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Hydrology and Earth System Sciences
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Discussions

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

Anonymous Referee #2

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This is a remarkable piece of work. Congratulation! I liked reading the paper, which has a good structure and an easy to read language. The study is definitely worth to be published in HESS, after the following major points have been addressed:

1) One uncertainty in comparing measured SWE with parametrized SWE from a nearby snow depth measurement stems from the fact that the sum of the heights of the measured SWE samples does usually not correspond to the daily measured snow depth at a graduated pole. This can be due to e.g. to uneven ground or uneven snow distribution (see also chapter 2.4.1.2 in https://www.wmo.int/pages/prog/www/IMOP/publications/CIMO-

Guide/Prelim_2018_ed/8_cryo_2_en_MR.pdf). Moreover, since the SWE measurement is a destructive method the exact location throughout the winter is changing. For all these reasons the SWE measurements throughout the season and years should be referenced to fixed graduated pole of the daily snow depth measurement by deriving SWE from multiplication of the measured bulk density with the daily measured snow depth at the graduated pole. Please describe how you handled this problem?

2) How can you provide uncertainty statistics (in mass and timing) about SWEpk when your manually measured reference SWE is only measured weekly or bi-weekly. The manually measured SWEpk may have missed the real SWEpk?

3) Please also provide relative error measures. Only relative errors allow to compare results for shallow or deep snow packs.

4) The statement "hardly any numbers for SWE accuracy of thermodynamic models are available" demonstrates that the authors should have done a more thorough literature research. Therefore, please also include at least parts of the results of the following papers:

https://arc.lib.montana.edu/snow-science/objects/issw-2002-353-360.pdf

https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2008JD011063

https://www.the-cryosphere.net/9/2271/2015/tc-9-2271-2015.pdf

Specific comments:

20: I suggest to use the official abbreviation HS throughout the paper. See: Page 10 in https://unesdoc.unesco.org/ark:/48223/pf0000186462

21: why "areal" density. Use bulk density or density!

69: I suggest to add the results of the following two studies to table 2:

- doi:10.5194/tc-9-2271-2015

- Rasmus, S., Gronholm, T., Lehning, M., Validation of the SNOWPACK model in five different snow zones in Finland, 2007, Boreal Env. Res., 12(4), 467-488.

120: Either “depth” or “height”, I would suggest depth throughout the paper.

124: The approach of Martinec and Rango (1991) was introduced for SWEpk. Rohrer and Braun (1994) extended this approach for daily SWE (https://doi.org/10.2166/nh.1994.0020).

159: I suggest to change “fresh snow” to “new snow” throughout the paper according to Fierz et al. (2009).

306: “model snowpack”?

309: Where does the 450 kg/m3 come from? Rohrer and Braun (1991) already used 450 kg/m3.

355: “the viscosity at ïĄš equals zero”?

364: The last two parameters, cov and kov, determine...

503: See also general comment 1

506: “with filling height having the largest influence”? Please elaborate!

576: Please elaborate “diverse measurements”?

581: Omit “now”

582: Unfortunately, there are not the same colors used!

614: “does not play a role” = could not be detected?

664: It should be mentioned in the figure caption, that the gridded information is based on the model Snowgrid. Moreover, it is not all clear if the SWE values used for the spatial extreme model are directly from Snowgrid model output or converted form Snowgrid snow depth by the DeltaSnow.Model?

667: The average reader has no idea that 25 kN/m2 is much too high!

691: You may also mention that your approach only allows either snow fall or melt, but not both, although in reality this happens often. Moreover, what a about mass loss by sublimation?

708: I suggest to replace Leppänen et al. (2008b) with https://doi.org/10.1002/hyp.13785 throughout the paper.

732: Gapless snow depth records are required. . .

768: It is not right to make a general statement like “Typical mean density for fresh snow(24 h)seems to be clearly below often assumed100 kg m−3” as long as you can only consider snowfall, when SWE increases, i.e. you miss snowfall events, when concurrent melt occurs, whose mass loss is larger than the mass gain by snowfall (see also comment on line 691). Moreover, you miss all small snowfall events, which are smaller than your uncertainty measure of 2.36 cm.

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