Reply to the comments by Dr. Holger Kantz (referee)

Takahito Mitsui[1], Peter Ditlevsen[2], Niklas Boers[3,4,5], and Michel Crucifix[5]

[1]Faculty of Health Data Science, Juntendo Univerity, Urayasu, Chiba, Japan
[2]Niels Bohr Institute, University of Copenhagen, Copenhagen, Denmark
[3]Earth System Modelling, School of Engineering & Design, Technical University of Munich, Munich, Germany
[4]Potsdam Institute for Climate Impact Research, Member of the Leibniz Association, Potsdam, Germany
[5]Earth and Life Institute, Université catholique de Louvain, Louvain-la-Neuve, Belgium

Correspondence: Takahito Mitsui (takahito321@gmail.com)

Thank you very much for reviewing our manuscript in detail and giving us very useful feedback. Below, we reply to your comments and questions (shown in *italic*), and propose several changes to the manuscript (shown in **bold**). We believe that these revisions will enhance the quality and clarity of our work.

Main issue

I missed (or may have overlooked the discussion of) only one aspect in this issue of the 100 kyr cycles: The lack of spectral power at 100 kyr in the 65N insolation time series means that the driving signal lacks this frequency component. Nonetheless they state in line 60 that 'proximity of the intrinsic time scale .... to the 100 kyr periodicity of the eccentricity cycles' is relevant, i.e., they consider the 100 kyr period of the driver to be due to eccentricity. This seems to be in contradiction to the fact that in the PSD of 65N insolation there is no enhanced power in this frequency band, and they also cite Berger who proposed a kind of beating frequency of the 23.7 and 19 kyr modes to be responsible for the 100 kyr cycle.

Thank you for pointing out this aspect. The apparent contradiction is resolved as follows: The Earth system does not simply respond to the precession cycles but responds to the beat frequency generated by the addition of the 23.7- and 19-kyr precession cycles. The beat frequency is strictly equal to the frequency of the 95-kyr eccentricity cycles (cf. $1/19 - 1/23.7 = 1/95$). Thus, the nonlinear, subharmonic-type, response to the 23.7- and 19-kyr precession cycles is physically similar to a response to the 95-kyr eccentricity cycles. In the Introduction of our discussion paper (lines 27–30), we have briefly mentioned the above fact. **However, in the revised paper, we will make the corresponding text clearer and will mention the above solution to the apparent contradiction again in the summary paragraph**.

The fact that the eccentricity period of 95 kyr is close to the 100 kyr, is this essential or just by chance? Perhaps the authors can comment on this.

We consider the so-called 100-kyr cycles to be a simplified characterization of the ice age cycles, whose mean periodicity is closer to 95 kyr, as observed in the power spectra of the records (Fig. 1). Thus, there is no exact 100-kyr cycles.[1] **In the revised**

[1]The eccentricity has also a periodicity of 98. 857 kyr [Laskar et al. 2005] (or 99.590 kyr in Berger et al. 2005), which is closer to 100 kyr than 95 kyr. However, the power of the 98. 857 kyr cycles is much less than that of 95 kyr cycles.

manuscript, we state that while the ice age cycles are generally described as having a roughly 100-kyr periodicity, they may be more closely associated with the 95-kyr eccentricity cycles.

Other minor issues

Line 25, "Hencefore, the $\approx$ 100 glacial cycles...": kyr is missing.
It was a typo. **We will add '-kyr'**.

Line 117: "... the VCV18 model CANNOT be qualified as ... synchronization"?
It was our mistake. **We will change 'can' to 'cannot'.**

Line 155: What is the difference between $I(t)$ and $f(t)$? In line 86 it is said "$I(t)$ is the standardized summer solstice insolation anomaly at 65N", as well as in line 107. $f(t)$ is defined in line 128 as '65N summer solstice insolation anomaly'. Perhaps the authors can invest one more line to clarify this (also where the mean over the past 1 Myr appears and what f_1, f_2 are).

Thank you for pointing out this. $I(t)$ is the standardized anomaly scaled by its standard deviation, and $f(t)$ is just an anomaly NOT scaled by its standard deviation. **In the revised paper, we will add the following sentence: "Note that $f(t)$ is an anomaly that is not scaled by its standard deviation, different from $I(t)$ in the previous two models." We also clarify the critical insolation anomalies f_1 and f_2, between which the system has two glacial and interglacial attractors.**

We would like to thank you again for your thoughtful comments and very useful feedback.