

Interactive comment on “Obtaining sub-daily new snow density from automated measurements in high mountain regions” by Kay Helfricht et al.

Kay Helfricht et al.

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We would like to thank the anonymous referee for the time invested in reviewing our manuscript, and for the positive and constructive feedback. We will revise the manuscript with special focus on the conclusiveness of names and abbreviations and rephrase the sections mentioned by the referee. The presentation and structure of the manuscript will be revised substantially.

We kindly forward the decision regarding the inclusion of all additional figures in the supplement to the editor.

1. The manuscript neglects spatial variability in between snow depth and SWE measurements. Although the authors discuss errors arising from the two measurements,

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there might be (and certainly is at WFJ) a spatial distance between the point measurement of snow depth and the more spatially integrating observation above snow pillows. Schmid et al. (2014 - doi: 10.3189/2014JoG13J084) found a small scale heterogeneity in HS of at least 4% at WFJ. In SWE, they observed an uncertainty of $\pm 5\%$ for all available measurements. It remains questionable what the Golden Standard is, however an uncertainty of 5% may exist. For this manuscript, just relative changes are being used, which might reduce errors due to spatial variability. However, such uncertainty has to be included in the discussion of the results. Especially, since all of your validation data arise from the assumption that both, the ultrasonic transducer and the pillow, measure exactly the same occurrences.

Uncertainties from observations and snow cover variability will be addressed in more detail in a revised version of the manuscript.

2. Another major part preventing the manuscript from publication at the current state is the presentation of the paper. First of all, the manuscript is far too long. You certainly don't make efficient use of the journal's space in relation to the information you provide. Rewriting your manuscript can reduce the number of pages by approx. 50%. Right now you provide large amounts of redundancy and not supportive information, for instance: P3 2nd paragraph bridging effects do not need to be introduced and explained here. Just cite a respective publication e.g. Johnson and Schaefer. 2002 – doi: 10.1002/hyp.1236

The manuscript will be shortened and redundant information will be deleted.

P3 2nd paragraph bridging effects do not need to be introduced and explained here. Just cite a respective publication e.g. Johnson and Schaefer. 2002 – doi:10.1002/hyp.1236

Will be revised.

P2 2nd paragraph – here you don't need to provide a review on snow crystal growth in

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the atmosphere.

Will be revised.

P4 down to L30 has to be shortened significantly

Will be considered.

P5 L5-26 and L27-31 provide redundant information with two Tables

Will be shortened.

P6 L3-8 Please shorten and refer to Olefs et al. (2010). No need for repetition of all the details.

Will be shortened.

Are you entirely sure that you need all Figures presented in the manuscript and the supplementary? Isn't it more useful to present quantities in a Tab? Especially since you only include Kuehroint within the MS. All data from Fig. 5 and corresponding Figs in the supplement can easily be concluded in a single table using maybe the coefficient of variation as measure of distribution instead box plots.

From our point of view, the additional figures in the supplementary materials complete the in-depth presentation of the analysis. The Kuehroint figure was included as an example, with a reference to the additional figures for interested readers. It also highlights the soundness of the data filtering and the unequal distribution of NSD, seeing as not only mean and standard deviation but also median and quartiles are been presented. We defer to the editor on the decision whether or not to include the additional figures.

Fig. 6 (+ similar suppl.) and Tab 3 are redundant; same for Figs. 9,10 and Tab. 5.

We think that not all numbers presented in Tab. 3 and Tab. 5 can be estimated from the corresponding figures, in particular the correlation coefficient and the coefficient of determination. Conversely, getting a general overview from the tables only is difficult.

The authors are in favour of presenting results in a way that appeals to readers who prefer visual information as well as those who prefer data tables and would prefer to include both figures and tables. However, we will reconsider the presentation of the data mentioned by the reviewer and aim for a more concise solution.

The Discussion section is far too long and extensive.

Will be shortened.

3. The structure of the MS is not acceptable. In results you interpret the presented data i.e. P8 for numerous times, P9 L15-30, P10 L4-10 etc.. In Discussion, you do present results: P11 L29-39 and kind of introduce the topic P12 L.16ff. I suggest combining Results and Discussion in one section.

The general structure of the manuscript will be revised substantially.

4. The presentation of equations is unacceptable as well. Please read the guidelines provided for this journal and follow them. I will certainly reject a revised version of this MS if equations remain unreadable. Multi-letter variables are not supportive in equations and according to the guidelines “should be avoided”. Even worse are variables like SD_{HN} with a subscript t. For preparation of a manuscript it is not adequate to copy and paste equations from scripts.

Our apologies for not considering equation guidelines in the discussion manuscript. This will be revised in the next version.

5. It appeared - at least to me - that the usage of the term “threshold” is very misleading/wrong. In my opinion, for the first time, it is correctly used within the MS on P8 L21.

We will reconsider the use of the term “threshold”.

Please explain Fig. 2 more in detail. So far, the reader gets no idea what you are intending to present with these plots.

The presentation of Fig. 2 will be clarified.

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6. The presentation of the Figs. should be improved as well. It is inadequate to use left, middle, second from right for the description of subplots. Please use letters or similar to differentiate plots.

Will be revised.

7. Phrase like. . . are obvious . . . in a statistically vague manner. . . should not appear in a conclusion. Either quantify or describe that no statistical relations can be found. You often use imprecise wording to describe coherences.

Will be revised.

Some more minor points which have to be revised before publication: - WFJ is not located at the N “fringe” of the Alps and in your comparison it is actually the most southern site. As a consequence, I do not accept the argument presented on P8 L7ff, which again should be part of the Discussion instead of Results.

Will be clarified.

- P8 L26ff, this is very confusing! You observe a data reduction to only 6% remaining at WFJ and to only 5% at Kuehtai. However, WFJ has the highest filtering rate, please clarify and probably rewrite emphasizing more on the periods to facilitate understanding.

Will be clarified.

- Be CONSISTENT! Apart from the equations the whole MS appears to be not thoroughly reviewed before submission, i.e. snowpack vs snow pack, Kuehtai, Kuehrint in at least 3 different writings. . .

The manuscript will be revised with focus on conclusiveness of names and abbreviations.

- P6 L17ff you vary “thresholds” by values below the resolution limits of the instruments. I do not consider this as a threshold nor do I think that such increments are actually

useful.

Even when the resolution of the instruments is of a similar magnitude, the measured values have a higher resolution of at least one order of magnitude. Fig. 2 is useful to present the influence of choosing reasonable lower limits / thresholds.

- The number 4 does not have to be introduced (P5 L4)

Will be deleted.

- Snow pillows actually do not measure SWE. They weigh the overlaying mass and allow for derivation of SWE

Will be rephrased.

- Weight cannot settle P6 L27

Will be rephrased.

- P6 L19 described in Anderson. . .

Will be revised.

- Fig 7 is referenced before Fig. 4 etc

Will be revised.

- What is “lateral bonding” P10 L39?

This is related to bridging effects and will be rephrased.

- . . . filtered OUT. . . P11 L13; . . .more wind influenced stations. . . P11 L41

Interactive

Will be revised.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-579>, 2017.