Reply to R1, Dr. Benoit Montpetit

This brief communication presents DTE-Snow, a digital twin of Earth framework to deliver a reanalysis of snow water resources. This framework relies solely on EO data and various models, including the assimilation of snow depths derived from C-Snow, which derives snow depths from temporal changes in backscatter from Sentinel-1. The manuscript is well written and presents the framework well. That said, some of the information can be misleading in my opinion and could be refined in the text.

Public discussion: We thank the Reviewer for their review. Revisions are feasible, and we detail our revision strategy below.

My first comment regards all the datasets and models used. There are lots of acronyms and it gets confusing quickly. To have a good understanding of the impacts of the datasets and models, it would be good to have the uncertainty on the different inputs and model outputs. It would be good to include the different models, the downscaling steps and the uncertainty on the different inputs/outputs in Figure 1.

Public discussion: We agree and will revise Figure 1 to provide a clearer overview of the datasets, models, and downscaling steps. Additionally, we will report quantitative uncertainty estimates when they are available from the corresponding datasets or literature; otherwise, we will indicate the main known sources of uncertainty.

My biggest concern with this manuscript is that the authors imply the DTE-SNOW is to derive SWE, but nowhere do they show the results of the framework to estimate SWE. This is quite misleading and needs further clarification. Obviously, many studies have shown that assimilating snow depth information will improve SWE estimates, but this is not proven in the current manuscript. The authors only show that they can generate a good reanalysis of snow depth. That said, a good reanalysis of snow depth is an excellent step towards providing quality snow information for decision-making. The messaging has to be refined here or a comparison with SWE datasets has to be conducted to demonstrate the capability of DTE-SNOW to provide SWE. The latter could be done in a different study where there's a focus on snow density, which is a different challenge to snow depth to retrieve SWE.

Public discussion: Thanks for this comment. As we mention in the manuscript, spatially and temporally consistent observational datasets of SWE covering all four study catchments are not available, and remain scarce in general. This motivated our choice to evaluate the reanalysis primarily against snow-depth observations. Snow depth represents a major component of the spatial and temporal variability of SWE, although we fully agree that snow density introduces an additional and non-negligible source of uncertainty. Therefore, agreement in snow depth alone cannot be regarded as a direct validation of SWE. We will make this distinction much clearer throughout the manuscript, particularly in the Introduction, Results, and Discussion, and will moderate statements that could otherwise be interpreted as a direct validation of SWE. Such a direct validation is beyond the scope of this Brief Communication, as we also highlight in our response to Reviewer 2.

Abstract: For those unfamiliar with DTE, I suggest to start the first sentence with something along the lines of: "In the context of developing a Digital Twin of the Earth System (DTE) under the Destination Earth (DestinE) initiative, we present DTE-SNOW, [...]"

Public discussion: We agree with this comment and will revise the text as recommended.

l.20: you could be even bolder here and say that "snow is the only component of the water cycle to not have a dedicated EO mission". If a reference is needed, you can cite Montpetit et al. (2026), but with the reference limitation, it is not necessary in my opinion, and it is a known fact, especially in the cryosphere community.

Public discussion: We agree with this comment and will revise the text as recommended.

l.59: you can remove the parentheses around "dry"

Public discussion: We agree with this comment and will revise the text as recommended.

.92-97: it is clear from figure 3 that DA C-Snow has a better agreement for snow depths $\geq$ 2.5 m. It would be good to provide the correlation coefficients for values $\geq$ 2.5 m only to show the considerable improvement when assimilating C-Snow. I would even do this at $\geq$ 2 m since the scatter plot seems to break away from the 1:1 line around 2 m for the open-loop

Public discussion: We agree with this comment and will revise the text as recommended.

L.131: "remotely sensed dataset"?

Public discussion: We agree with this comment and will revise the text as recommended.

References: TC guidelines states a maximum of 20 references, which is exceeded here. Not sure how rigid TC is on this guideline.

Public discussion: You are right about this limitation in references. However, we note that this is a highly interdisciplinary manuscript, involving datasets from the cryospheric and eco-hydrologic community, as well as both EO and modeling. Thus we are hoping that this limitation can be relaxed. The handling Editor has already checked this before the review process and confirmed that we comply with the requirements of a Brief Communication.

Figure 1: Figure 1 is misleading. This suggests that SWE is the main driver of DTE-Snow, but in the results, only snow depth is being assessed. The conversion to SWE is not straightforward, especially in such diverse landscapes.

Public discussion: We will modify Figure 1 to include snow depth instead of SWE.

Figure 2: Figure 2b is not necessary in my opinion. We can see clearly the variations in SWE, and even without knowing the elevations (a DEM could be used as a basemap here), we can infer changes in altitude.

Public discussion:We agree that Fig. 2b is not strictly necessary to interpret the spatial

maps. However, one of the points we aim to demonstrate is that the framework reproduces the observed elevation dependence of snow accumulation across very different Alpine catchments. This is not guaranteed a priori, particularly given the use of a spatially distributed EO-based precipitation product. Since we also discuss this result quantitatively in the text, we believe that retaining the elevation-dependent representation provides useful complementary evidence. We will revise the caption and text to make the purpose of this panel clearer.