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Interactive comment

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

Michael Winkler et al.

michael.winkler@zamg.ac.at

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We want to thank Anonymous Referee #1 (AR1) for her/his supporting and motivating words as well as for the valuable and positive criticism. The following is our general answer, detailed changes in the manuscript following AR1's suggestions will be outlined in a separate comment together with the revised paper.

First we want to address the referee's "main point (1)", according to which the paper was written like a story, it should be re-sharpened and shortened to a concise description of the particular innovation:

We can understand this criticism, but the narrative style was chosen consciously. Assumingly, the reason why it was “liked to read” (cit. AR1 and AR2) and written in an “easy to read language” (cit. AR2) grounds on this way of writing. We will define the aim of the paper more precisely in the revised manuscript’s motivation.

Ad “specific comment (ad 1)”, (i):
We present a new model, a new method to simulate SWE. We will try to emphasis this even more during the revision, although this is already literally stated in the abstract, in the header of section 1.3 and (twice) in its body as well as at the very beginning of the conclusion section. Maybe the frank acknowledgements (at two occasions in the text) of not having improved physical knowledge on snow, leads to the impression of not having developed a new, standalone modeling approach. The model of course originates from former developments, but it cannot be seen as “the improvement of an existing model” (cit. AR1). Therefore, we cannot linearly build the paper from “Gruber/Sturm-Holmgren/Rango-Martinec approaches” – as AR1 suggests to do. Still, we will sharpen the manuscript in this respect: (1) “model structure and audacity to consequently use only snow height in digital times” is taken from Gruber2014 (who takes the power law from Martinec/Rango1991) and (2) “exponential compaction” is taken from Sturm/Holmgren1998. Unfortunately, it is not trivial to compare the ΔSNOW.MODEL with Martinec/Rango1991 and Sturm/Holmgren1998. The same is true for Gruber2014, whose code suffers from some major bugs. Martinec/Rango1991 as well as Sturm/Holmgren1998 do not provide a method how to deal with declining snow heights, the question how mass loss is computed is not answered in their papers. This strengthens our argumentation to provide a thorough explanation how our new model is coded (see below). We do compare the ΔSNOW.MODEL with observations, see whole Sect. 3. Involving “a parameterization exercise” and “a model intercomparison” (cit. AR1) is an absolutely necessary part of every model presentation. Omitting these parts would understandably lead to loud criticism. The reason, why parametrization and comparison are found in the results(!) section, is given in the motivation section (1.3). We don’t understand, why the paper’s focus should be on “(v) snow load” (cit.

AR1). Section 4 is titled “Example of Application”, which makes clear that it is not focal, but completes the circle to proposed usability (Sect. 1.3).

Ad “specific comment (ad 1)”, (ii):
The description of the data is provided in the cited sources (Gruber2014 and Marty2017). AR1 also criticizes the length of the manuscript. We agree on being lengthy at times and try to shorten the revised version. Including data description, although this information can be found elsewhere, is perceived as contradictory. Nevertheless, we will provide a map in the revised version’s appendix.

Ad “specific comment (ad 1)”, (iii):
See (vi).

Ad “specific comment (ad 1)”, (iv):
The authors do not really understand this criticism. We describe the physics of our model as well as its implementation in the code. That is how modern, open modelling approaches should be introduced. This is not a paper on new physical insights, but it describes how well-known concepts are used, rearranged and set together (“coded”) in order to get a new model.

Ad “specific comment (ad 1)”, (v):
AR1 is right, this is overweening since we do not have a clear physical, experimental problem to be solved. We omit it.

Ad “specific comment (ad 1)”, (vi):
We agree, the new approach could easily be combined with temperature and humidity. As the title already defines (“exclusively”), the ΔSNOW.MODEL should be able to simulate SWE only (!) from snow height. The motivation for the consequent dispensation on further input variables is broadly given in Sect. 1. Including further variables could be future work but – admittedly – not a very innovative one, because such snow models do already exist. During the development of the new approach it was also tried to filter information about rain-on-snow events solely from the snow height records. As others

before, we failed to find a convincing way to do it. We could have changed our plans and include further input variables (which often do not exist, see Sect. 1) to parametrize rain-on-snow, but we decided to ignore it and get surprisingly good results although not explicitly incorporating rain-on-snow. This issue is discussed in Sect. 2.2.3 and 5. The authors will try to formulate all this more precisely for the revised version.

Ad (2)
In the opinion of the authors, these expressions foster readability (which was appreciated by AR1 him-/herself and AR2). We agree, they are not “clear scientific” and will replace them.

Ad (3)
The authors are no native speakers. Nevertheless, we are convinced that the text is written in a proper way: Firstly, there is the somewhat contradictory statement of AR1 (“liked to read”), and AR2’s likes the “easy to read language”. Secondly, there is hardly any misunderstanding to be eliminated based on AR1’s review and not a single typing error was found. Thirdly, a great portion of the requested “technical corrections (ad 3)” are language corrections that could be debated about. We happily observe that AR1’s criticism does not involve any objective features of the methods (i.e. the model) and the results, but only the structure and the language of the paper. The latter is quite subjective and the authors argue that this does not justify a major revision.

in detail:
-1: around the globe
-3: SWE is not “synonymous” for snow water equivalent, but SWE is “an abbreviation” of snow water equivalent. However, SWE can be used as a synonym for snow load and snow mass (at least in this paper). That’s what is written on line 3 in a grammatically correct way. In the revised manuscript we put SWE in brackets to make it more clearly.
-4 and 5: changed accordingly
-7 and 8: Our intention was to make short and clear sentences in the abstract. We will formulate this differently for the revised version.

-12: We agree, this is a multi-clause sentences, which should be avoided here. Changed accordingly.
-13: changed accordingly (although probably readability suffers)
-14: Both is correct. We discussed it during the writing process and decided to use “squared”. Now we change to “square”.
-16: changed accordingly
-18: We skip the whole sentence.
-29-30: See five references at the end of the sentence.
-25-48: This is only true for small sampling cylinders (where a pit has to be dug), but not for big snow tubes (see reference). We will add a respective statement.
-54: changed to “hydrological, agricultural, and many other applications”
-70: changed to “, mainly precipitation, temperature, humidity, wind speed and radiative fluxes.”
-71: Often there is only a H record, no associated T, rH etc. record. Changed for better understanding.
-68-77: We omit the brackets.
64-77: The requested references will be embedded in the revised manuscript.
-107: There is nothing to correct. Maybe a ligature issue?
-119: “Snow depth” is only used in word-for-word citations when the original author(s) used it as well. At all other occasions we used “snow height” in the manuscript.
We argue “snow height” and “snow depth” are the same and widely used interchangeably, but “snow depth” seems to be the more accepted term. The decision to use “snow height” was not made thoughtlessly: The quantity is measured from ground upward to snow surface and – in this respect – it is a matter of “height” (not “depth”, which would be measured from snow surface downward to the ground). Moreover, it is commonly symbolized with “HS” (not least following Fierz2009) and with “H” in this paper. This abbreviations better fit to “snow height”.
As AR2 clearly suggests to use “snow depth” (instead of “snow height”) and “HS” (instead of “H”) we will change the naming for the revised manuscript in the respective

way. This will also change the title.
-125-135: We agree to AR1, this could be misunderstood. We will find other words here. We are convinced that the ΔSNOW.MODEL can be regarded as a stand-alone model.
-141-142: Sorry for that. In the meantime "nixmass" was also ported to CRAN and can easily be found there.
-149: changed accordingly
-157-160: See above (Ad "specific comment (ad 1)", (iv)). The authors cannot follow this criticism and related suggestions. It is the core purpose of the paper to describe how physics is converted to model code. Therefore, we have to describe how specific physical processes, the treatment of measurement and model uncertainties etc. are implemented in the modules of the ΔSNOW.MODEL.
-177: changed accordingly
-196-197: Kept unchanged, see above.
-200: We leave this here, but omit it at the other two locations.
-228: As stated above, the rain-on-snow issue will be sharpened during the revision.
-236: We agree, this is misleading. We will delete the words in brackets.
-242-243: At this location it gets especially obvious how thoroughly we try to describe in which way the physical concepts (taken from Jordan (2010) here) are converted to model code. This is what we think is the core of a transparent introduction of a new model, we therefore do not change the text here.
-355: As stated with quotes and respective reference this definition of η_0 was literally taken from Sturm and Holmgren (1998). It is changed to "viscosity at [which] ρ equals zero".
-380: Yes, because the Hydrographic Service of Tyrol is also responsible for the region East Tyrol. Its southernmost mountains belong to the "Lienzer Dolomiten" and the "Karnische Alpen", which indeed are parts of the Southern Alps. There will be a map in the revised manuscript's appendix which will clarify.
-512: Changed accordingly.

-526: The authors of the manuscript are not aware of any other available calibration/validation data for those two models.
-545 558 560 569: Changed accordingly.
-685: Not essential. We will omit “due to the Fréchet-like distribution”
-695: Unfortunately not. For most of the longterm and historic snow height records we do not have temperature, let alone humidity etc. (see Sect. 1) – except reanalyses with respective uncertainties. We will sharpen the text for the revised version. See also arguments above.
-709/710: We will omit the question. However, concerning SWE(!)-related studies we still think there is room for more.
-761-763: Changed accordingly.
-Figure 1: The authors think that is the key message of the figure. It shows how physics and model code are combined, it will be tried to graphically separate the two (code/hydrologic processes) during the revision.
We tried to do it as a block diagram but this would take too much space. Moreover, a slender block diagram also needs a longer than average caption. So we stayed with the bar chart and the – admittedly, but consciously – quite extensive caption. Figure 1 (plus its caption) can stand on its own and describes the ΔSNOW.MODEL to a great extent.

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