

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.

Anonymous Referee #1

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This technical note presents a method that brings together previous research on snow density as it relates to depth. The authors compare the estimations to a single year at a SNOTEL station as well as a single year with a snowmelt lysimeter.

Overall, I think the idea has merit and could be a useful tool when a more complex computational model is not appropriate. However, I think that the manuscript in its current form lacks enough validation or error analysis for the presentation of a new method. The comparison of the method to a single station for a single year is not enough; I would really like to see multiple locations with varying conditions and snow years to be convinced. Furthermore, I think that the comparisons to other models is

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lacking. While I am happy to see the comparisons to Jonas et al., 2009 and Sturm et al., 2010, much has been done in the past 8-9 years. Perhaps a comparison to a more complex model like SnowPack in addition to what is already shown. That could provide evidence that if all you need are bulk properties that this new approach could be just as good and computationally easier.

More specific points are as follows:

I appreciate the listing of 7 possible reasons for the errors, but these should really be better quantified in magnitude of error and some can actually be directly addressed with the available data.

For example: 1. snow density of newly fallen snow: The authors show the sensitivity of the equation to this parameter, but that doesn't mean this parameter is the reason for the error. Furthermore, enough information is available to compare what the SWEE estimation is to the Snotel new snow density (depth change during storm and SWE increase can be used to determine new snow density).

2. snow erosion and abrasion affect snow depth: By how much? what is the impact on the estimation (quantify)

3. Snow pillow measurement error - again, how much? There is literature on this, it will depend on the time of the season, etc. but how will this impact your comparison? To this end, more snow pit observations would go quite far in this study.

4. air temperature substitution - how much does this impact your results?

6. Accuracy of snow depth - this can be addressed a couple of ways. First there is literature on this and/or manufacturer defined accuracies that should be taken into account. Second, quantification of the theoretical error from this could easily be done. If the depth is off by 2 cm, how much does that change the SWEE result? Is this different for shallow or deep snowpacks?

7. Treatment for snow depth is inappropriate - If this is the case, first quantify the

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effects, then second why not change the treatment? When applying a new method like this I think the data pre-processing can be huge. If your input is bad then your output will be bad. So why do you think the treatment may be inappropriate and how can you change it? Then do this to prove the presented method.

line 153: the authors state that "However, this could lead to better SWE change estimation" talking about the thought that SWEE may be better than the other regression models to estimate melt, then the manuscript compares the new method to lysimeter data, but not the other models. With a claim that the new method may be better, it should really be shown because you have the data to test this statement.

According to table 1, there are two other methods that work better and are simpler to apply, please argue convincingly as to what the best method is and why (with data).

Fig. 1 - labeling each panel would help a lot. Why is the SNOTEL density not included in the density panel? This could help show how much better/worse each method is for estimating density.

Throughout the paper the authors use "change $[\Delta]$ SWE", but the $[\Delta]$ symbol is generally used to denote "change in". Is it supposed to be something different here, please double check.

please correct "Strum et al." in figure 1 to "Sturm et al."

the term "SWEE" is confusing to me, especially when the wording went back and forth with SWE observations comparing to SWEE. Perhaps changing it to "estimation of SWE (eSWE) to help the reader.

I hope that these comments are helpful.

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