Comment on tc-2022-210
Anonymous Referee #2

Referee comment on "Brief communication: Nonlinear sensitivity of glacier mass balance attested by temperature-index models" by Christian Vincent and Emmanuel Thibert, The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-210-RC2, 2022

Review of

Brief communication: Nonlinear sensitivity of glacier mass balance attested by temperature-index models,

by Christian Vincent and Emmanuel Thibert,

submitted to The Cryosphere

General comments

The submitted manuscript presents numerical experiments on glacier surface mass balance (SMB) based on a temperature index (TI) model. The authors aim on demonstrating that even with linear relationships between air temperature and snow/ice melt, such models show a non-linear sensitivity to climatic variations. The manuscript is a direct response to a publication by Bolibar et al. (2022), which presents comparisons between a deep ANN approach for estimating the glacier response to climate projections with simple TI models. Vincent and Thibert criticise the proposition in Bolibar et al. (2022) that simple TI models do not show a non-linear sensitivity to climate variations in their experiments, in contrast to the deep ANN experiments.

This is not the place to discuss details and validity of the Bolibar et al. (2022) paper, but rather to review the issues presented in the manuscript at hand. The authors raise an interesting question, which was also discussed earlier: what is the characteristic behaviour
of TI models for changing climatic boundary conditions? It seems that one potential conflict is not as severe as it is presented. Even though Bolibar et al. (2022) state that TI models usually show a linear response to climate variations, they mainly compare a fully linear Lasso approach with their ANN model. They even claim that TI models with different degree day factors for snow and ice melt, show some non-linear behaviour, but that their response is not adequate compared to the ANN approach. Therefore, the response of Vincent and Thibert should be focussed on the validity of the TI model response, rather than on just demonstrating the non-linearity per se.

However, this manuscript provides rather interesting insights into the fundamental behaviour of TI models and this analysis could serve as a great study about the model characteristics, if some shortcomings could be fixed. The authors concentrate on demonstrating the non-linear response on a change in forcing, instead of discussing the fundamental interaction of the differences in snow and ice melt for the final glacier mass balance. There is a multitude of publications, which discuss the limitations of TI models due to their fixed relationship between air temperature and melt, while temperature is an indicator of energy availability, not energy transfer. But as long as the forcing stays within certain limits, TI models provide a robust and simple method for SMB estimates. Therefore, the critical investigation of the non-linearity characteristics within these limits would add high value to the discussion, also in the light of the application of AI approaches.

Major concerns:

The data section does not provide the necessary information to evaluate the experiments. Only the two glaciers are described, but details neither about the mass balance data are given, nor about the necessary additions information, like DEMs etc.

The methods section does not provide sufficient details. It is not clear how the model is applied to the glaciers. Is it a spatially distributed model, which cell resolution is used, is the glacier surface elevation static, or is there a dynamic response? What is the time step? How was the forcing parameterised across the elevations/aspects?

With regard to the general criticism of the Bolibar et al. (2022) paper, it needs to be highlighted that they estimate SMB for an entire region, while here two individual glaciers are considered. This allows a more detailed investigation of local SMB reaction, compared to general trends.

There is no section about the determination of the DDF values. I would expect a section about calibration and validation with the available forcing data set, or at least information where to find these details.
In the Results section, you describe the non-linearity of the model response with an increase of sensitivity with respect to the anomaly (L.81). A major point would be to relate magnitude of these sensitivities to the sensitivities found by Bolibar et al. (2022) with their deep ANN approach and discuss the consequences within the bounds of potential future anomaly ranges.

I wonder why you did not investigate summer snow fall in your experiments, as this is the major actor of non-linear response in the ANN approach. It should also be expected that summer snow fall has a strong non-linear response in the TI model, because of the difference in DDF values for snow and ice and the strong reduction of melt in the main ablation season.

Sensitivity to winter balance: in L.94-98 you describe the contrasting results of the TI-model with respect to the ANN model with regards to a decreasing winter balance. However, there needs to be an explanation why the TI model explains reality and what are the consequences in the view of the ANN results. As ANN is more or less a black box, the results cannot be judged in the view of physical constraints, but just in respect to validation data sets. An investigation on the physical basis for the TI models’ sensitivity would improve the discussion about the pros and cons of the two different approaches.

Minor issues:

L.24: I would prefer “Surface mass balance” projections instead of “glacier mass projections”, as the projections aim on SMB not on the full mass variations (which include basal melt and other processes).

L.24-29: There should be at least a short characterisation of the differences in model approaches, in order to clarify the topic.

L.37-38: Already here it would be helpful to shortly discuss the basic non-linearity of coupled linear relationships.

L.54: The information about the reanalysis data needs to be described in the data section. It requires also some information on periods used, resolution etc.

L.64: Is k a function or just a two-value parameter?
L.74: how was the anomaly applied to the original data? I guess that you applied a constant anomaly to the daily values of the forcing series, in order to calculate a SMB anomaly.

L.77: It is not clear what you did here. I assume that you ran the model at specific points (where you presumably also have stake information) and as a distributed model across the entire glacier (which grid, etc.?).

L.79: Your statement with respect to Bolibar et al. (2022) is not exactly correct: Bolibar et al. (2022) write about piecewise linear relationships and implied non-linear response on page 5. However, their conclusion is that the TI model results are rather similar to the Lasso approach, which is clearly not confirmed by your investigations.

L.82: Details about the synthetic input series are missing (How did you construct these series? Do they represent a certain realistic SMB range?).

L.85: It might be a good idea to show the length of ice ablation period vs total ablation period and the onset date of ice ablation. The onset of ice ablation is a measure of the non-linear character.

L.91: You describe an increase in sensitivity, but this should be quantified with respect to the disappearance of winter snow to judge the physical basis.

L.110-111: There is a basic difference between the Lasso-models and the TI approach, as the second one uses a step function of the DDF parameter. Therefore, it cannot be expected that the two models provide the same response. This should be made clear.

L.117: It might be a good idea to mention also earlier investigations who pointed out this basic behaviour.

L.121-126: This is a solid argumentation and provides a core conclusion. But is should be expanded by the major points, mentioned above.

L.129: It is only mentioned that the physical reasons are given for a higher sensitivity of TI models to lower winter MB, but I did not find a sound discussion.
The main concern is, that TI models applied far outside the calibration range of parameters might not be able to represent the energy exchange between the atmosphere and snow/ice correctly. However, this might also be true for the ANN approach, because it is unclear how good the performance is far beyond the training domain. Therefore, only a comparison of the different models in a large parameter space with a physical energy balance model would provide serious assessment of the model performance. This is out of scope of this manuscript, but could be mentioned as a valuable future step.

Data availability requires at least the references to the data sets.