Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-152-AC2, 2020

Hydrology and Earth System Sciences Discussions
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Interactive comment on "Snow Water Equivalents exclusively from Snow Heights and their temporal Changes: The $\Delta_{\mathrm{SNOW.MODEL}}$" *by* Michael Winkler et al.

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Received and published: 5 June 2020

Many thanks to Anonymous Referee #2 (AR2) for the praise and the recommendations to improve the paper. We will comment on them in the following.

(1) Thanks for describing this source of uncertainty, which we were not really aware of. However, for both datasets used in the study we cannot really allow for this source of uncertainty. For the Austrian measurements the respective snow depth values from the SWE observations coincide with the associated value from the daily snow depths record. Swiss data (Marty2017, https://www.envidat.ch//metadata/gcos-swe-data) are provided as biweekly SWE observations. Only those daily snow depth measurements

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coinciding with the biweekly values were used. This is already described in the paper at L387ff. However, we will add a comment concerning this issue and name the omitted/accepted records.
(2) Thank you, you are right. We will add a note, that SWE_{pk} is defined as the highest *measured* value, but not necessarily the maximum SWE that occurred in a certain season.
(3) We will add information on how errors differ for shallower and deeper snow packs, perhaps for classes if there are enough readings to justify a statistic. We argue that absolute errors are more meaningful than relative numbers, which are extremely influenced by the denominator.
(4) Thank you for the references, we were not aware of the first one and will add parts of its results. As far as the second study is concerned we soon get into trouble not to compare the incomparable. The paper does not provide distinct numbers for *certain* models, but is a great (maybe the best) survey on how different models perform under different conditions/at different locations. We do not think that it is reasonable to compare our model to the mean bulk performance of other models, but we will add some numbers to the revised manuscript. As far as the third reference is concerned, we ignored it in favor of the more optimistic (from the SNOWPACK point of view) study of Langlois2009. We will add more numbers in the revised version.

Specific comments:
L20: Changed as suggested. See also our answer to AR1's comment (AC1).
L21: SWE is an "areal density", not a (volumetric) density.
L69: See above. We will try to add results of the two suggested studies. We have not been aware of the second study. Thanks to AR2 for the notification.
L120: We will switch to snow depth throughout the paper. This will also change the title. Confer answer to AR1's comment (AC1).
L124: This will be considered in the text.
L159: We will switch to new snow throughout the paper.
L306: Changed to "model snowpack" in quotes.

L309: The value $450\,\mathrm{kg\,m^{-3}}$ was chosen arbitrarily as an example and is not connected to Rohrer and Braun (1991). By the way we are not aware of a publication "Rohrer and Brown, 1991", we guess this should be 1994.
L355: The phrase is literally taken from Sturm and Holmgren (1998) and is marked as such. However, it will be changed to "viscosity at [which] ρ equals zero" (see also comment AC1).
L364: The sentence will be changed to "The last two parameters, c_{ov} and k_{ov}, determine..."
L503: Following your comment and also one of AR3 we will subordinate this paragraph within this section and add a reference to point (1) above.
L506: This is of minor importance and will be omitted.
L576: An explanation of "diverse measurements" will be given in the revised manuscript.
L581: "now" will be omitted.
L582: All colors in Figs. 1 and 5 are the same, except for the "Scaling module". This is a black arrow in Fig. 1, whereas it is white with a black border in Fig. 5. We think that black bars in Fig. 5 would be distracting and therefore do not change.
L614: No, the jumpy behavior was detected, but does not play a role, because it is simply very small.
L664: Thanks for the hint to this unclear passage. Only mean snow depth of SNOWGRID is used as covariate in the spatial model. The smooth model uses SWE simulated with the ΔSNOW.MODEL. This will be changed accordingly.
L667: We will add a comment for the average reader.
L691: We will mention this restriction of the ΔSNOW.MODEL. The ΔSNOW.MODEL cannot allow for mass loss due to sublimation. We will remark that at proper location in the revised manuscript.
L708: We assume Leppänen(2018b) is meant to be changed. The suggested study is brand-new and was not finally published when the manuscript was submitted. Thanks to AR2's comment this now can be updated.

L732: As suggested, the sentence will be changed, but the word “gapless” will be avoided. See respective comment of AR3 and our answer AC3.
L768: We agree, this statement might be too strident. It will be changed to “Nevertheless, the synopsis of the ΔSNOW.MODEL and measured data gives hints on two important variables in modeling alpine snow: Typical mean density for new snow (24 h) might be better assumed below often used $100\,\mathrm{kg\,m^{-3}}$…”
The restrictions mentioned by AR2 will be added in the text. Regarding the first, see above at answer to L691. The second, the neglection of small snow fall events, will be outlined in the revised manuscript too. Still, including those minor events would even strengthen the arguments towards assuming new snow density below $100\,\mathrm{kg\,m^{-3}}$.

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