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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #3

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Snow water equivalent (SWE) has dominating significance for snow hydrology and climatology, however, it is difficult to measure and most available records are based on snow height (H) which is simpler to retrieve. This manuscript (MS) presents an interesting approach to estimating SWE from snow height records and addresses hence an important task. The method described here is not entirely new and the authors frankly state the source of their inspiration, but repeatedly sell their implementation as a new method. There is enough innovation to warrant a publication. and in my view, the MS is served better without this persuasive layer of arguing for newness.

Nevertheless, considerable revisions would be needed to shorten the somewhat

lengthy MS and to clarify its presentation. Currently it is not focused in its objectives and it is hard to understand at a conceptual level: what does the paper want?

a) Describe new method and proof the concept?

Or

b) Show its superiority wrt other existing methods?

This is somewhat unclear but the MS covers aspects of both a and b, without satisfying each. The authors should make a decision and revise the MS following a clearly laid out motivation.

SWE and H are related by the bulk snow density rho_b, such that SWE = rho_b * H

Changes are either by adding/ subtracting mass from the test volume, i.e. by snowfall, deposition of wind blown snow, meltwater runoff or wind erosion or due to gravitational compaction or a combination.

The MS would gain if authors first picture their understanding of the process in detail. This would help to clarify descriptions of their own methods and those of others. If the processes were described in advance it would be easier to put a name on things and state which assumptions are made in different methods.

The presented method is based on deriving SWE from H, and falls therefore back to estimating rho_b. In this MS, assuming an initial new snow density, the evolution of rho_b is considered due to compaction (distinguishing two regimes for dry and wet conditions). Deviations between simulated and observed H are assigned to represent mass changes (snowfall, meltwater runoff).

The authors repeatedly make a point out of that their method only uses H as input and appear allergic to including auxiliary meteorological data which could be useful to improve the model. Their argument of data sparsity in high mountain regions is valid, but one may wonder whether snow height records actually are more widespread available than for instance temperature measurements?

I recommend re-structuring the content of the MS to clearly distinguish between objectives, description of methods, results and discussion. Objectives and conclusions need to be separated: the presentation of a null-hypothesis (L132) followed by the conclusion that it can be rejected, before even the method is introduced, appears a persuasive presumption that is inappropriate in a scientific paper. Instead, the authors should describe what would be needed to reject the null-hypothesis. And if this is the guiding objective in this study, it should be stated “the null-hypothesis” not “a possible null-hypothesis”, be clear about your aims!

I found the description 2.1 a wordy mixture of discussion and the actual description. This should be better separated. In this section please just describe how your model works, and discuss choices and their consequences afterwards in the discussion section.

The “results” section (sec 3) describes the outcomes of a parameter sensitivity analysis and the results of the model application are described in a separate section 4, the discussion section then is kept very short. The presentation appears somewhat unclear and merges discussion with presentation of results (especially sec 3).

I recommend to clearly separate discussion material from the presentation of results and moving them to the expected places: lot of sec 3 is discussion material and should be moved there. The discussion should then comprise several parts: parameter uncertainty and uncertainty due to model structure (the latter part is basically missing).

As a matter of fairness, it should be stated that the physically-based “thermodynamic” snow models do describe dry snow compaction according to Newtonian viscosity, though with a transient viscosity. Currently, the MS leaves the impression the usage of viscosity is a unique, ingenious feature of this model. The crucial statement “deltaSnow.Model “rearranges” existing components in a physically consistent way” is a bit hidden and should be more prominent.

The model description would gain from a short description of the procedure and possibly a flow-chart illustration to help the reader understand the procedure. Currently this information is a bit lost in the details.

Specific comments: Throughout the MS: in written language, spell out to avoid the contractions “it's”, “don't”, “shouldn't”, etc

Language should be direct and in complete sentences. Please remove the frequent side remarks in parentheses, Examples: L37, L66, L99, . . .In some instances, the side remarks are marked by hyphens and sometimes by brackets. L73ff; why is this in parenthesis? if you have to say something state it.

L8: the requirements of a gapless record together with the statement that temporal resolution does not matter does not make sense without specifying “gap” in contrast to “temporal resolution”. Records are not continuous but come at discrete time intervals, which could be seen as gaps.

L29; “sonic or laser distance ranging”

L44ff: no need to introduce acronyms such as GPR, CRNS, or GNSS since they are not used

L49: this is not true. Passive microwave radiometry has been used since long to monitor SWE

L75ff: this is an odd conception to consider statistical models as “thermodynamic”. . .seems like everything else than deltaSnow is considered a thermodynamic model?

L158 “deformation strain” is an odd expression. Strain is a description of deformation.

L168ff the title “dry metamorphism” is misleading. The section describes the “dry compaction” but not the metamorphism.

L214: tau is a threshold deviation, if I understand correctly.

L322: this is an incomplete sentence.

L397: avoid jargon “L-BFGS-B” and “bobyqa” and name the methods, references needed if you do not describe them

L464: “its” instead of “it’s”

L464: how significant are the trailing decimals of 0.0299 if your search range was [0.01 0.2]? this suggests a precision that you presumably do not have.

L500: to start the model validation with questioning the observations is inappropriate! Describe the behavior of your model as exposed to observations. If applicable, quality issues with data may be discussed, but sub-ordinate to evaluating the model.

L522ff: avoid jargon “Pi16, Gu19; Pi16cal, Gu19cal, Jo09, ….” in the text. The abbreviations are used to label plots and should be explained in the associated figure caption.

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