Author’s response

Referee 1 Jeff Dozier

The paper shows some nice comparisons between model results (ERA5-Land combined with SnowModel) and measurements from the Airborne Snow Observatory. Given the more discouraging conclusions about such models by Liu et al. (2022), I am surprised, but the analysis here seems robust. As the Conclusion notes, the analysis provides a viable method to estimate the water resources in the snowpack in areas with only an austere information infrastructure.

We would like to thank Jeff Dozier for taking the time to review our manuscript. We are confident that we can address every comment in a revised manuscript as explained below.

A few comments to improve the manuscript:

Line “06” (106?, bottom of section 2.1). The reported accuracy of SWE, <0.01 m, is ambitious. In their reports to the water agencies, ASO quotes a density RMS uncertainty of ±20 kg/m^3, but verification is based on snow courses and snow pillows, which are all on open, flat terrain and have their own uncertainties of SWE and depth.

Especially, you should note that ASO’s translation of snow depth to SWE depends on local measurements of density, typically snow pillows that have a depth sensor also along with snow courses where both SWE and depth are measured.

We added the following paragraph to Section 2.1:

The reported accuracy on the 3 m snow depth products is 0.08 m (Painter et al., 2016) and from spatially intensive sampling, the reported accuracy for the 50 m snow depth products is < 0.01 m (Painter et al., 2016, Figure 15). There are no published references for the 50 m SWE product. However, for a 1m deep snowpack and a conservative 10% uncertainty in snow density (20-50 kg/m^3), we estimate the uncertainty of the 50 m SWE products to be 0.02 - 0.05 m w.e.

Section 2.2.2: How are you getting snow albedo for the EnBal part of SnowModel? The ASO spectrometer can be used to retrieve values, but the combined ERA-Land/SnowModel uses the ASO data for validation, not as a driver. The melt rate and disappearance date of the snowpack are sensitive to albedo and consequent radiative forcing by light-absorbing particles (Painter et al., 2010).

We used the default values implemented in EnBal (Liston and Elder, 2006). The default value of the snow cover albedo is 0.8 in dry conditions. The default value of melting snow albedo is 0.45 under the forest canopy and 0.60 in non-forested areas. This was clarified in the manuscript (Section 2.2.2).

We added in the Discussion that another significant source of uncertainty is related to the albedo parameterization in SnowModel. The deposition of light absorbing particles like dust can reduce albedo and therefore increase melt especially at high elevation (Skiles et al., 2018; Dumont et al., 2020). This might explain the relative increase of the SWE bias between the 1st of April and the 27th of May at all elevations above 2500 m (Figure 5).

Figure 3: The colors used to identify the lines in the plots are too indistinct. Perhaps combine color with line style to make the differences more obvious?

We updated figure 3 with this design

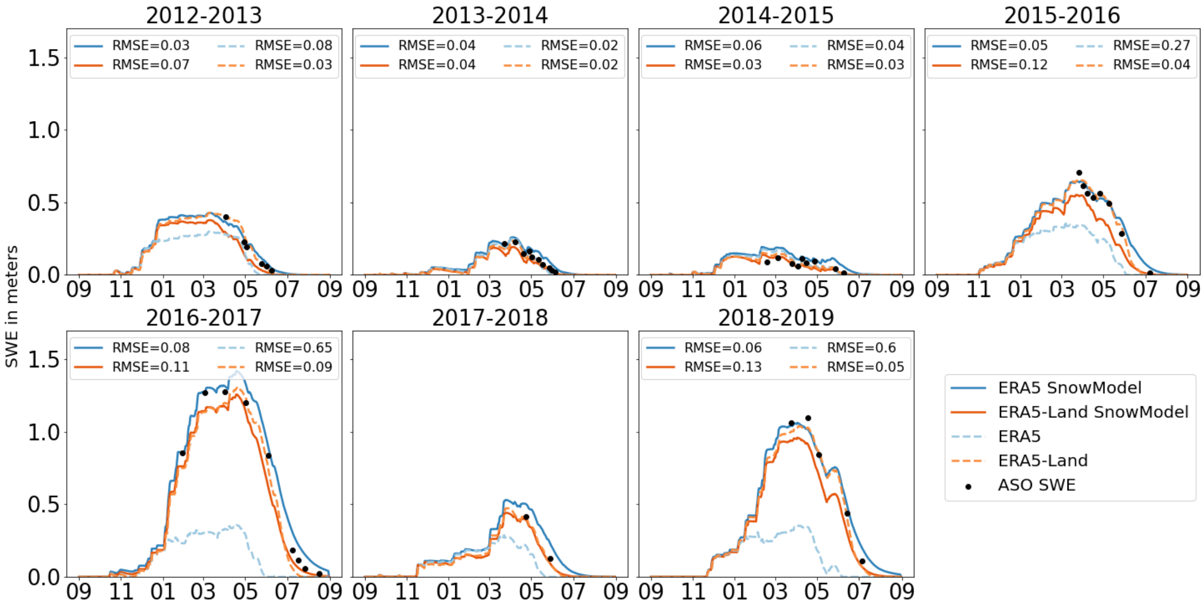


Figure 6: Label the axes. They appear to be UTM zone 11N coordinates, but the identification of the comparison in rotated text is confusing. At first I thought they had something to do with the y-axis.

We have added the axes labels as suggested.

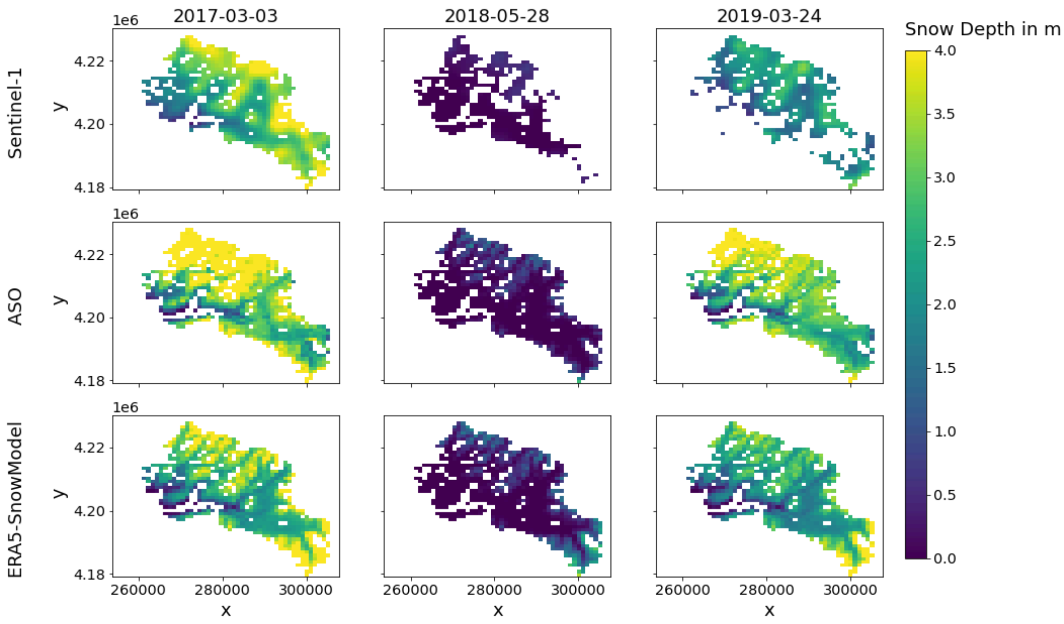


Figure 7 Line 47 in caption: recommend data "are" instead of "is".

ok

Line 01 in the Discussion. The phrase "the ASO program has shown that useful SWE products can be derived from remotely sensed snow depth" needs a caveat, in that the ASO model of snow density is adjusted based on in situ measurements of snow density.

We agree with this relevant comment. We changed the sentence to "Although we are interested in SWE and not snow depth, the ASO program has shown that useful SWE products can be derived from remotely sensed snow depth when combined with in situ measurements and modeling of snow density"

Line 21-22 in the Discussion. Perhaps cite the Liu et al. (2022) analysis here?

We agree the Liu et al. (2022) analysis should be cited. We added the citation in the introduction : *However, reanalyses cannot be used directly to force a mountain snowpack model because the grid cell size is too coarse (approximately 30 - 50 kilometers for ERA5 and MERRA-2 respectively), which creates large biases in the computed SWE (Wrzesien et al., 2019; Liu et al.,2022).*

Line 21-22 in the Discussion refers to the meteorological forcings of ERA5 used in the study (while Liu et al. (2022) focused on direct SWE products from the reanalyses). We rephrased with : *Another limitation is the fact that ERA5 meteorological forcings may not be homogeneous across the globe due to the uneven distribution of the assimilated observations.*

I agree with the final paragraph of the Discussion. The combination of ERA5, SnowModel, and Sentinel-1 provides a way to analyze the snowpack in mountains with only an austere infrastructure. There are uncertainties of course, but the methods could provide some information in areas where few data exist.

Support for Open Science: The manuscript should identify the sources of data and code availability used in the analyses. I could do my own searches, but statements like “from the Copernicus Climate Change Service (C3S) and can be queried via their application programming interface” (Line 92) could be phrased more helpfully.

We added a reference to the tutorials of the Climate Change Service on how to retrieve the data: (Retrieving data — Climate Data Store Toolbox 1.1.5 documentation, 2024)

Similarly, the citation to “Copernicus Digital Elevation Model, 2023” (Line 96) is not in the bibliography.

The reference is actually the second item of the bibliography.

Some information is missing about the “code availability section” mentioned on Line 45.

We added a code availability section at the end of the paper.

References

Retrieving data — Climate Data Store Toolbox 1.1.5 documentation: https://cds.climate.copernicus.eu/toolbox/doc/how-to/1_how_to_retrieve_data/1_how_to_retrieve_data.html, last access: 27 June 2024.

Liston, G. E. and Elder, K.: A distributed snow-evolution modeling system (SnowModel), J. Hydrometeorol. 76 1259-1276, 2006.

Liu, Y., Fang, Y., Li, D., and Margulis, S. A.: How Well do Global Snow Products Characterize Snow Storage in High Mountain Asia?, Geophys. Res. Lett., 49, e2022GL100082, https://doi.org/10.1029/2022GL100082, 2022.

Painter, T. H., Berisford, D. F., Boardman, J. W., Bormann, K. J., Deems, J. S., Gehrke, F., Hedrick, A., Joyce, M., Laidlaw, R., Marks, D., Mattmann, C., McGurk, B., Ramirez, P., Richardson, M., Skiles, S. M., Seidel, F. C., and Winstral, A.: The Airborne Snow Observatory: Fusion of scanning lidar, imaging spectrometer, and physically-based modeling for mapping snow water equivalent and snow albedo, Remote Sens. Environ., 184, 139–152, https://doi.org/10.1016/j.rse.2016.06.018, 2016.

Painter, T. H., Deems, J. S., Belnap, J., Hamlet, A. F., Landry, C. C., and Udall, B.: Response of Colorado River runoff to dust radiative forcing in snow, Proceedings of the National Academy of Sciences, 107, 17125-17130, https://doi.org/10.1073/pnas.0913139107, 2010.

Anonymous Referee 2

General comments

The authors present a comprehensive evaluation of high-resolution snowpack simulations forced with globally available datasets, in particular coarse resolution meteorological data downscaled to the model grid. Thus, the study showcases a generic tool for performing snow cover simulations in any region of the world efficiently and with low effort. The simulations presented in the study, performed for the Tuolumne River catchment (Sierra Nevada, USA), were evaluated against high-resolution snow water equivalent (SWE) data derived from Lidar measurements of snow depth and modelled bulk snow densities. The simulations show promising results with comparable performance as satellite-derived snow characteristics for the study basin. In contrast to the remote sensing observations, the snow model results are always available, which is a significant advantage over the occasional satellite retrievals.

Overall, appropriate methods are used in the study and the results are relevant and promising. However, the presentation and discussion of the results sometimes lacks clarity and depth in my opinion. The description of the results deserves a few more details, whereas the discussion requires stronger links to the results themselves (foremost by including more references to specific figures). Furthermore, the paper should likely also be improved language-wise, preferably by a native English speaker. In spite of the shortcoming listed above, the paper is pleasant to read, contains a wealth of interesting results and is a valuable contribution to the snow modelling community. Detailed comments are listed below.

We thank the reviewer for the careful evaluation of our work. We appreciate the positive comments and relevant suggestions. We implemented every suggestion in the revised manuscript as detailed below.

Specific comments

Page 1, line 13: Consider changing “sourcing” to using and “climate” to “meteorology”.

ok

Page 1, line 18: Change from “snow depth to Sentinel-1 snow depth retrievals” to “snow depth to Sentinel-1 retrievals”.

ok

Page 1, abstract: The concluding sentence of the abstract should be improved. One option would be to add a sentence stating directly that the snow model provides results anywhere at anytime in contrast to satellite retrievals.

We reformulated the last sentence:

However, Sentinel-1 snow depth products are sparse and often masked during the melt season, whereas ERA5-SnowModel provides spatially and temporally continuous SWE.

Page 2, line 34: Please also cite Lievens et al. (2022) and adapt the sentence accordingly.

ok

Page 2, line 46: Include the missing "have": "There reanalyses have also…"

ok

Page 2, lines 59-60: The sentence "However, the evaluation of these simulations relied on sparse in situ observations or MODIS snow cover area" seems incomplete. What is the drawback with these observations and why are more studies needed? Is it the coarse resolution of MODIS snow covered area?

Our intention was to highlight that these data do not allow to validate the spatial distribution of the snow depth or SWE across the landscape.

We reformulated the paragraph and added the following sentence:

However, in situ data are sparse and MODIS snow cover area does not allow a thorough evaluation of the model ability to capture snow mass across the landscape

Page 3, lines 68-79: Consider adding the spatial resolution of the model simulations already here.

ok

Page 5, lines 00-01: Please mention the physical reason why the satellite retrievals do not provide data during the snowmelt period and add a reference supporting the statement.

We added the following sentence and references:

When the snowpack is wet, there is a larger absorption and reflection of the microwave signal emitted by Sentinel-1 which greatly decreases the performances of the C-SNOW algorithm (Lievens et al., 2019; Tsai et al., 2019).

Page 5, line 06: Important, the statement "…50 m SWE is less than 0.01 m w.e" needs a reference.

We agree and added the following paragraph:

The reported accuracy on the 3 m snow depth products is 0.08 m (Painter et al., 2016) and from spatially intensive sampling, the reported accuracy for the 50m snow depth products is < 0.01 m (Painter et al., 2016, Figure 15). There are no published references for the 50 m SWE product. However, for a 1m deep snowpack and a conservative 10% uncertainty in snow density (20-50 kg/m3), we estimate the uncertainty of the 50m SWE products to be 0.02 - 0.05 m w.e

Page 5, line 15: What is "grassland rangeland"?

It is the SnowModel class name for herbaceous vegetation (graminoids and forbs).

Page 7, line 40: Consider changing from “Appendix Table A1” to “see Table A1 in appendix”.

ok

Page 7, line 58: Consider changing to “very coarse resolution of approximately 31 and 9 km (Fig. 1 and 2)”.

ok

Page 7, lines 62-63: Consider changing to “…the snow depths given by ASO, Sentinel-1, and ERA-SnowModel were…”.

ok

Page 8, lines 65-66: Please reformulate these two sentences. The second sentence needs to reference the first, otherwise it is not clear for what the performance metrics were computed.

We reformulated : *We computed the distributed residuals by subtracting the ASO snow depth from both SnowModel simulations and Sentinel-1 data. We averaged the residuals to compute the bias for each date. We also computed the standard deviation of the error and the RMSE over the catchment for each date .*

Page 8, line 76-78: Please reformulate the sentence. It is too long and hard to read.

ok

Figure 3: Consider using dashed lines for ERA5 and ERA5-Land.

ok

Page 9, lines 84-85: It is likely not needed to describe the lines here since this information is already provided in the legend of the figure.

ok

Page 9, line 89: The sentence “Considering the entire simulation period, 10% of the cells have an RMSE above 0.5 m w.e.” seems somewhat misplaced and is hard to understand.

This is the transition between the catchment scale analysis to the pixel scale analysis.. It is rephrased in the revised manuscript with : *We computed a map of RMSE using all the 49 validation dates we have between 2013 and 2019. 10% of the cells in this map have a RMSE above 0.5 m w.e*

Page 10, lines 1-2: Why were these two dates selected for the analysis?

We clarified this point in the revised manuscript as follows:

We aimed to distinguish the model performance in terms of accumulation and ablation processes to better separate the sources of uncertainties in future studies. Therefore we *selected a date before the melting season (April 01) and a date near the end of the melting season (May 27).*

Figure 5, caption: Why is the second date not mentioned in the caption?

This was an oversight. Now corrected.

Page 11, line 14: Is “mean residuals” the same as bias?

yes it is. Now rephrased with “*mean of residuals (bias)*” in the revised manuscript.

Page 11, line 25: Consider changing to “…resolution using upscaled ASO…”.

ok

Page 11, lines 28-29: What does “these missing values are propagated at 1 km resolution” mean?

We resample the ASO products by averaging all pixels inside a square cell of 1 km. If there is at least one missing value among the contributing pixels, a missing value is attributed to the target 1 km cell. This was specified in the revised manuscript.

Page 11, line 30: Is not the exact area used between the methods or the dates, or both?

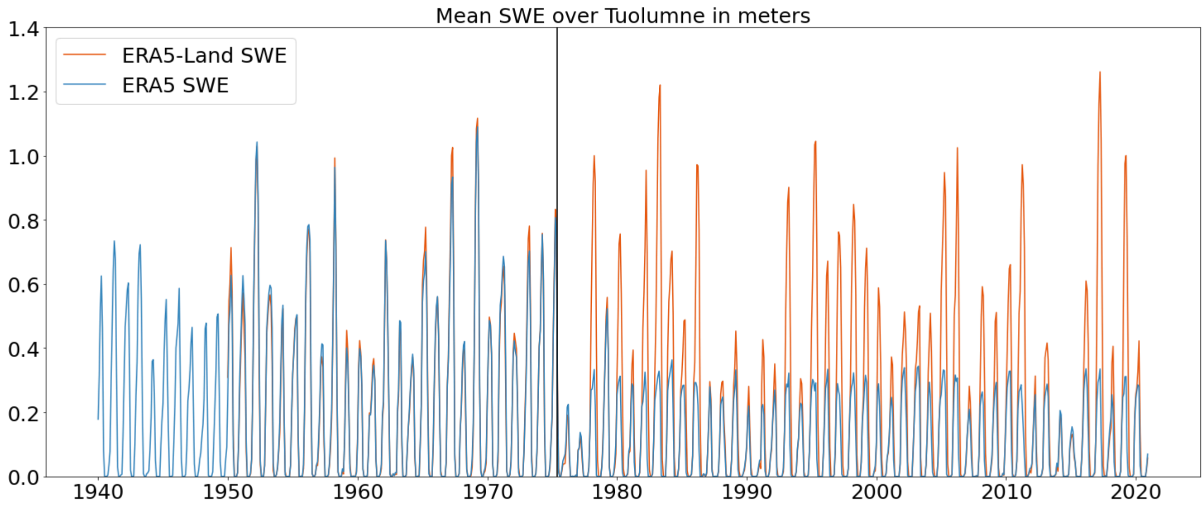
Both : all the pixels shown in figure 6 are taken into account in Table 1. For ERA5-SnowModel, the mask is the same for the three dates because the missing values are due to the missing values in the ASO data. With Sentinel-1, the missing values are due to i) the missing values in the ASO and ii) the missing values in the Sentinel-1 algorithm. The second one are time dependent and therefore the statistics in Table 1 are not computed on the same area from one date to another, nor on the same area as ERA5-SnowModel.

Figure 7: Consider merging Table 7 into this figure by including texts with the statistics. For an example of what I propose, see Figure 5 in Fontrodona-Bach et al. (2023). The scatter plots could potentially also be improved by showing the scatter density, just like the left panels in the Figure 5 by Fontrodona-Bach et al. (2023).

We merged Table 1 and Figure 7 as suggested. However, the numbers of points were not sufficient to make nice density plots (2D histograms). It added unnecessary information (colorbar) and decreased the readability of the plots.

Page 13, line 53: What discontinuities in ERA5 SWE? Are these visible in Figure 3?

They are not visible in Figure 3. There are some discontinuities in the ERA5 SWE appearing in 1976 due to the implementation of new snow depth products into the ERA5 assimilation scheme. When these products are assimilated, ERA5 caps the snow depth data at 1.4 m to avoid an overestimation of the snow depth (personal communication from Patricia de Rosnay, ECMWF). This creates a strong discontinuity in the ERA5 snow time series (see figure below). Because the meteorological forcings would not be impacted by this threshold on snow, using this pipeline could be a way to bypass this discontinuity. However, other meteorological variables in ERA5 might also be affected by the growing number of data assimilated (Bengtsson et al., 2004).



Page 14, line 58-59: Please improve the language of the sentence "We find an overestimation of snow accumulation in high elevation however which occurs only above 3000 m asl".

We reformulated:

We find an overestimation of snow accumulation at high elevations, specifically occurring above 3000 m asl.

Page 14, lines 66-67: Avalanches move snow from higher to lower altitudes but does not reduce snow amounts. Please rephrase the sentence.

High elevation and steep slopes are prone to avalanches thereby reducing the accumulated snow in these areas during the winter season (Quéno et al., 2023)

Page 14, lines 75-77: Please refer to Figure 5.

ok

Overall, as mentioned in the general comments, provide more links in the discussion to results by adding appropriate cross-references to figures and tables.

We followed this suggestion in the revised manuscript : *This result is in line with Muñoz-Sabater et al. (2021) who find better performances of ERA5-Land than ERA5 between 1500 m and 3000 m a.s.l. because 68% of the Tuolumne River catchment is in this elevation band.*

Page 15, lines 91-93: The sentence is formulated awkwardly. What does "carries 68 % of the Tuolumne River catchment" mean?

It meant that 68% of the catchment has an elevation between 1500 m and 3000 m. Rephrased in the new manuscript

Page 15, lines 1-2: This statement requires at least one reference.

We added this reference to the sentence in the manuscript : (Margulis et al., 2019)

Page 15, line 6: What is hard to understand about the error patterns of Sentinel-1 compared to the other methods?

Figure 7 shows that Sentinel-1 snow depth dataset seems to represent quite accurately the spatial variability inside the catchment, although we note a slight underestimation for all three dates before the melting period (2017 and 2019) and after it (2018). There is no clear pattern in the errors that emerge from these three dates.The modeling approach with ERA-5 (Land) and SnowModel yields similar performances in terms of snow depth as the C-SNOW product on the same dates. However, two patterns appear on Figure 7 for these approaches. i) The simulations with ERA5 and SnowModel are mostly centered around a negative bias constant with the observed snow depth before the melting period (2017 and 2019), probably representing a small negative bias in the ERA5 precipitation. ii) The simulations with ERA5-Land SnowModel seem to cap at 4 m which could be the result of the two consecutives downscaling in the precipitations : the combination of an underestimation of ERA5 precipitation and its downscaling, plus the limitation of the elevation difference between ERA5-Land stations and the DEM so the MicroMet precipitation factor can not enhance enough the high resolution precipitations

Page 15, lines 11-14: What has the first part of the sentence about errors has to do with the second part about model differences? Please split this sentence into two, and improve the language.

There are different error sources in the three methods which are neither insignificant nor prohibitive for an operational use. The key difference is that the model provides temporally continuous SWE, snow depth and other relevant variables like snowmelt runoff, whereas C-SNOW snow depth products are temporally sparse and often masked during the melt season.

Page 16, lines 34-35: Consider providing a short description for each components of this tool since many readers start by reading the conclusions of a paper.

It uses SnowModel/MicroMet to downscale meteorological variables from ERA5 before computing accumulation and ablation processes using other SnowModel submodels.

Page 16, line 38: What does the “0.08 m” refer to?

Indeed, this was not clear, we will reformulate as follows:

Based on 49 reference SWE surveys spanning seven contrasted hydrological years, we find that the ERA5-SnowModel combination simulates well the SWE at the scale of the Tuolumne river catchment, with RMSE of 0.06 m (and 0.08 m with ERA5-Land) and correlation of 0.99 (with both datasets)

Page 16, lines 34-43: Example of paragraph that likely needs language improvements.

Technical comments

Page 3, line 70: Misplaced white space in 50 m.

ok

Page 7, line 39: Missing whitespace.

ok

Page 7, line 56: Missing comma after additionally.

ok

Page 15, line 92 and 93: Wrong reference format.

ok

References

Fontrodona-Bach, A., Schaefli, B., Woods, R., Teuling, A. J., & Larsen, J. R. (2023). NH-SWE: Northern Hemisphere Snow Water Equivalent dataset based on in situ snow depth time series. Earth Syst. Sci. Data, 15(6), 2577-2599. https://doi.org/10.5194/essd-15-2577-2023

Bengtsson, L., Hagemann, S., and Hodges, K. I.: Can climate trends be calculated from reanalysis data?, J. Geophys. Res. Atmospheres, 109, https://doi.org/10.1029/2004JD004536, 2004.

Lievens, H., Demuzere, M., Marshall, H.-P., Reichle, R. H., Brucker, L., Brangers, I., de Rosnay, P., Dumont, M., Girotto, M., Immerzeel, W. W., Jonas, T., Kim, E. J., Koch, I., Marty, C., Saloranta, T., Schöber, J., and De Lannoy, G. J. M.: Snow depth variability in the Northern Hemisphere mountains observed from space, Nat. Commun., 10, 4629, https://doi.org/10.1038/s41467-019-12566-y, 2019.

Lievens, H., Brangers, I., Marshall, H. P., Jonas, T., Olefs, M., & De Lannoy, G. (2022). Sentinel-1 snow depth retrieval at sub-kilometer resolution over the European Alps. Cryosphere, 16(1), 159-177. https://doi.org/10.5194/tc-16-159-2022

Margulis, S. A., Fang, Y., Li, D., Lettenmaier, D. P., and Andreadis, K.: The Utility of

Infrequent Snow Depth Images for Deriving Continuous Space-Time Estimates of Seasonal Snow Water Equivalent, Geophys. Res. Lett., 46, 5331–5340, https://doi.org/10.1029/2019GL082507, 2019.

Tsai, Y.-L. S., Dietz, A., Oppelt, N., and Kuenzer, C.: Remote Sensing of Snow Cover Using Spaceborne SAR: A Review, Remote Sens., 11, 1456, https://doi.org/10.3390/rs11121456, 2019.