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The Cryosphere Discussions

Interactive comment on "Investigating the dynamics of bulk snow density in dry and moist conditions using a one-dimensional model" *by* C. De Michele et al.

Anonymous Referee #1

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General comments

De Michele et al present a numerical model for snowpack evolution. The melting is represented by a classical temperature index approach. Other phase changes are neglected. Such simple models are widely used by the hydrological community to estimate melting flow and snow water equivalent evolution. When the snow depth is also estimated, very simple parametrizations of snow density are often used. Here, the originality is (1) the simulation of liquid water retention with a parametrization of drainage taken in literature and (2) an evolution law of dry density due to compaction and snowfall. This allows computing the total density accounting for to the presence of

liquid water in the snowpack. The concepts and equations of the models are presented in details. Then an evaluation is performed for 3 or 4 seasons on 2 local sites of different elevations in Western US with measurements of snow depth and snow water equivalent. Results are quite satisfactory in both the calibration and the validation periods.

This article presents a quite innovative way of modelling snow depth with limited input data (precipitation and temperature) as most models based on the temperature index method do not separate the very different behaviour of snow density for dry and wet snowpacks. In this way, this paper provides an interesting contribution for the hydrological community who often uses such simple snow models in areas where meteorological data are insufficient to apply more sophisticated models based on energy balances.

However, the purpose of developing such a model is to my mind unclear in the paper (I agree in this point with the short comment of R. Essery). The authors wrote that the aim of their work consists in is simulating the bulk snow density. But what is really the interest in simulating snow density? I assume that the final purpose is not an investigation on snow processes, as more detailed models resolving energy balance and with a detailed vertical discretization are best suited for this. However, applying such a model is probably interesting in applications where snow depth is required (not only SWE). But no example of such applications is given here. This approach might improve the performance of basic temperature index models. Unfortunately, the performance of the new model is just compared to measurements and not compared to a simpler model without these representations of compaction and liquid water retention. Therefore, the interest of this new approach is not demonstrated in the paper although it looks promising regarding the results.

I would suggest that the authors rewrite the introduction to more clearly emphasize the position of their model among the available snow models (in terms of complexity and input requirements), to give examples of the possible applications of their model and the advantages to use it regarding other available models.

Then, I highly recommend that the authors provide in section 3 a comparison of results with a classical temperature index model and a simple parametrization of density like one of those cited by the authors. This would probably not require long developments and would illustrate the added value of this work. A comparison with physical based models (like those cited in the paper) would also be useful to assess the influence of the temperature index approximation and of the neglected processes.

Finally, the evaluation is just performed on two local sites without measurements of liquid water content. For a study particularly focused on this point, why not choose one of the sites where these data are available (Col de Porte, France, Davos, Switzerland, etc.)? The authors should also add a short discussion after presenting the results about the possible application of their model in hydrological contexts, the requirements of new calibrations in other areas, and the known limitations of temperature index models (for some specific climatic conditions, some specific regions, etc.).

The model description and the introduction of the three differential equations are very complete but need some modifications as detailed in the specific comments below. However, I am wondering if the chosen evolution law for snow density couldn't be significantly improved by accelerating compaction during melting or rainfall events as the metamorphism of snow grains is very quick with liquid water and the compaction clearly different than in dry conditions (Vionnet et al, 2012, equation 8). In one way, the reader is quite disappointed with the model formulation as after the introduction and with the title of the paper, we expected that the density of the solid component would evolve differently in dry and wet conditions. This is not the case. Actually, the only way to account for liquid water in the computation of density is the liquid water density term.

The form of the paper is also questionable: the input data are presented in the results section whereas it is more common to present them before the description of the model. For me, this unusual position does not really alter the understanding, but I am wondering if the editor would not prefer a more classical arrangement of the different sections. A more problematic issue is the part between lines 11 and 27 page 2316

which describes the numerical method to solve the differential equations. These lines have nothing to do with the results and must be moved at the end of section 2.

Other comments

- Page 2307 line 15: multi-layer models "occurs". Please rewrite, numerical models do not occur, they are developed by scientists.
- The sentence lines 24-25 p. 2307 is confusing. The link between multi-year simulations and accounting for dry and moist conditions is not clear: dry and moist conditions also occur during the same season.
- In section 2.1 lines 5-8, the 0°C threshold on air temperature to separate dry and wet snow is a model assumption. The formulation suggests that it would be the real behaviour of snow which is not the case. For instance, it is very common to observe dry snow with air temperatures higher than 0°C in North aspects in winter when the sky is clear (low long-waves radiation) and the direct solar radiation masked all day by the slope. Therefore, the text should be modified to acknowledge that it is a simplification.
- In section 2.1 again, I don't understand the interest of lines 9-17: the definitions of n and Φ are not used later, V_W is obviously always lower than nV_S and I don't understand the difference between V and V_S or h and h_S.
- In section 2.2, the way to compute T_S appears too early in the text as this variable is only required in equation 7. I suggest moving this part after equation 7.
- In section 2.2.2, I think that it should be mentioned that compaction is actually due to both snow weight and snow grains metamorphism and that equation 6 do not account for the second process.

- If melting occurs, the snowpack is no longer dry (page 2314, lines 5-6), please clarify.
- The accuracy of measurements may be quickly discussed in the results section.
- As the article is relatively short, I would suggest to increase the size of figures 2 and 3 which are quite difficult to read.
- Page 2318, the comparison of the time evolution of liquid water content between the US and the Alps is clearly not relevant.

Reference

Vionnet et al, 2012 The detailed snowpack scheme Crocus and its implementation in SURFEX v7.2, Geosci. Model Dev., 5, 773-791, doi:10.5194/gmd-5-773-2012

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