Reply to Referee 1

First of all, thank you very much for reviewing our manuscript in detail and giving us very useful feedback. In what follows, we respond to your comments and questions, point by point, and propose several changes to the manuscript. We consider that these changes will substantially improve the quality and clarity of our manuscript.

In order to improve the readability of our replies we applied a color/type coding to discriminate our replies from the referee's comments. We have attached our replies as a pdf document since color coding is not available in the browser based text editor.

Color/type coding:
Comment by the referee.
Reply from the authors.

General comments

The authors develop regression models to predict the magnitudes of interglacial and glacials over the last 800ka, as quantified by maxima and minima in benthic $\delta^{18}O$. *They first derive a series of models for interglacial intensity as functions of previous glacial strength* $\delta^{18}O_{max}$ *and half-year summer caloric insolation at 65°N and 65°S temporally integrated across the termination,* I_S *and* I_N.

They apply BIC to convincingly conclude that all three of these inputs are necessary and able to explain 89% of the variance in interglacial intensities. A model based on $I_{AV} = (I_N+I_S)/2$ *is preferred, and is significantly better than a model based on only* I_N.

To predict glacial intensity, the authors build a more complex regression model that is a function of i) previous interglacial strength $\delta^{18}O_{min}$*, ii) a temporal term that depends upon the length of the glacial and assumes a linear relaxation of* $\delta^{18}O$ *towards* $\delta^{18}O_{min} + \beta_1$ *with a timescale of 25kyr, and iii) a second temporal term, being the time during the glacial when caloric summer insolation at 65°N is below an empirical threshold. Although this model explains 86% of the variance in glacial strength, I have some concerns that it is overly complex given the small size of the training dataset (11 data points).*

The regression relationships are used to decompose the dependence of interglacial/glacial strengths into the different driving factors. The authors conclude that increased obliquity, which drives the variability in I_{AV}, explains the stronger interglacials after 430ka, at which time the insolation term switches from being a negative contribution to a positive contribution to interglacial strength.

Thank you for summarizing our results. We address your concern about the complexity of the model for glacial strengths below.

The work is interesting, novel and within the scope of CP. The manuscript is clearly written and appropriately referenced. Related work is credited, though the paper would benefit from discussion of related work by the lead author, see below. The data (benthic $\delta^{18}O$, temporally integrated summer insolation at 65°N and 65°S I_N and I_S, the duration of the glacial T, and the time during which insolation is below the threshold L) are clearly defined and the sources referenced, and the work is therefore reproducible. However, it would be useful if these postprocessed data were included as supplementary material. I for one would have been interested to spend a few hours exploring these data but did not have the time to reproduce them from scratch.

Thank you for your interest and we apologise that you were not able to carry out the tests because the data were not readily available. We will provide data and some programing codes used in this work as Supplementary data.

Specific comments

Why was the period only after 800ka chosen? The LR04 stack and insolation data extend back far earlier that this, and it seems a potentially missed opportunity for additional training data, at least going back a couple of interglacials to the Mid Pleistocene Revolution?

Thank you for raising this point. We have focused on the last 800 kyr for two reasons. (1) While there is a broad consensus on which δ^{18}O-peaks correspond to interglacials (and which do not) over the last 800 kyr [Past Interglacials Working Group of PAGES, 2016], there is comparably larger uncertainty as well as debate in the classification before 800 kyr BP [Tzedakis et al. 2017; Köhler et al. 2020]. (2) A single model with the same coefficients would not work throughout the time interval across the Mid-Pleistocene Transition (MPT) (~900 kyr BP [Elderfield et al. 2012] or 1250−700 kyr BP [Clark et al. 2006]). This is not surprising, as it is clear that the data show a change in the response to similar orbital forcing before and after the MPT. Actually we have conducted some preliminary analyses for the extension of the model across the MPT to see whether the same models, with or without changed parameters, can be used. However, the results are too much to be added in this article and deserve further investigation. Thus, we would like to address this in a future study. We will mention this in the revised article.

There is some conceptual overlap with a recent publication by the lead author (Mitsui and Boers, 2021, QSR), which used machine learning to similarly conclude that the MBE can be explained by increased obliquity. Some discussion of the distinctions and what this new paper brings would be useful.

Thank you for pointing out this. We will add the following sentence in the revised manuscript in Discussion (paragraph entitled MBE). "Recently Mitsui and Boers (2021) have constructed an Artificial Neural Network model that performs a short-term prediction of $\delta^{18}O$. Through sensitivity analyses, they concluded that the intensification of interglacials across the MBE is attributed to the amplitude increase in the obliquity forcing. This is in line with the present result. On the other hand, the present linear regression model is more physically interpretable and more precise in predicting $\delta^{18}O_{min}$ than the Neural Network model in Mitsui and Boers (2021)."

The regressions for interglacial intensities are simple and convincing. However, the regression for glacial intensities would benefit from some additional explanation and sensitivities.

$$\delta^{18}O_{max}-\delta^{18}O_{min} = \beta_0 + \beta_1\left(1 - e^{-\frac{T}{25}}\right) + \beta_2 L.$$

This equation contains two hidden parameters, i.e. the 25kyr timescale and the empirical insolation threshold of 5.735 GJ m^{-2} . This means we have an equation with five parameters which is being fitted to 11 data points and suggests some risk of overfitting. This potential concern should be discussed.

Thank you for pointing out this. There are indeed 5 potential parameters in total. However, since the 25-kyr timescale is selected based on a large number of data points (LR04 series) in Fig. 5a, this parameter has a different status from the other 4 parameters. Also in the adopted model, the parameter β_0 is not used to fit the data. Therefore, really free the parameters involved in fitting 11 data points are three: β_1, β_2, and the threshold 5.735. We will clarify this in the revised manuscript. In machine learning, the risk of overfitting is often assessed by cross-validation. In the revised manuscript, we will show, with the leave-one-out cross validation, that the risk of overfitting is low.

How was the empirical threshold of 5.735 GJ m^{-2} *that is used to calculate L determined? It looks like ~95% confidence to yield a positive d18O gradient, which seems reasonable enough but all the same a little arbitrary? More importantly, how sensitive is the model to this choice and are the conclusions robust with respect to the uncertainty in this value?*

In the revised manuscript, we will provide a supplementary figure for a sensitivity analysis with respect to the threshold value.

The second and third terms both represent a form of time dependency (could the third term in effect be a correction for the uncertainty in τ*, which is fixed at 25, but lies between 10 and 50 kyr, perhaps depending upon the period of low insolation?). It would feel more natural (to me at least) to instead have separate terms in time and energy. The authors note that the model is rather insensitive to this choice in Figure S2, so I wonder why they chose the model with 'time below threshold' rather than 'integrated insolation below threshold'?*

It is because the model in Fig. 4 reproduces the intensities of strong glacials, including MIS12, relatively well, while the slightly different model in Fig. S2 does not reproduce the level of MIS12 well. However, the correlation coefficients of Figs. 4 and S2 are indistinguishable. We mention this point more explicitly.

Related, in the S2 version of the model it's not clear to me that the insolation threshold is necessarily needed. Would a simple integral of the insolation from tmin to tmax generate a useful model? If so, this would eliminate the need for the threshold parameter and would make a simpler and more convincing model.

Following this suggestion, we have investigated the model with the simple integral of the insolation from t_{min} to t_{max}. However, we couldn't get a result better than or comparable with the models already in the manuscript. Thus we have concluded that a nonlinear response to the lower insolation spells is essential to model the glacial intensity. In that case, introducing a threshold is the simplest way to represent the nonlinearity. We will mention this point in the revised manuscript.

Line 36, missing "," after δ18O.

We add the comma: "benthic $\delta^{18}O$, atmospheric CO_2".

Line 186 "between 2 and 4 parameters". I'm not certain whether you are neglecting τ, *threshold insolation or* β_0 *(here. I guess* β_0 *(as it is not favoured by BIC, but this worth clarifying.*

We will correct this.

Table 2. R^2 *of 0.99 for "Without intercept" model looks wrong, it should be ~0.86?*

Yes, it should be 0.86. Thank you for this comment.