Rebuttal to *Interactive comment on* "Bispectra of climate cycles show how ice ages are fuelled" by Diederik Liebrand and Anouk T. M. de Bakker

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1. General comments

Drs. Liebrand and de Bakker provided here new, original statistical analyses of the LR04 $\delta^{18}O$ stacking to document the nonlinear interactions between the Milankovitch cycles which lead to the generation of new cycles in the palaeoclimatic data and lead to power transfer from the precession band (dominant in the insolation series) to the obliquity and to the 100-kyr eccentricity cycles. In particular, bispectra are used to observe nonlinearities between the insolation forcing and the $\delta^{18}O$ series, which is an excellent and original idea in palaeoclimatology.

In addition to R2's summary, we would like to emphasize that we apply bispectral analysis only to the LR04 record, and thereby describe nonlinear interactions among climate frequencies as present in this data set (i.e., a transform of the asymmetric cycle geometry), and not between insolation and $\delta^{18}O$. The comparison to insolation forcing is merely qualitative (see Fig. 6), and is presented for comparison based on a theoretical/physical understanding of astronomical climate forcing.

However, I find section 3 of the manuscript hard to read to someone who is not familiar with the reading the interpretation of bispectra despite I could see the authors made many efforts to make their paper accessible.

To clarify the interpretation of bispectra we provide a thoughtfully constructed reading key in Section 2.3.2. ("Interpreting the bispectrum"). In this section we also refer to Figure 4, to support the interpretation of the bispectrum. This information is crucial for understanding the results (Section 3) and the interpretation of the bispectra.

In section 3.2., I do not understand what is reference to calculate the gains and the losses of energy and I do not understand how the authors can find this information in the bispectra. This step must be clear for any reader to then completely follow the result description in section 3.4.

To read Section 3.2., and understand how gains and losses are computed we refer R2 (and the interested reader) to Section 2.3.2., in which we explain in more plain language how the bispectrum is interpreted, and how the bispectral notation should be read.

In short: an energy gain (EG) is depicted by the warm colours (red), whereas an energy loss (EL) is represented by cold colours (blue). An energy gain at one frequency results in energy losses at two other frequencies and vice versa: $EG_{f1}+EG_{f2} = EL_{f3}$ and $EL_{f1}+EL_{f2} = EG_{f3}$. The "energy" reference (i.e., amount of energy that is exchanged in such a nonlinear triad interaction) is given as a function of the variability in benthic foraminiferal $\delta^{18}O$, expressed in ‰^{3} kyr^{-2} in the bispectrum, and as ‰^{3} when integrating over the bispectrum (as is described in Section 3.4.).

We have added information about these energy units to Section 2.3.1. and Section 2.4.

I can see that bispectra document the energy (or power) transfers and the evolutive spectral analyses seem to document these power transfers very clearly. However, I do not understand how the bispectra contribute in understanding the mechanisms of nonlinearities in the $\delta^{18}O$ already evoked in prior publications and this needs to be clarified.

To date, no similarly detailed (i.e., in a time-evolutive manner) description of nonlinear energy transfers among Pliocene and Pleistocene climate cycles is available. The detailed documentation of these transfers presented in this study 'show how' (i.e., through which cycle-cycle interactions) energy is transferred from the insolation frequencies (mainly precession) to those of the ice ages (40, 80, 120, and 95 kyr), and how these transfers evolve through time. In light of these new results, we deem it valuable to tentatively link them to mechanisms that have previously been proposed in the literature. In our opinion, it would be a missed opportunity not to (at least) attempt to draw further conclusions about potential nonlinear mechanisms, given the best available description of nonlinear energy transfers.

A suggestion for a follow-up study would be to test with fully coupled climate-ice sheet models which mechanisms correspond to the specific frequency interactions we observe in the LR04 stack. We have added this suggestion to the Outlook (see the third bullet point in Section 6).

In summary, much clarification is needed to allow a larger community to access this enthusing way to observe cycles in sedimentary series and observe their interactions. I thus suggest this manuscript deserves to be published after revisions will be done.

On this particular point (i.e., "much clarification"), we disagree with R2. Section 2.3. is concerned with the interpretation of bispectra and was designed with great care. It provides an explanation of the bispectrum, and how to read one. This explanation is more detailed than most existing papers on bispectra. Section 2.3. is written with the specific aim of explaining the bispectrum to the nonexpert, and we believe that after reading this section, the remainder of the text is accessible to most palaeoclimatologists/-oceanographers.

The authors can find more specific comments below:

2. Specific comments

Throughout the manuscript, the term "energy transfer" is used. What do the authors refer to when they use this terminology? This must be more clearly stated, unless I missed it in the manuscript.

To further clarify what is meant by "energy transfers" in this particular case study on the LR04 record, we have added a sentence to Section 2.3.1. and to Section 2.4.1.

In page 3, line 26, the author mention they used SiZer to resample every 1 kyr. What is the method used by SiZer to resample? Is it a linear resampling? Is it another method of resampling? Can the authors write exactly the method of resampling? Because it can impact the spectrum at high frequencies.

The SiZer method computes the statistically significant zero crossings of the first and second derivatives of an unevenly sampled time series, to compute a 'family of smooths' that fit these criteria. We used the raw data of the LR04 stack to compute these smooths and selected the smooth (out of 41 smooths) that preserved the most structure in the data, given the 1 kyr resampling resolution. SiZer smooths are not linear interpolations. We refer to Chaudhuri and Marron (1999) for a more detailed description of the SiZer method.

We added information about our smooth selection criteria to Section 2.1.

With respect to the impact on higher frequencies: The highest frequency considered in this study is 100 Myr^{-1}, equivalent to a periodicity of 10 kyr. A resampling resolution of 1 kyr (1000 Myr^{-1}) yields a Nyquist frequency of 500 Myr^{-1}, which is well above that of the cycle frequencies considered in this study (i.e., 0 to 100 Myr^{-1}). Furthermore, in an earlier stage of this study, we have performed sensitivity tests for different resampling resolutions (not included in the current study or supplements), and found no difference on the astronomical frequencies considered here.

In page 4, line 13, what do the authors call "time averaging operator"?

This is the window length considered. In this study, most often 668 kyr long windows were used (apart from in the supplements where 500 kyr and 1000 kyr long window lengths are considered).

We have added "(i.e., window length)" to the sentence.

Section 3.2. ("Bispectra of Pliocene and Pleistocene climate cycles") is hard to follow in my point of view at least for the following reasons:

The authors mention gain or loss of energy. Gain or loss should be a difference compared to a reference. What is the reference used to calculate these gains and losses?

See our rebuttal to previous comments by R2 (i.e., the third "General Comment" and first "Specific Comment")

The authors mention positive or negative interactions, e.g. (page 8, lines 24-25): " negative interactions are concentrated at and between triads along the lines from $B_p^{Im}(40\uparrow, \infty\uparrow, 40\downarrow)$ to $B_p^{Im}(40\uparrow, 40\uparrow, 20\downarrow)$". I do not know where to observe this in Figure 4. Can the authors either explain this with a theoretical example easy to understand prior to the real data or at least show where to observe this in Figure 4?

In section 2.3.2. we explain the bispectral notation. This information explains how the bispectrum is read and interpreted. In this section we also give a simple example, not theoretical, but based on Figure 4a.

From these two examples, I think much effort have to be made to guide step-by-step a reader who is not familiar in the use and interpretation of bispectra. Otherwise section 3.4., which describes the results of bispectra, will remain inaccessible for many readers. So, I suggest more step by step explanation to make easier the observation and the interpretation of the bispectra.

See previous rebuttal comments to R2. (i.e., Section 2.3 is key in understanding Section 3).

In Figure 5, I do not understand what the authors refer to by writing "Input –> "black box" climate –> output". What do the authors mean by "black box" here?

We agree with R2 that the title of this figure caption is a bit cryptic. However, this was done on purpose, with the aim to provoke thought about the workings of the Earth System. "Black box" is a commonly used metaphor for a system of which the inner workings are only partially understood. Earth's climate system is such a "black box". Its past behaviour can only be approximated (by proxy records) or understood theoretically/through modelling. Spectra of insolation represent the climate driver (i.e., input) and the spectra of the benthic $\delta^{18}O$ record constitute the "black box" response (i.e., output).

We have now labelled the corresponding panels in the Figure caption.

Still in Figure 5, I would clearly state what conservative net energy transfer means

We have added text in between brackets for the explanation of panel (b).

In Figures 5 and 6, I would label the frequencies on which energy transfers occur

Both frequencies and periodicities are labelled along the axes. Adding numbers within the figures would, in our opinion, make them more cluttered and less easy to read.

In section 5.2.1. "Based on the bispectral results, we infer that during the Pliocene and Early Pleistocene this predominantly monsoonally-driven precession motor fuels the 40-kyr obliquity-paced ice age cycles, aided by more linear climatic-cryospheric responses resulting from variability in insolation at this periodicity, especially at higher latitudes" » I do not really understand how the authors can deduce that from bispectral analyses. The authors can of course observe transfers of power from the precession to the obliquity band, but how can they link that to the moisture and heat transfer at low latitude? There is a step I do not understand. Comparatively, the interlatitudinal insolation gradient evoked by Bosmans et al. (2015) appears much more intuitive and easier to understand.

The modelling study by Bosmans et al. (2015) is concerned with explaining obliquity signals at low latitudes, and suggests that these signals may originate in the (sub-) tropics. However, we aim to explain the transfer of energy from precession to obliquity cycles in a high latitude land-ice volume dominated climate record. Although obliquity is observed at low latitudes, many of the (sub-) tropical palaeoclimate records remain precession dominated (e.g., monsoonal/loess records, sapropels, caves records, etc.). Therefore, we speculate that insolation changes at the lower latitudes (mainly precession paced) may fuel the transport of heat and moisture to the poles, and the build-up on obliquity time scales (with associated energy transfers from precession to obliquity as documented in the bispectra) of Northern Hemisphere land ice. The study of Bosmans (2015) does not include a dynamic ice sheet and can therefore not capture these hypothesised energy transfers.

I experience the same feeling with section 5.2.2.: how the power transfer observed in the bispectra can help in linking the transfer from the obliquity to the eccentricity with crustal sinking and delayed rebound? I think the authors need to clarify how the bispectra can contribute to the debate

We copy our reply to R1, who also raised this point: Our analysis does not point to crustal sinking as the mechanism. However, we merely state that the bispectral results obtained in this study, are in agreement with a nonlinear mechanism, such as crustal sinking and resonance with eccentricity modulation precession (e.g. following (Pisias et al., 1990; Abe-Ouchi et al., 2013)).

3. Technical corrections

In Figure S3, labels a, b, c in the caption do not correspond with the panels in the figure. Can the author correct that?

We have corrected the figure caption.

4. References

Bosmans, J.H.C, Hilgen, F.J., Tuenter, E., Lourens, L.J., 2015. Obliquity forcing of low-latitude climate. Clim. Past, 11, 1335-1346.

We have added this reference to the manuscript.

Rebuttal references:

Abe-Ouchi, A., Saito, F., Kawamura, K., Raymo, M. E., Okuno, J., Takahashi, K., and Blatter, H.: Insolation-driven 100,000-year glacial cycles and hysteresis of ice-sheet volume, Nature, 500, 190–194, https://doi.org/10.1038/nature12374, 2013.

Bosmans, J. H. C., Hilgen, F. J., Tuenter, E., and Lourens, L. J.: Obliquity forcing of low-latitude climate, Clim Past, 11, 1335–1346, https://doi.org/10.5194/cp-11-1335-2015, 2015.

Chaudhuri, P., and Marron, J. S.: SiZer for exploration of structures in curves, J Am Stat Assoc, 94, 807–823, https://doi.org/10.2307/2669996, 1999.

Pisias, N. G., Mix, A. C., and Zahn, R.: Nonlinear response in the global climate system: evidence from benthic oxygen isotopic record in Core Rc13-110, Paleoceanography, 5, 147–160, https://doi.org/10.1029/PA005i002p00147, 1990.

We would like to take this opportunity to thank M. Martinez for his constructive feedback.