Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-152-AC3, 2020

Hydrology and Earth System Sciences Discussions

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Interactive comment

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 #3 (AR3) for thoroughly reading the manuscript and providing valuable criticism. We will carefully incorporate the suggestions, eliminate errors and inadequacies, and we are sure to end up with a significantly improved paper. AR3's major point of criticism is that the paper was "not focused in its objectives". In some respect this argument overlaps with AR1's, in parts also with AR2's criticism. The authors are very motivated to solve this issues during the revision and come up with a more precise concept and motivation.

Like it was stated in the "Answer to Anonymous Referee #1's comment from 28 April"

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Discussion paper

(AC1) we still want to see the ΔSNOW.MODEL as a new method to simulate SWE, which combines parts of older approaches. For the revised manuscript we will better specify the way how this is realized. But of course we also want to compare it with existing methods and proof its promising performance. We are convinced that both – "a)" and "b)" in AR3's comment – are necessary for the paper and we want to satisfy both.

During the presentation of the method we will be more clear on (physical) processes and will follow AR3's suggestion to describe them in advance. This might also help to satisfy AR1's arguments on keeping apart model code and snow processes.

Our "allergic" (cit. AR3) appearance regarding the usage of temperature is twofold: Firstly, using temperature would be nothing new at all. Many snow models could be run with temperature *and* snow height as input (and parametrizations of other variables), but hardly any with snow height *alone*. Secondly, there are indeed many precious daily snow height records without respective temperature observations. Some of them go back to the 19^{th} century and even today there are manual snow height observations made with no respective temperature measurement. Downscaling or interpolation of temperature is often tricky, e.g. in alpine valleys with frequent temperature inversions etc. The ΔSNOW.MODEL evolves from a project to renew the Austrian snow load standard where we rely on old records for extreme value analysis. We will clarify this for the revised manuscript.

AR3 (like AR1) is highly critical of the null-hypothesis of the paper. We will omit it.

For the revised manuscript we will review what clearly is a result and what is discussion. We want to keep best parameter choices in the results section since they are important *outcomes* of the study. Description of parameter sensitivity is shifted to the discussion section as well as the interpretation of the optimized values. We are not quite sure what is meant by "uncertainty due to model structure" (cit. AR3). In case uncertainties are meant, that arise due to "structurally" omitted processes (wind drift, sublimation, etc.): Also AR2 criticized this aspect. There will be a respective statement in the revised version. However, we cannot quantify these uncertainties.

Many thanks for the hint that the manuscript “leaves the impression the usage of viscosity is a unique, ingenious feature of this model.” (cit. AR3). This is not at all what we want to purport. Of course Newtonian viscosity is broadly used for dry snow compaction. We will state that more clearly.

The authors will try to avoid side remarks and parentheses during the revision.

More details:
L8: We agree and will clarify.
L29: changed accordingly
L44ff: We agree and omit the acronyms.
L49: Sorry and thank you. This is indeed wrong the way it is written in the manuscript. We are aware of SWE from passive microwave radiometry, but it is not available for the local and point scale, operational for many years. That is the point we want to make here. We will clarify.
L75ff: We agree; this might be confusing. The respective sentence is omitted in the revised manuscript and a note is added earlier where “thermodynamic snow models” are introduced.
L158: We agree to AR3’s argumentation, but the term “deformation strain” is used by Jordan2010. We will name it “deformation” for the revised paper.
L168ff: We agree and will change to “Dry Compaction”. Thank you for that. There will be a table in the revised version which clarifies the relation between modules and physical processes.
L214: This is true. τ will be introduced accordingly in the revised manuscript.
L322: “. . . ” is changed to “etc.”
L397: The acronyms of the methods are better known than their written-out names. Therefore, we keep them but provide the respective references in the revised manuscript.
“L-BFGS-B” is a limited memory (L) quasi-Newton method with the capability of han-

dling bounds (B) of Byrd et al. (1995). BFGS stands for Broyden–Fletcher–Goldfarb–Shanno algorithm. “Bobyqa” implements optimization by quadratic approximation. The name is an acronym for Bound Optimization BY Quadratic Approximation (Powell, 2009).
L464: changed accordingly
L464: Rather certainly, but "presumably“ (cit. AR3). Still, for consistency reasons we chose to provide three significant figures for all our optimized parameters. We change to two significant figures for the revised manuscript (except for ρ_{max}).
L500: We agree and will change that accordingly.
L522ff: Changed accordingly.

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