

Interactive comment on “Efficient multi-objective calibration and uncertainty analysis of distributed snow simulations in rugged alpine terrain” by James M. Thornton et al.

Anonymous Referee #1

Received and published: 26 October 2019

This paper presents an uncertainty analysis of snow simulations with the model WaSiM in a mountainous region in western Switzerland using multi-objective pre- and post-calibration methods including the usage of Landsat 8 images. The paper is well-written and the authors took especially great care in producing good and understandable figures as well as supplementary material. Moreover, it seems that the authors have a comprehensive overview on literature close to their study. However, on the one side as mentioned by the authors themselves, the absence of ‘real’ continuous snow measurements within the study site is a clear limitation of this study, but on the other side the authors try the best in including ‘reconstructed snow measurements’. I have some general and some specific points, which should be considered carefully before publi-

C1

cation.

General: - The results should also be separated in snow accumulation and snow ablation phase (as you shortly mentioned the two phases on p.38, l.819 and in the abstract). I guess there would be then distinct differences in your calibration. This should be presented and discussed.

- In my opinion, the results and discussion part are not a clearly separated. In both sections, points of results and discussion can be found. Maybe you merge these two sections or make a clearer separation into results and discussion.

- It comes far too late in the manuscript that you are using the model WaSiM. This should already be mentioned in the abstract and the introduction.

- Snowpack simulations cannot be described as ‘actual’ measurements!

- Just a very general remark: Why don’t you use additionally Alpine3D and compare it to your results?

- The paper is very extensive and long. Some passage are only descriptions and repetitions of other literature. I would suggest to shorten (in total up to 3 pages) several points especially in the sections introduction, data and methods and to focus more on the relevant points of your work.

- It is irrelevant which type of processing software (e.g. R) you used. . .

Specific:

- p.6, l.147: addressed with ‘d’

- p.6, l.148: stations without ‘s’

- p.6, Please insert which snow model you used. I guess you are talking about WaSiM? But can this really be described as a snow model?

- p.8, Figure 1: You mentioned the two headwater sheds Vallon de Nant and Vallon de

C2

La Vare – please mark them in Figure 1.

- p.9, Section 3: In the section 'Data' you also describe the processing / generation of the data. Please consider renaming this section accordingly.
- p.9, Section 3.1: Start by describing which input the model needs. The information is just somewhere in the following text.
- p.10, Eq.1: please insert a reference for Eq. 1
- Section 3.2: Here information on the applied snowpack model (I guess SNOWPACK (Lehning et al. 2002) is missing. Which were the input data for the SNOWPACK simulations? Actually, you need specific meteorological input data (and not necessarily snow measurements). Why couldn't you use the SNOWPACK simulations at the meteorological stations within your catchment? Please also think about using Alpine 3D (Lehning et al. 2006). And, if you used snow measurement data from instruments at the higher station, please specify, which snow sensors were used.
- p.11, l.261: '. . .gradients (with 's')
- p.13, Figure 2: In my opinion this figure is not needed. Anyway, in Fig. 2b, the classes were not rounded to e.g. 2 digests and the classes seem to be a bit random; in Fig. 2c, the legend does not correspond to the image (no snow should be white) and to the colours in Fig. 3.
- p.11, l. 282: Why did you exclude all cloud covered Landsat 8 data. Maybe there were some useful images, which were only partly cloud-covered. It should be discussed why you prefer using only cloud-free data whilst allowing a lesser spatial resolution in potential available images.
- p.15, l.338f: IDAWEB? IMIS? Not clear to all readers please describe acronyms.
- p.15, l.345ff: Which parameters for the station was used by applying Jonas et al (2009)?

C3

- p.15, l.354: I strongly disagree that these are actually direct snow measurements. This has to be corrected.
- Section 3.3: What is the temporal resolution of the streamflow data?
- p.16, l.362: Define 'rating curve construction'.
- Section 4: As you actually describe some methods also in Section 3, I would suggest to rename this chapter and relate it especially to your modelling, calibration and uncertainty estimation 'work' with WaSiM.
- p.16/l.365: without 'to'
- p.19, Table 2: This table is not necessary and can be replaced by one sentence in the text. - p.20, l.486ff: Is there a reference for the chosen weighting? Or why did you choose the weighting of 60:40?
- p.22, l.537: 'were also developed': I suggest to write 'were also calculated'
- Section 5: Please rethink your subchapter captions and a general merge with Section 6.
- p.26, l.603ff: For example, this point belong more to the discussion. . .
- p.30, Figure 7a: Please explain more clearly what is compared and shown in this graph.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-181>, 2019.

C4