the new reference for that purpose. Unfortunately, the links currently provided in the introduction, conclusion and list of references are not working, so I was not able to check the availability of the dataset and its conformity with the description. This obviously has to be solved and checked by the editorial team before publication.

Thank you for your careful review and constructive feedback on our manuscript. As noted in AC1 (`https://doi.org/10.5194/egusphere-2026-159-AC1`), we apologize for the inconvenience caused by a formatting error of the URLs in the preprint. Upon publication, the temporary review link will be replaced by a permanent DOI, resolving any data access issues.

The introduction of the manuscript provides a robust analysis of the existing literature and products and their associated limitations, justifying the need for a new snow cover reanalysis over Switzerland. The manuscript reminds the main methodological principles of the simulation system which have been presented in various previous publications. The assimilation method is especially innovative compared to existing literature. The evaluations directly provided in the manuscript are limited to comparisons between snow depth simulations to non-independent observations. Although a previous publication suggests that leave-one-out experiments exhibit scores similar with simulations assimilating all observations, this choice is somehow questionable in terms of representativeness of the skill at large scale. The article also presents annual maps of peak SWE, Snow Melt Out Date and number of snow days obtained from the simulations. It illustrates well how the reanalysis can be beneficial for large scale snow monitoring. It could have been expected to compare some of these diagnostics simulations with satellite products as Snow Melt Out Date can be derived from optical imagery.

The language is perfectly clear and accurate and I did not find any typos in the document. The quality of the presentation of results is excellent. The article structure might be considered as not very standard compared to common literature because the discussion of the known limitations of the system is relatively short in the current version and is not a dedicated section after the presentation of the results. Nevertheless, this article is still easy to read and make the very valuable effort to make public a dataset useful for a potentially large community of users (although unfortunately the dataset can not be accessed from the preprint as mentioned before).

I recommend publication after solving the data access and considering the minor comments below.

We appreciate the positive feedback and acknowledge your recommendations on the evaluation and discussion of the presented dataset. Following your suggestions, we will revise these sections of the manuscript. Please see our detailed responses to your individual points below.

- L60 Crocus instead of CROCUS (it's not an acronym).

  We will do as suggested.

- L83, L325, L330 and L520 The link does not work and must be replaced by a doi which is the interest of zenodo.

  We apologize for the inconvenience caused by a formatting error of the provided URLs in the preprint. As the review link will be replaced by a permanent DOI (which is already reserved) once this publication and the accompanying dataset are accepted, this will not be an issue in the published version.

- L167 It is unclear how the kilometric liquid / solid precipitation fraction of ICON is downscaled on the 250 m grid.

  Downscaling the precipitation fraction of ICON requires a rather technical solution, which is based on the partitioning scheme in Eq. (1) of Oberrauch et al. (2024). For each ICON grid cell, we adjust the temperature threshold T_{thresh}, so that the computed fraction of solid precipitation f_{solid} matches the partitioning of the ICON model output. The adjusted temperature threshold is then interpolated to the 250m grid and reapplied to calculate f_{solid} at high resolution. We will include a description of this downscaling procedure in the manuscript.

- L221-225 The authors explain that the assimilation procedure removes temporal discontinuities due to the switch from COSMO to ICON. However, from our experience (Vernay et al., 2022) temporal discontinuities in the assimilated surface observations might also result in discontinuities in reanalyses. Could the author comment the avaibility of the 444 snow depth stations over the 10-year period and implications in terms of temporal homogeneity of the resulting reanalysis ?

  Snow depth observations are available for all stations over the entire 10-year period. Furthermore, data are manually quality-controlled and gap-filled. Occasionally, individual stations with unrepresentative records are removed (e.g., instantaneous snow depth increases caused by an avalanche hitting the station). While we have not specifically investigated potential discontinuities arising from the removal of a small number of stations, Oberrauch et al. (2025) demonstrated very little difference between model runs using all 444 stations and those using only 300 stations. Since we remove at most a handful of stations per season, no noticeable discontinuities are expected.

- L232-236 In a publication presenting a new dataset, it could have been expected to present directly the main evaluations of the published version instead of relying on a previous publication based on a different version of the system. If possible, I think it would not be redundant (and even very useful) to incorporate evaluations of Snow Cover Fractions from optical imagery.

  An extensive evaluation of the same model system used here is available in Oberrauch et al. (2025), including validation against Snow Cover Fractions from optical imagery. While cited in the previous section (line 223 *ff.*), here we clearly missed the opportunity to mention this validation again. To address your comment, we will add a section that includes a new figure showing results aggregated from Oberrauch et al. (2025).

- L242-249 I am not fully convinced that Figure 4 gives an accurate overview of the skill of the dataset as these observations are assimilated and despite the interpolation process, they can not be considered as independent evaluation data. Oberrauch et al., 2024 clearly show that the leave-one-out experiment exhibits a lower skill. I think it would be more fair to present the skill of the reanalysis from a leave-one-out experiment.

  We agree that this does not constitute a fully independent evaluation and will update the point-based evaluation using the leave-one-station-out simulations.

- L250-260 These comments are very useful but they could be moved to a discussion section after the presentation of results in Section 4. I also think the discussion about limitations should be a bit extended. First, for the self-sufficiency of the paper, it would be nice to incorporate a short paragraph summarizing the main limitations of the assimilation procedure (as discussed in detail in Oberrauch et al., 2024) and a short paragraph explaining the limitation of the 250m resolution in terms of vegetation description (for instance based on the literature of G. Mazzotti). Then, the choice to not assimilate SCF products contrary to some references mentioned in the introduction could also be discussed : what are the main motivations : data availability ? challenges of spatial data assimilation ? and what are the future perspectives to assimilate remote sensing observations in future releases of this dataset ?

  Following your suggestion, we will add a separate discussion section to include a summary of known limitations. As correctly suspected, limitations in data availability and quality are the primary reasons spaceborne observations were not assimilated. To not introduce any kind of temporal discontinuities, we abstained from assimilating SCF datasets of the required quality (e.g., Keuris et al., 2023), which were not available to us over the full 10-year period.

- L303 It would be useful for the users to have an idea of the volume of the total dataset and to mention whether the compression facility of netcdf is used or not in the nc files (and with which compression level) or if compression is only achieved through the zip of all files.

Total file size for individual water years is under 800MB, making ZIP compression more of a convenience for downloading. While the archive size reduction is minimal, further compression (using the NetCDF internal compression) does not seem necessary.

- L309-310 I am not sure if "version history" refers to the dataset or to the codes. Tagged versions of the different code components used to run the simulations will be a useful addition in terms of metadata for reproductibility.

  Herein, "version history" refers to the dataset. In line with the comment below, we will add the code version (tag to a public GitHub repository) to the metadata.

- L330 Even if the purpose of the mansucript is to describe a dataset, a description of code availability used to produce the dataset would improve again the agreement of this work with the FAIR principles.

  We will add a code availability statement that provides references to the version of FSM2OSHD used to prepare this dataset.

Author's response to RC2

This is a review of "A high-resolution snow dataset for Switzerland (2016–2025) combining physics-based simulations and in situ observations". The authors present an existing model setup and assimilation scheme to produce a high-resolution snowpack dataset for 10 years.

It is an absolute delight to read a well written and essentially ready-to-go manuscript. A few minor nits, noted below.

My main criticism is that this should include the NWP met forcing data, so the study could be reproduced. This dataset immediately strikes me as a reference dataset for comparing other numerical models against. And in that context, this would require the input data. I realize there might be redistribution license issues with the forcing data. But, if possible, the inclusion would elevate this from "very cool" to "exceptional community dataset for model validation".

I was able to download and spot check the data. The spot checked metadata looks good.

Thank you for taking the time and effort to review our manuscript.

Providing the necessary forcing fields at hourly resolution and 250 m grid spacing for the entire 10-year period would amount to approximately 2 TB of data, which is not practicable for distribution on a public repository. For that very reason, we have assembled a separate dataset specifically tailored to the needs of model validation, containing detailed observational data alongside NWP forcing data (Magnusson et al., 2025). This separate dataset is from a subdomain of the dataset presented here, featuring a particularly high density of monitoring sites and thus provides an even better testbed for model evaluation. We will mention this in the revised manuscript.

- L29: Add Canada and US example context

  We will add Vionnet et al. (2021) and Godsey et al. (2018) as examples for observational datasets from Canada and the US, respectively. Should you have additional specific datasets in mind, we would welcome the suggestions.

- Figure 1: I realize this is just elevation. However, does the green regions correspond to the treeline? I, personally, find a low-bound of green problematic because it gives a sense of vegetation cover that may or may not be there. It would be excellent to have tree line noted in this figure

  The green colors in the figures cover elevations up to about 2,000 m, which roughly corresponds to the treeline in Switzerland, ranging between 1,900 and 2,300 m (see red lines in Figure A and Szerencsits, 2012). Therefore, if the green shading is interpreted as vegetation or forest cover, this is not an entirely misleading impression. However, explicitly indicating the treeline in the figure could itself be misleading, given that only about 50 % of the area below the treeline is actually forest-covered.

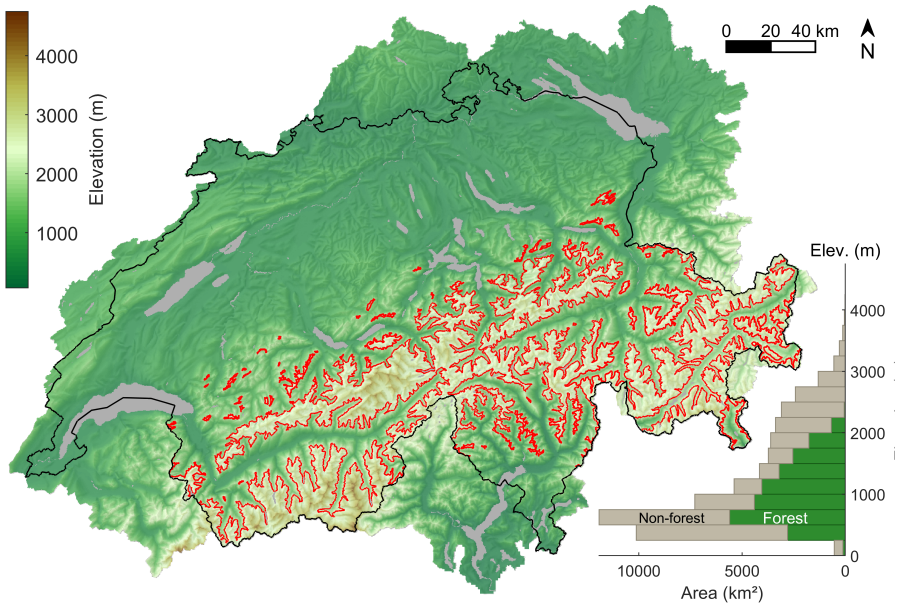

Figure A: Alternative version of Figure 1 in the manuscript showing the model domain. Red lines show a GIS-based approximation of the treeline from Szerencsits (2012).

Instead, we suggest only indicating the forest-covered fraction within the hypsometry plot, as shown in Figure A.

- L98 I don't think m a.s.l requires the a.s.l

  We will remove "a.s.l." throughout the manuscript.

- L102 & L105 Why is Alpine caps? Is it a proper name? If it is, place a map marker in Fig 1 as I don't know where this is.

  Alpine with capital A refers to the European Alps, in contrast to *alpine*, in the more general sense of *mountainous*. Hence, it is a geographic reference, but not a single place which could easily be indicated on the map.

- L135 OSHD = define here

  The acronym OSHD stands for Operational Snow Hydrological Service and is defined in the introduction at Line 63, where it first appears. We acknowledge that this definition may be easily overlooked and therefore suggest reintroducing the acronym in the first sentence of Section 2 (Line 90).

- L140 "period of 24 hours" is this during the model run? Or some other analysis that uses a constant parameterization in the model

  Fresh snow density measurements are manually taken at select locations by measuring the depth/volume and weight of snow accumulated on an artificial surface

(snow board) over the past 24 hours (see e.g., Fierz et al., 2009; Haberkorn, 2019). Hence, we cannot tune directly for fresh snow density but must also account for settling over the 24-hour period. We acknowledge that the sentence was unclear and will revise it in the manuscript.

- L176 "PF-based" for the heading I would not use the abbrv and would write out Particle Filter

  We will do as suggested.

- L192 In the context of the distributions, it would be good to state explicitly which dist is used for each variable

  We will do as suggested and add the distribution parameters (μ and σ) for the three perturbation prior distributions.

- Figure 5 (and surrounding) how is frozen soil infiltration estimated?

  Snowmelt runoff in our model is the meltwater leaving the base of the snowpack, without considering soil processes or what happens to the meltwater once it leaves the snowpack. We will add a proper description of the snowmelt runoff variable to the manuscript.

- L299 is having these data in UTC+0 possible? How is DST handled? A fixed reference UTC+0 would make this easier to ingest (for model inter comparisons)

  The operational service runs on UTC+01, which corresponds to local time in Switzerland for the majority of the winter season. Timestamps in the dataset are provided with explicit time zone information, allowing straightforward conversion. Note that UTC(+XX) timestamps do not account for daylight saving time. Furthermore, since only daily values are provided, the precise timestamp is of limited practical importance.

- L308 Remove extra space in EPSG: 2056

  We will do as suggested.

- L330 I strongly think that if at all possible, the input NWP fields should be included

  As discussed in our general response, providing all forcing fields at hourly resolution and 250 m grid spacing for the entire 10-year period would amount to approximately 2 TB of data, which is unfortunately not practicable for distribution on a public repository.

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