The Cryosphere Discussions

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Comment on tc-2020-367

Adrien Gilbert (Referee)

Referee comment on "Firn changes at Colle Gnifetti revealed with a high-resolution process-based physical model approach" by Enrico Mattea et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-367-RC2, 2021

This paper is a modeling study of near-surface firn temperature evolution at Colle Gnifetti (Swiss Alps) between 2003 and 2018. The study uses a collection of unique meteorological dataset from high elevation to force a distributed surface energy balance coupled with a firn-pack model (Van Pelt et al., 2012). This study has the potential for an excellent scientific contribution regarding the quantification of thermal changes happening in cold accumulation area in response to atmospheric warming. I really appreciate the effort put by the authors in building the meteorological dataset based on an impressive amount of data to force their model. The use of a full surface energy balance associated with a representation of melt water percolation and refreezing allows to capture the firn temperature spatial pattern observed in the unique collection of temperature measurement realized at Colle Gnifetti over the last (almost) 20 years. The paper is also pretty well written and structured.

However, the manuscript suffers at this stage of incomplete or inadequate referencing to previous studies in the introduction and along the text and more importantly of the absence of sensitivity test regarding the subsurface model parameters. The consequence is that the discussion concerning model bias is not convincing and poorly supported. The parameters value of the sub-surface model as well as its mathematical description are absent which is critical for a paper focusing on firn temperature.

The manuscript therefore clearly needs major revision before publication. I hope to help to improve its weaknesses by highlighting the major points to revise in my general comments bellow and by providing a list of specific comments embedded in the attached PDF.

General Comments

- In the general introduction paragraph, the referencing to relevant studies in really poorly done (line 16 to 38). The same reference about the use of ice core archive is used multiple times when there is lot of other more relevant and specific studies. Please do a proper research in the literature. Also you cite Master degree thesis (inaccessible and not reviewed) when relevant published work exists. See my specific comments in the attached PDF.
- Also concerning past studies, you mostly ignored other studies done on the same topic and for very similar setup. I am probably oversensitive to it since it concerns my work but lot of the work done in Gilbert et al. 2014a and Gilbert et al. 2014b should be discussed and compared to your results. You will see many reference to it in my specific comments.
- The description of the sub-surface model should be included in the paper. There is no reason to describe the surface energy balance and not the energy transfer within the firn-pack. This is the essential part of the modeling and parameters are not even listen nor their value given.
- From my understanding, you do not take vertical advection into account, the vertical advective heat transport can be significant in cold accumulation zone and should be taken into account. Also what are you doing with precipitation? It is not explained, maybe the vertical advective transport is actually taking into account? Since the subsurface model is not described, it is not clear. The only thing that makes me thinking you actually do, is the thickness of your active layer reaching 20m-depth which is possible only with advection. You need to clarify this in the manuscript.
- The bigger weakness of the manuscript is the absence of sensitivity test concerning the sub-surface model parameters and their influence on the modeled firn temperature. You cannot discuss the model bias without it. For instance, discussing short wave radiation redistribution due to reflection in order to explain your bias is not convincing at all when many parameters modification could explain the biases. From my experience, cold biases in firn temperature model often arise from neglecting short wave radiation penetration. Gilbert et al. (2014a) show that a characteristic penetration length of 2.5 cm is able to significantly change the modeled firn temperature and explain the cold bias observed in their study site at 4250 m a.s.l. As you mention, warm bias could be explained by not accurate representation of water percolation and refreezing. I agree, but to be convincing, you have to perform sensitivity tests on the water percolation parameters and explicitly show the result of these tests. We don't even know what the real meaning of the percolation depth parameter is, since the model is not described. Also the residual saturation parameter due to capillarity force is a critical parameter which is not well constrained. I suggest to test its influence on your results, you could be able to correct your warm bias.
- What about the firn thermal conductivity? Recent work of Calonne et al. (2019) should be used. The author corrected a significant bias on the commonly used conductivity/density relationship.
- My final general comment is about the presentation of the results. You have a really nice distributed model but you do not really use it to show the spatial heterogeneity of the firn warming which would be a valuable result. I suggest to add a map of current firn 20m-depth temperature and a map of the associated warming rate. You will see it in my specific comments in the attached pdf.

Specific Comments

You will find a list of specific comments embedded in the attached pdf. They are sometimes redundant with my general comments but will help to clarify them.

References

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Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-367/tc-2020-367-RC2-supplement.pdf