Reply to R2, Anonymous Referee #2

This brief communication reports results of an ongoing project to provide a gridded SWE product over several European regions. There is no state of the art in the introduction. The manuscript does not address a scientific question and focuses on the evaluation of this product. It is useful to publish an evaluation of a new product in a peer-reviewed journal if this product is available to the community. Here, the presented product is not yet publicly available according to the Data availability statement.

> <u>Public discussion:</u> We thank the Reviewer for this comment. The objective of this Brief Communication is not to present the release of a new snow product, but rather to demonstrate and evaluate a framework for producing snow reanalyses within the DestinE/Digital Twin context using spatially distributed remote-sensing and model-based information, without requiring in-situ observations as forcing or assimilation data.
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> We acknowledge that this distinction was not sufficiently clear in the original manuscript. We will therefore revise the Introduction to better position DTE-SNOW relative to existing snow-reanalysis and snow-data-assimilation approaches and to formulate more explicitly the question addressed by this work: whether a modeling framework driven by spatially distributed remotely sensed and Earth-observation-based datasets can reproduce observed snow-depth dynamics and physically consistent spatial patterns across several large and climatically diverse European mountain basins.
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> We also agree that the current Data Availability section may create ambiguity regarding the availability of the DTE-SNOW simulations themselves. All major input datasets used in the framework, as well as the S3M model, are publicly available. While revising our manuscript, we will additionally share outputs of DTE-SNOW. At that point, all input and output data will become public.

In the Results section, the first finding is that the modelled snow water equivalent increases with elevation, as expected. The performance of simulated snow depth with and without assimilating the C-SNOW product is discussed. Previous studies analyzed the benefit of assimilating C-SNOW over the Alps with similar schemes, reporting a modest improvement in simulated snow depth (Brangers et al., 2024; Lannoy et al., 2024). However, they also evaluated SWE and streamflow.

> <u>Public discussion:</u> We agree that an increase of snow accumulation with elevation is, by itself, an expected result. Our intention was not to present this relationship as a novel process finding, but rather as a spatial consistency check of a framework whose precipitation forcing is not derived from local snow or precipitation gauges. We will revise the Results section to make this purpose clearer. We also agree that previous studies have already demonstrated the value of assimilating C-SNOW retrievals over the Alps. We will better acknowledge these studies in the Discussion.
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> The purpose of the present work is complementary rather than a repetition of those studies: here we evaluate the behavior of a common DTE-SNOW configuration across four large basins using the same forcing and modeling framework, including a basin outside the core Alpine domain. We will revise the manuscript to make this distinction and the incremental contribution of the present study more explicit.

Regarding SWE validation, we agree with the Reviewer that this remains an important limitation of the present analysis. As also noted in our reply to Reviewer 1, spatially and temporally consistent observational SWE datasets covering all four domains are not available. We will therefore clarify throughout the manuscript that our quantitative evaluation is primarily an evaluation of simulated snow depth, and not an independent validation of SWE.

The main difference here is the addition of the Ebro catchment. In the Discussion, the performances of this new product are not compared to existing snow reanalyses. The bold conclusion that this method can bring a "globally consistent estimation of snow water resources" or support "SWE of everywhere" paradigm is not sufficiently demonstrated. Indeed, ERA5-Land precipitation is an interpolation of ERA5 precipitation, which assimilated data that are unevenly distributed across the world. S3M was developed using in situ data in the Alps as acknowledged by the authors in conclusion. Therefore, it is hard to believe that this approach can yield similar performances at global scale. Moreover, it is possible that some in situ data that were used by the authors to evaluate their posterior simulation were also used to calibrate C-SNOW over the Alps (Lievens et al., 2022). If correct, it would be more convincing to exclude these data from the model evaluation to avoid circular reasoning.

Public discussion: Our simulations demonstrate the feasibility of the approach over four European basins. While they do not constitute a global validation, the four basins span substantially different climatic, hydrological, and snow regimes, as well as different uncertainty in input data. We therefore consider these experiments a useful proof of concept for the consistent estimation of snow water resources across different landscapes and climates. Further evaluation across a broader range of hydroclimatic environments will nevertheless be required before global applicability can be demonstrated. We will better discuss this throughout the Abstract, Discussion, and Conclusions, and frame this instead as a longer-term objective or opportunity to be tested through future applications in other hydroclimatic regions.

We also agree that comparison with existing snow reanalyses would provide useful additional context. A comprehensive quantitative intercomparison, however, would require harmonizing products with substantially different spatial resolutions, forcing datasets, assimilation strategies, temporal coverage, and target variables, and is beyond the scope of this Brief Communication. We will nevertheless expand the Discussion to better position DTE-SNOW relative to existing regional and continental snow reanalyses and identify a systematic intercomparison as an important next step.

Although no local in-situ observations are directly assimilated into or used to force the DTE-SNOW simulations presented here, some upstream datasets and model components have themselves benefited from in-situ information during their development, as already acknowledged in the Discussion. We will revise the manuscript to make this even more explicit and avoid implying complete independence of the full processing chain from ground observations.

In conclusion, I would recommend resubmitting this evaluation once the product has been officially released and distributed by ESA, with a comparison to other snow reanalyses, including national-scale products maybe.

Public discussion: We understand the Reviewer's concern. However, we view the scientific contribution of this manuscript as the presentation and evaluation of the DTE-SNOW framework rather than the formal release paper of the final ESA product. In this sense, we believe that publication of the methodology and of its first multi-basin evaluation is useful independently of the timing of the product release. In addition, the DTE-SNOW simulations presented in this manuscript will be made publicly available upon resubmission with a persistent DOI, consistently with the other DTE Hydrology datasets.

From a technical standpoint, I am unable to understand how a triple collocation method can be employed with SM2RAIN-ASCAT, IMERG-Late Run, and ERA5-Land in the context of solid precipitation. SM2RAIN-ASCAT is a precipitation product derived from ASCAT soil moisture data, and therefore, it does not measure snowfall ("regions with negative surface temperatures are masked in this product due to the inability to retrieve soil moisture from satellite sensors under frozen conditions" (Filippucci et al., 2025)).

Public discussion: Thanks for this comment. The triple-collocation weights are computed using only the dates on which all three products — ERA5-Land, SM2RAIN-ASCAT, and IMERG-LR — are simultaneously available. A single triplet of weights is then derived for each pixel and, in the specific case of HYPER-P sat, only the weights of IMERG and SM2RAIN-ASCAT are considered for the merging. On dates when one product is missing, its weight is redistributed to the available product. Indeed, SM2RAIN-ASCAT can only provide rainfall estimates and misses the solid-precipitation fraction, because the soil moisture content of a frozen soil does not change. As the reviewer correctly pointed out, SM2RAIN-ASCAT is masked in regions with negative surface temperatures; in such cases, the HYPER-P sat product relies solely on the information provided by IMERG-LR. Nevertheless, we believe that HYPER-P sat still offers an advantage over IMERG-LR, for two reasons: (i) the contribution of SM2RAIN-ASCAT during the short periods in which solid precipitation falls on a non-frozen soil and subsequently melts, and (ii) the downscaling procedure applied to the IMERG-LR product by Filippucci et al. (2024). The above will be added to the manuscript.

In regions above the freezing level, the precipitation is likely identical to IMERG. The same reference indicates that "SM2RAIN-ASCAT product also includes a monthly BIAS correction using ERA5-Land rainfall data (total precipitation – snowfall, Brocca et al. 2019)". Consequently, the HYPER-P_sat product used in this study does not solely rely on satellite products as stated L48.

Public discussion: SM2RAIN-ASCAT is bias-corrected using ERA5-Land rainfall data (total precipitation - snowfall). However, this bias correction is purely climatological. As stated in Brocca et al. (2019): "The monthly correction approach computes the monthly ratios between the climatology of estimated and reference rainfall, i.e. 12 correction factors per pixel. Then, the SM2RAIN-estimated rainfall is multiplied by the monthly correction factors to obtain the climatologically corrected SM2RAIN-estimated rainfall." No concurrent ERA5-Land measurement enters the rainfall estimation of SM2RAIN-ASCAT; only a fixed factor for each of the 12 months is applied. The temporal dynamics of the product are therefore driven solely by satellite observations.

L72: "Also note that we employed the wet-snow masks in C-SNOW to filter data retrieved in such conditions.". This sentence confuses me as it is written above that "Wet snow conditions (...) are detected by the algorithm and masked" (L61).

Public discussion: We agree that these two sentences are redundant and potentially confusing. The intention was simply to state that retrievals flagged as wet-snow conditions by the C-SNOW processing chain were not assimilated in DTE-SNOW. We will rewrite this passage to make this explicit.

Fig 2, The Inn catchment is cropped.

Public discussion: Yes, indeed we refer to that as the "Upper Inn catchment". This will be fixed throughout the manuscript.

Fig 2, "— Powered by ESRI"

Public discussion: We thank the Reviewer for pointing this out. We will ensure that the required basemap/data attribution is reported appropriately in the caption.

Fig 3, model performance may be distorted due to the substantial number of zero values present in the evaluation dataset. A bias that is close to zero holds limited significance if all the stations are located in regions where the snow depth consistently remains zero.

Public discussion: We agree that the large number of snow-free observations may influence aggregate performance metrics and that a near-zero overall bias alone is not sufficient to characterize model skill. We will therefore complement the existing statistics with metrics calculated for snow-covered conditions only, and we will report the number and distribution of snow-covered observations used in the evaluation. In response to Reviewer 1, we will also provide statistics for deep-snow conditions, where the impact of C-SNOW assimilation is particularly evident.

L105 "Accurate estimation of snow water resources using only satellite-derived precipitation, snow modeling, and the optional assimilation of Sentinel-1 snow depth retrievals is not only feasible, but yields mean biases below 0.05 m and correlations exceeding 0.77 at 1-km resolution." The sentence omits that these biases and correlations pertain to snow depth (which is not the "snow water resource").

Public discussion: We agree with the Reviewer. The reported bias and correlation values refer to snow depth and should not be presented as a direct quantitative evaluation of SWE or, more generally, of snow-water resources. We will revise this sentence and related statements throughout the manuscript. This revision is also consistent with the response to Reviewer 1 regarding the distinction between snow-depth validation and SWE estimation.

I acknowledge that some of my comments may appear harsh to the authors. My intention was to provide the editor with an objective assessment. I hope that my comments will ultimately be beneficial to the authors.

Public discussion: We thank the Reviewer for their careful and critical assessment. The comments identify several places where the scope, novelty, terminology, and limitations of our work were not sufficiently clear, and we believe that addressing them will substantially improve the manuscript.