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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #2

Received and published: 16 November 2017

The manuscript "Obtaining sub-daily new snow density from automate measurements in high mountain regions" by Helfricht et al. presents a valuable data set for new snow densities at four Alpine sites with automatic weather stations (AWS). The authors determine new snow density from the combination of observed changes of ultrasonic transducers and obtained increases in mass measured by snow pillows. They filter the data for several possible sources of errors. The data processing and filtering is adequate and offers an interesting approach to determine new snow density values automatically. The presented results are of high interest to the scientific community and are worth publishing. However, significant changes have to be made before acceptance for publication is possible. Hence, most of my major points of criticism are related to the presentation of this manuscript, I ask for a more detailed description of

the uncertainty of the used measurements. For instance, in P10 L40 you describe the uncertainty of your method to be at +-25 kg/m3. The approach by Lehning et al. has a RMSE far below this uncertainty. So how can you argue for deviations of model parameterizations while the given uncertainty of the presented derivation of new snow density is larger? Please restructure the MS while more focusing on the named aims of the paper, include subchapters.

Other major points of criticism:

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

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

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

P4 down to L30 has to be shortened significantly

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

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

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.

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

The Discussion section is far too long and extensive.

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.

4. The presentation of equations is inacceptable 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.

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.

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

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.

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.

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.

- 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.

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

- 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.

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

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

- Weight cannot settle P6 L27

- P6 L19 described in Anderson. . .

- Fig 7 is referenced before Fig. 4 etc

- What is “lateral bonding” P10 L39?

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

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