Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2018-451-RC2, 2018

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Technical note: Snow Water Equivalence Estimation (SWEE) Algorithm from Snow Depth Time Series Using a Snow Density Model" *by* Noriaki Ohara et al.

Jonas (Referee)

jonas@slf.ch

Received and published: 4 December 2018

This technical note reports on using an existing snow density model to derive SWE from a temporally continuous record of snow depth. Given the effort required to operate and maintain a snowpillow, being able to estimate SWE from alternative snow depth measurements has its potential uses and merits, e.g. for gap filling purposes, or in case other meteorological data being unavailable to run a full snowpack model.

Obviously the authors were not the first to come up with using a snow density model in that particular context. The performance is similar (or in fact slightly worse) in comparison to two parametric models that were developed about 10 years ago with the same application in mind. However, unlike those two alternative offerings the approach presented here is capable of providing meaningful time series of SWE at high temporal resolution (at the cost of requiring complete time series of snow depth and temperature input data). This would be a good selling point of the paper if it wasn't for other publications that have already tackled this very aspect, see e.g. the excellent paper by McCreight and Small from 2014 (doi.org/10.5194/tc-8-521-2014).

Now the question arises, what then is the selling point of this paper? As a technical note, it might suffice to present the method as an alternative approach – if the authors manage to identify at least some differences to existing models. Better transferability to other sites without recalibration maybe? And obviously, testing the model with data from one season and one SNOTEL station only is not nearly enough.

I suggest the authors put more effort in precisely framing their work in the context of alternative snow density models; and perform a multi-site and multi-season validation.

Below just a few more specific comments:

Line 1: the title is somewhat misleading given that your model also requires temperature data as input

Line 48-51: extend this into a convincing last paragraph of your introduction, which highlights shortcomings of existing models a/o the merits of your approach (to be demonstrated below)

Line 52: start method section here

Line 101: and the former?

Line 117: while this assumption is what you often have to work with, it would be interesting to also deploy your model at a site where you actually do have snow temperature data to test, how big of a problem is this assumption?

Line 127: since you require temperature data as model input anyway, why not using a new snow density formulation that is temperature dependent?

Line 156: ". . . which is independent from climate ..."? You may have a point there, but let's first see what happens if you apply your model in Japan, Siberia, Lesotho; without recalibration of course. You can refer to Matthew Sturm's snow classification scheme to back up your claim.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018-451, 2018.