

The Cryosphere Discuss., referee comment RC3  
<https://doi.org/10.5194/tc-2022-146-RC3>, 2022  
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## Comment on tc-2022-146

Anonymous Referee #3

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Referee comment on "Exploring the use of multi-source high-resolution satellite data for snow water equivalent reconstruction over mountainous catchments" by Valentina Premier et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2022-146-RC3>, 2022

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This paper presents a reconstruction method to produce time series of high-resolution (HR) snow water equivalent (SWE) maps from different sources of remotely sensed data, with specific focus on the applicability on mountain areas. The method's description and results in two selected catchments are discussed, and the potential sources of errors are addressed in the context of general applicability and future improvements.

The topic is relevant for the scientific community in snow-dominated areas due to the lack of HR mapping on a time scale that allows monitoring of quick changes in the snowpack extension and mass change, and the scarcity of direct SWE measurements and methods to provide a dense network of monitoring stations for spatial interpolation approaches. The innovation and relevance of these objectives are sufficiently addressed in the manuscript. However, in its current version, some issues are found that require further assessment before considering further review and potential publication in this journal. Please, find below these major items. I hope that these comments are useful to improve the work and help to further comprehend its context and applicability further than the present results.

- The Introduction section contains good points but requires some structure to get more focused on the specific goals' context. I would also recommend to present this earlier in the narrative. Lines 100-108 can be easily moved/merged to/with section 2 for the sake of clarity.
- The general objective should be better elaborated in line 99, i.e. not only state what but also what for and some specific scope. For example, the target type of catchment is relevant but it is not declared until lines 130-131 that size is limiting the potential further applicability of the method. Moreover, the order of magnitude of "a not too vast catchment" must be assessed.

- Section 2.1 is determinant in the methodological approach. In the explanation, it is not clear whether the catchment state is identified for each pixel or for the whole catchment area; this needs a revision to be clear throughout the text. Moreover, the spatial definition of the “total delta-SWE” is missing, which is required, and additionally the use of this variable should be uniform for the three states (i.e. is also total in line 149?). In line 149, I am not sure about the meaning of “no changes WITHIN the catchment”, do you mean really that or rather no change when considered as a whole?
- I have doubts on the simplification done on the potential combinations of positive/zero/negative values of delta-SWE and delta-SCA in this section. First, it seems that both variables have different spatial definitions, since pixel changes in terms of SCA are assessed. Additionally, some situations are discarded, for example, accumulation is not allowed to happen with negative delta-SCA values, but this is not infrequent in mountain areas in some regions in the world. Other situations are not included in the three potential states. In general, the assumptions are difficult to be validated in semiarid regions with snow relevance or during patchy snow periods in steep slopes, especially if the catchment state is defined uniformly in space. These issues should have been assessed and their discarding justified or at least clarified in terms of the applicability of the method.
- In section 2.3, two issues require further assessment. First, the use of day-degree modelling for melting rates’ estimation is not the best choice if accurate HR maps are the goal, in my opinion. At least, some justification of this apparent lack of coherence should be included, together with the comparison of the error of SWE estimation associated to the use of such methods and the error from low resolution satellite products. Secondly, the adoption of the temperature threshold is one of the major sources of error in the SWE estimation in mountain areas, as many works have already shown; so, the selected value needs some justification. Thirdly, and more relevant, lines 280-283 involve that melting is the only process in the ablation of the snowpack, which means that sublimation is neglected (but nothing is said on this); this may result in non-negligible loss of mass in the closure of the balance equation, and it is a constraint for the applicability of the method in some regions or during some periods/under some atmospheric conditions. This must be addressed in the description of the methodological assumptions and their validity. Finally, some comments on the scale effects from the subdaily evolution, not operating in the method, should be included.
- In section 4, the results are shown as selected points/transect/ periods in the study catchments, and detailed datasets are included as appendixes. The selection must be justified in all cases. The associated figures and tables’ captions must include the catchment name in all cases (see figures 5 to 7, and table 1). Some sentences lack a proper justification, for example, lines 389 and 399 contain comments that can’t be rigorously concluded in general from what has been shown. Or line 408, regarding Fig. 13, has a mass balance closure test been done? Figure 9 caption, are these “trends”?
- The discussion in section 5 repeats many facts or comments that have been previously presented or commented. Moreover, the discussion is focused on the sources of error at each step of the proposed method. I miss the discussion on the goodness of the results when compared to other products/methods/data sources that provide less resolution, or other standard or alternative existing methods. This is important as HR SWE mapping is the target goal.
- The error indicators in results cannot be properly valued since little information is included from the study catchment in terms of SWE regime, in section 3.
- The discussion/conclusions should also include more reference to what processes can be tracked from the time series obtained of these SWE maps, and what cannot due to the assumptions, etcetera in the approach. This is very relevant to address the further applicability of the method.

Some additional comments:

- In general, the English usage and edition is good, but some revision is recommended.
- Please, review the use of some wording. For example, line 381, "while the others (seasons) are drier" really means snow-scarce, which can also be due to high temperature; or the use of "bias" in the work to define "difference" or absolute error.
  
- When some references are included in a list, please, use a constant criteria to order (increasing or decreasing date).
  
- Reference in line 35 looks not recent enough to be a updated review for remote sensing products, at least, some others could have been included.
- Line 65, please provide some reference, there are works on that (i.e. Pimentel et al., 2015;2017; or others).
- Please, assess the error associated to the ASO product, taken as ground-truth to test the results.
  
- Beyond the comparison of results and ASO in the appendixes, dispersion graphs are needed to further assess the performance of the method, and some selected cases should be included in the results' section.