

The Cryosphere Discuss., referee comment RC1
<https://doi.org/10.5194/tc-2021-145-RC1>, 2021
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Comment on tc-2021-145

Anonymous Referee #1

Referee comment on "A local model of snow–firn dynamics and application to the Colle Gnifetti site" by Fabiola Banfi and Carlo De Michele, The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2021-145-RC1>, 2021

I had also performed the review for the first manuscript. I confirmed that the comments I had made earlier had been addressed to a certain extent. The position of this model seems that it is a one-dimensional component for a future developed three-dimensional model that integrates hydrological, nivological, and glaciological components. The revised manuscript showed some usefulness of the multi-layer model in this paper by comparing it with the ice core profile.

As the concern point, results of the validation of one-layer model were also shown in Fig. 8 as averaged density, but the reason to perform comparison of both one-layer and multi-layer is unclear. In my opinion, the comparison of multi-layer is more valuable. But according to L482-484, the advantage of the one-layer model was stated that it is easier to incorporate into hydrological models. Are you going to use a one-layer model in the final integrated model? If so, the position of the multi-layer model becomes unclear. In Fig. 4, the multi-layer model is compared with the actual measurement. If this result is reflected into the parameters of the one-layer model for improvement in some way, the position of one-layer and multi-layer model will be clearer.

minor comments

L293-296 Why did you use the optimized values in the Japan site despite the studied field site being in Europe? If it is because Japan is the only place where the parameters of e could be adjusted, it should be written clearly. Also, it may lead to problems using a and e optimized at different locations in a single equation.

L333-337, L377-389 From this result, the accuracy among several models are compared. Overall, the one that is calculated at high density seems to be suitable. Do you have any ideas to improve the model accuracy of the density profile more?

L350-352, L468-470 In 1D simulations, I think the purpose of considering erosion is to avoid overestimation of the amount of new snow and underestimation of density. Do you have any verification of the amount of the erosion?

L355-366 In Fig. 4, the density profile was reproduced using a multi-layer model. On the other hand, what is the purpose of the validation of the density of one-layer model with averaged observed density. Also, I think quantitative comparison about the accuracy between one-layer and multi-layer model.

L457-461 I understand that you lowered the temperature for snow melt to match the observed density. However, I wonder if it can change a fixed value in natural science such as the melting point in the simulation. It is still understandable if you are assuming the influence of unknown factors such as salinity. Please describe why you used melting point as an adjustment.

L482-484 I understand that a simple one-layer model is more convenient to integrate into a hydrological model, but it is a rough validation compared to the multi-layer model. If

you are planning to use the one-layer model in the final integrated model, the multi-layer results should be fed back to the one-layer model in some way, otherwise the multi-layer comparison will be meaningless. Do you have any plans to connect the multi-layer results with the one-layer model in some way?