Review of
Insolation evolution and ice volume legacies determine interglacial and glacial intensity
by Mitsui, Tzedakis and Wolff

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 $\delta^{18}O_{min}$ + β_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.

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 post-processed 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.

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?

The 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.

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

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

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.

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?

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'?

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.

Technical corrections

Line 36, missing "," after $\delta^{18}O$.

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.

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