Reply to Reviewers :

We have complied with all the comments made by Reviewers (see the point-by-point response). The replies are in italics.

The manuscript has been revised by a professional native speaker (not this Reply).

Reply to Reviewer 3

General comments :

This brief communication investigates the nonlinear sensitivity of simple degree-day models to changes in air temperature and precipitation. The paper is written as a direct response to the article published by Bolibar et al. (2022), Nature Communications, in order to take up one important element of that study and provide additional insights. Overall, this scientific debate is interesting, and I'm convinced that the glaciological literature will benefit from this short paper by Vincent and Thibert that makes a clear and straight-forward statement. Attesting a nonlinear sensitivity to degree-day models is not at all new both in my understanding and that of the authors, as well as J. Bolibar, Reviewer 1 to the present study, and author of the paper addressed in this brief communication. Nevertheless, I find the simplicity of the approaches and the clarity of the statement provided here very useful.
A review of the present brief communication is however not easy as we also need to appropriately consider how the statements relate to the original paper by J. Bolibar, as well as his detailed review. Therefore, the situation is relatively complex, and it will heavily rely on the editor's judgment how to weight the different arguments.
In general, I personally agree with most of the basic statements made in the present study and, hence, would recommend it for publication. However, there is still ample room for improvement in the description of the data, the methods and the presentation (see below), which can definitely be achieved by the authors with relatively limited effort. With regards to the direct opposition to the study by Bolibar – that provides a variety of valuable new insights into processes and methodologies – I would suggest to try and make several of the statements not sound as reproach but more to position this study as a stand-alone research resulting in an important and clearly presented outcome.
This would also somewhat lift the obvious conflict between the original study by J. Bolibar and this paper. Nevertheless, I should mention that I agree that putting the statements of Bolibar et al. (2022) into context is justified, and is not just "cherry picking" as it is termed by Reviewer 1: The Abstract of Bolibar et al. (2022) states twice very clearly that temperature-index models have a linear sensitivity. And even the title ("nonlinear sensitivity … unveiled …") indirectly implies that previous approaches were linear in comparison to the new model. Even though the text indeed provides additional statements that actually better agree with the outcomes of this study, it is the Title and the Abstract that defines what readers take with them. Therefore, I agree that the present paper by Vincent and Thibert, and also some of the formulations (see more details below) are justified.

Many thanks for your comments. We agree to try and make several of our statements not sound as reproach. For instance, in Introduction, the sentence "Their paper questions the use of temperature-index models for projections of glacier-mass changes in response to global warming." has been removed. Our unique purpose is to show that temperature-index models are able to capture nonlinear responses of glacier mass balance (MB) to high deviations in air temperature and solid precipitation.

Specific comments:

Line 14: Please reformulate to make this less sound as an opposition to Bolibar et al. (2022). In fact, I would not say that the study has "questioned" the nonlinearity in degree-day models but has maybe

not “adequately considered/presented” it in the analysis by using a LASSO model that is linear by definition.

It is very difficult to reformulate and refer to LASSO model in the Abstract, given that the Abstract should not exceed 100 words. We believe it is not possible to mention LASSO model and provide further details in the Abstract. More explanations have been added in the manuscript (l. 147-149): “….In the Bolibar et al. (2022) paper, the MB anomalies in response to climate forcing were obtained using a linear LASSO SMB model, which is based on a regularized multi-linear regression.

Line 22: Either define MB at first instance, or write out always. The use of surface mass balance (SMB) would probably be more appropriate.

It has been changed.

Line 27: It would be good to also explicitly refer to energy-balance models. Whereas both degree-day models and ANNs do not fully resolve the actual processes and thus heavily depend on the available calibration data, energy-balance models try to fully describe the processes and the feedbacks which is certainly the optimal approach (although yet mostly inapplicable at large scales).

In the Introduction, we wrote “Most glacier-mass projections in response to climate change in large-scale studies spanning the 21st century have been based on temperature-index models (Huss and Hock, 2015; Fox-Kemper et al., 2021), given the lack of available or reliable information on detailed future meteorological variables (Réveillet et al., 2018).” We refer here to energy-balance models (see Réveillet et al., 2018).

Réveillet, M., Six, D., Vincent, C., Rabatel, A., Dumont, M., Lafaysse, M., Morin, S., Vionnet, V., and Litt, M.: Relative performance of empirical and physical models in assessing the seasonal and annual glacier surface mass balance of Saint-Sorlin Glacier (French Alps), The Cryosphere, 12, 1367–1386, https://doi.org/10.5194/tc-12-1367-2018, 2018.

Line 36: Avoid the term “question” and formulate in a more neutral way. Overall, I suggest to not put the opposition to the paper by Bolibar et al. (2022) as the main motivation for the paper, but rather to focus on the research question and the statement (nonlinear sensitivity of simple degree-day models).

Agree. The sentence “Their paper questions the use of temperature-index models for projections of glacier-mass changes in response to global warming.” has been removed.

Line 48: Define which field observations have been used. Point mass balances, seasonal, monthly? Glacier-wide mass balances? Geodetic ice volume changes?

Details about mass balance and DEM data are now given in the Data section. The point mass balances are calculated for each elevation, for Argentière and Sarennes glaciers. In addition, we calculated the glacier-wide mass balance of Argentière glacier using the point mass balances for elevation range and geodetic mass balances (Vincent et al., 2009).

Line 55: Same statement and formulation as on line 41

Agree.

Line 58: Quite some unclarity remains regarding the application of the main equation: (1) Is the equation applied for each day individually, or for the entire year just once with total precipitation / cumulative degree days (I assume the first)

Agree. It has been clarified.

. (2) How is it decided which DDF is being used?

The model is able to calculate the amount of snow on the glacier (Reveillet et al., 2017)

(3) How are the values of the DDFs determined ? Are they the same for both glaciers investigated?

The origin of the values of the DDFS has been explained in the new version of the manuscript. The degree-day factors for snow and ice are 0.0035 and 0.0055 m w.e. $K^{-1}d^{-1}$ for Argentière glacier (Reveillet et al., 2017) and 0.0041 and 0.0068 m w.e. $K^{-1}d^{-1}$ for Sarennes glacier (Thibert et al., 2013). The point mass balances are calculated for each elevation, for Argentière and Sarennes glaciers. In addition, we calculated the glacier-wide mass balance of Argentière glacier using the point mass balances for elevation range and geodetic mass balances (Vincent et al., 2009). Parameter k depends on the site elevation to account for the precipitation gradient and is determined from winter balance measurements and precipitation data

(4) How is the model spatially discretized? In elevation bands, on a grid?

It is now explained in the new version of the manuscript

(5) How is temperature and precipitation extrapolated over the different elevations of the glacier ?

The temperature and precipitation are obtained from SAFRAN reanalyses (Durand et al., 2009; Verfaillie et al., 2018) as explained in the Method section. Parameter k used for accumulation depends on the site elevation to account for the precipitation gradient and is determined from winter balance measurements and precipitation data

(6) Wouldn't it make physical sense to set the threshold between solid and liquid precipitation slightly above 0 deg C? In fact, in almost all situations, snow has not transitioned into rain exactly at the melting point.

Yes, it could make physical sense to set the threshold at a value different from the melting point. Many numerical experiments have been done in (Reveillet et al., 2017) and (Thibert et al., 2013). It is important to simulate the surface mass balance. However, the impact on the sensitivity of MB to meteorological variable is neglictible.

Line 71: Well, probably the agreement is good because the model has been calibrated accordingly. More details on the cal-val procedure and the performance of the model (including RMSE, bias with observations) is needed.

In the new version of the manuscript, we added some information: "The degree-day factors for snow and ice are 0.0035 and 0.0055 m w.e. $K^{-1}d^{-1}$ for Argentière glacier (Reveillet et al., 2017) and 0.0041 and 0.0068 m w.e. $K^{-1}d^{-1}$ for Sarennes glacier (Thibert et al., 2013). The calibration and validation of these factors have been done in (Reveillet et al., 2017) and (Thibert et al., 2013)."
It is not possible to provide more details in the present paper (Brief Communication) but further information can be found in these papers. For instance, for Sarennes modelling, Pearson R=0.93, RMS=0.44 m w.e., average deviation = 0.36 m w.e. and bias with observations: -3.3 cm w.e. (model minus data)

Line 71: "using THESE data" – which data are you referring to here?

We replaced the sentence in the new version: "Using these reconstructed MB…"

Line 73: Why not the median elevation? E.g. for Sarennes the elevation chosen in likely above the median.

As seen in Figure 3, the calculations have been done also at 3250 m a.s.l., 2750 m, 2450 m and over the entire surface of the glacier.

Line 75: The approach of the T and P anomalies needs to be better described. So, the anomaly is the same for every day of the year?

The anomaly is a shift of the mean of the distribution of the original data in temperatures and winter balances. We kept the same distribution around the means to reproduce the year-to-year variability.

A new sentence has been added in the new version of the manuscript : « The anomaly is generated as a shift (increment/decrement) of the mean of the distribution of the original data in temperatures and winter balances. The distribution around the means is unchanged (same year-to-year variability)."

Line 82: The use of synthetic temperature data comes very abruptly. It has not been introduced in the methods. How is this synthetic series constructed, i.e. what is it based on. One problem with degree-day models that might be mentioned here or in the discussion is that calibrated parameters are often related to the characteristics of the series used. I.e. if shifting to a synthetic series, this might result in an invalidity of parameters (this might also be the case for ANN approaches). In any case, this would not question the sensitivity tests performed here but transferring the result back to real conditions is not straight-forward.

We added the following sentence in the new version of the manuscript: "The reference scenario (unforced temperature and winter balance reference conditions) of synthetic data is typical for a location in the upper ablation area of an Alpine glacier. "
Further information are given in the following sentence: "Runs of our PDD model on synthetic data under different conditions of winter balance (Fig. 5) used a reference scenario of 1,700 mm of winter balance changed by increments of ±300 mm in precipitation". Temperature data are shown in Figure 4a, typical for a course of atmospheric temperature from spring to autumn around 3000 m of elevation in the Alps. We use PDD factors for snow and ice from Thibert et al. (2013).

Line 97: Indeed, this is a very interesting finding, and it is important to state here.

Right.

Line 111: Also here, I agree – this question in really justified.

Right.

Line 117: The wording "refute" is too strong in my opinion. This study adds an important precision / an emphasis on one aspect of the study by Bolibar et al (2022) but it does not refute the findings of that study in general.

We changed the wording: "These results question those of Bolibar et al. (2022), which argue that temperature-index models provide only linear relationships between positive degree-days (PDDs), solid precipitation and SMB."

Line 125: Even though I agree with this statement (see above) I find it somewhat inappropriate to ask this in the conclusion of this (formally) fully independent paper, and would thus rather omit it, or strongly reformulate. The similarity to the figure by Bolibar et al (2022) and the ANN approach presented there is really intriguing! However, I suggest making this figure and the presentation of these results (that are crucial to the study) more consistent: Please use the same ranges of the values (both x and y-axis) for both glaciers. How were these ranges determined ? Wouldn't it make sense to test exactly the same ranges as Bolibar et al (2022) ? In addition, I do not fully understand why the

authors decided to only display results for a selected elevation while results for the entire glacier (see Fig. 3) would be available. This should be better motivated.

. *The sentence "We would suggest testing the capability of an ANN to capture nonlinearity by comparing its results with that of the GloGEM Positive Degree-Day (PDD) model that they used in their paper." has been removed from the new version of the manuscript according to your suggestion.*

. *About the ranges of the values of anomalies (both x and y-axis), it is not easy to compare the magnitude of sensitivities obtained in our study and found by Bolibar et al. (2022). Indeed, in the paper of Bolibar et al. (2022), the anomalies are calculated and averaged (i) from 660 glaciers covering a wide range of elevations, (ii) from glacier-wide mass blances of these glaciers, (iii) over the 1967-2015 period.*
As seen in Figure 3, the response of annual mass balance to air temperature and to winter accumulation is different according to the elevation. The magnitude of these sensitivities are different. It is also different from a glacier to another one.
The ranges of the anomalies are different but the anomalies covered in our analyses are sufficient to highlight nonlinearities.

. *Why did we decide to display results for selected elevations (or point mass balances)?*
The purpose of our study is to show that responses in MB are not linear in response to temperature or precipitation changes even using a simple degree-day model. We used the point mass balances because they are free from dynamics impact (surface changes). The surface changes which influence the glacier-wide mass balance can lead to additional non-linearities. But here, our topic is to discuss the response of surface mass balance using TI model only.

Fig 4/5: The bottom panels should by labelled "Cumulative daily mass balance (m w.e.)". In my opinion. "Mass balance (m w.e. a-1)" is not correct in this context as a daily time series is shown.

The changes have been done.