Reply to Referee 2

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.

Based on existing data sets - the LR04 stack and the caloric summer half year insolation - and multiple linear regression models, this study concludes that (1) the interglacial intensity of the last 800 kyr depends on the strength of the previous glacial and summer insolation at high latitudes in both hemispheres, (2) the MBE can be explained by the larger amplitude of obliquity cycles after 430 kyr, and (3) the glacial intensity depends on the strength of the previous interglacial, the time elapsed from it and the evolution of boreal summer insolation. It provides some interesting ideas in explaining the glacial and interglacial intensity. However, I have several major concerns on the methodology used in this study which prevent me to be convinced of the conclusions. Hope the authors will provide more explanations and clarifications.

Specific comments and questions:

The authors use the LR04 stack as a reference and define the glacial and interglacial intensity by using the delta 18O max and min. In their linear regression models, summer insolation is involved in predicting the delta 18O max and min. As the LR04 stack is orbitally tuned, I wonder to which extent the results are influenced by circular reasoning and whether the comparison between different data sets in Fig2 makes sense.

First, we have assumed that the orbital tuning in the LR04 $\delta^{18}O$ stack record is right, at least on orbital time scales. Thus, we take it for granted which insolation peak induces which interglacial. Under this assumption, we have explored the relationships between the amplitude (not the timing) of $\delta^{18}O$ peaks and the insolation forcing.

Taking first the model for the interglacial intensities, there is no circular reasoning in the calculation of the two insolation integrals I_N and I_S, since they are calculated purely from the insolation curve. The values of $\delta^{18}O_{min}$ and $\delta^{18}O_{max}$ can be affected by the way benthic records are averaged and combined in constructing the LR04 stack. However, comparison with the more recent probabilistic stack of Ahn et al. (2017) and with the Shackleton S05 eastern Equatorial Pacific composite record (see Tzedakis et al., 2017) does not show any major deviations in interglacial and glacial amplitudes over the last 800 kyr. Moreover, the model does not rely on the absolute ages and use only isotopic levels. Thus we consider that the effect of orbital tuning is minimal in our model for interglacial intensities.

In the model for the glacial intensities, the absolute ages of $\delta^{18}O_{min}$ and $\delta^{18}O_{max}$ are involved in the model. Thus, the orbital tuning could affect our result, but it is not correct to say that our reasoning is circular, since the amplitude of peaks (which we address here) was not involved in the age tuning.

In the revised manuscript, we will address the fact that we have assumed the orbital tuning, and our results must be tested when a new better age model appears.

2. CO2 is an important factor in the climate system, but it is not considered in the regression models in predicting the glacial and interglacial intensity.

Of course, there is no doubt that CO_2 is an important factor in the climate system. However, if we consider that the orbital forcing is the only external driver of the system, then CO_2 is a feedback, and cannot be used as a predictor of other climate variables. It would be rather simple and not very novel to predict such variables (including $\delta^{18}O_{min}$ (or $\delta^{18}O_{max}$)) from CO_2, but this merely begs the question of what drives CO_2 concentrations. Instead, given that the $\delta^{18}O$ (convolved ice volume and deep-water temperature signal) is a robust integrated metric of interglacial and glacial intensities (Past Interglacials Working Group of PAGES, 2016), we try to predict $\delta^{18}O_{min}$ or $\delta^{18}O_{max}$ based solely on the insolation curve. In other words we consider that the effect of CO_2 is reflected in the outcomes $\delta^{18}O_{min}$ and $\delta^{18}O_{max}$, and in our discussion we consider the role that CO_2 may be playing in the mechanistic link between insolation and $\delta^{18}O_{min}$ or $\delta^{18}O_{max}$.

3. There are many assumptions made artificially without clear physical meaning. This makes the study appear more like a mathematical game.

We admit that our models are mathematical. Nevertheless, we believe that our models elucidate physical elements that would have to be taken into account in explaining or simulating interglacial or glacial intensities.

For example, what is the physical meaning of averaging the 65N and 65S summer insolation,

We agree that the average of the summer insolation at 65N and 65S, is itself conceptual. In our model for interglacial intensity ($\delta^{18}\mathrm{O}_{\mathrm{min}}$), we consider each of the insolation terms separately, and we later discuss the role that each might play. We find that the best models have rather similar coefficients for each of the two insolation terms, and that a more parsimonious model (with fewer parameters) treats them as having the same coefficient. We then note that the average closely follows obliquity which allows us to discuss our results in terms of this easily understood factor.

why the threshold value 5.735 GJm-2 is chosen, what is its physical meaning,

The threshold is a parameter introduced to simply model that $\delta^{18}O$ increases rapidly when the insolation level is low (Fig. 5b). However, the value 5.735 GJm^{-2} is chosen to have a good fit. We will explicitly clarify this point in the revised manuscript.

what is the reasoning of the assumptions on the relation between delta 18O min and mas (line 174-176; line 132).

In the model for glacial amplitude we assume that the $\delta^{18}O_{max}$ value depends also on whether there is remaining ice (represented by $\delta^{18}\mathrm{O}_{\mathrm{min}}$) in the previous interglacial. In the model for interglacial amplitude, we simply note the observation by previous authors that strong interglacials tend to follow strong glacials, and we find that this is indeed a useful predictor. In the discussion we consider the possible physical basis for this.

4. The authors attribute the MBE in the LR04 stack to the amplitude change of obliquity, but the physical mechanism is not clear. Moreover, obliquity has a periodicity of 40 kyr, but the interglacial peaks are separated by ~100 kyr. It is unclear to me how the two could be linked.

The problem of ~100-kyr periodicity has been addressed in previous works. Our previous study with Michel Crucifix shows that every interglacial appears when the caloric summer half-year insolation at 65N exceeds a threshold that decreases with elapsed time since the precious interglacial onset (Tzedakis et al., 2017). This explains how one or two obliquity cycles are skipped without having terminations. The present study (on how the interglacial/glacial strength is determined) is partly based on the previous work (on when interglacials occur). In the revised manuscript, we will mention the model in the previous work on when interglacials occur (Tzedakis et al., 2017) in order to make clear the linkage to the present study.

With respect to the shift in interglacial intensities at the MBE, we suggest that the this may be attributed to the long 1.2 million year cycle that modulates the amplitude of obliquity (so greater obliquity maxima after 430 ka led to stronger interglacials).

There is also MBE in the interglacial CO2 concentration. I wonder why CO2 is not mentioned in explaining the MBE.

In fact, we have mentioned the role of CO_2 in Introduction and Discussion (lines 47, 199, 204, 224) citing literature. However as we have discussed above, the whole purpose of the present paper is to predict the amplitude of interglacials and glacials from the insolation, with CO_2 as part of the mechanism leading to the observed climate variables (the benthic $\delta^{18}O$). That is, CO_2 is considered as an agent in-between insolation and $\delta^{18}O$ change. We will mention this last point in Data and Methods in the revised manuscript.

5. Can the regression models based on the last 800kyr data explain the glacial and interglacial intensity before 800kyr?

Thank you for asking this point. Actually, we have investigated if the same form of the model works also before 800 kyr BP. Our preliminary results are positive. However, it appears that prolongation and intensification of glacial-interglacial cycles across the Mid-Pleistocene Transition (MPT) does not allow the use of the same coefficients in the model across the MPT. This makes the modelling rather complicated. This will be the subject of a future study. We will mention this in the revised article.

6. There is no real conclusion section.

The last section was called Summary and Discussion, rather than Conclusion, because the former name represents the section more suitably. In Summary and Discussion, we have concluding remarks, e.g., the very last paragraph, Lines 241-244. Nevertheless, we make concluding statements clearer in the revised manuscript.