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

Michel Crucifix (Referee) R1

michel.crucifix@uclouvain.be

Received and published: 14 May 2019

1. Summary

The authors present an extensive and systematic application of bispectral analysis to the LR04 benthic foraminifera stack. Bispectral analysis allows one to evidence so-called transfers of energy between different frequencies, and may therefore provide support for interpreting nonlinear phenomena known to occur in a system of which we can observe time series. Sections 1 and 2 are devoted to context and methodology, and the main results are given in section 3. Section 4 briefly comments on the suitability of the approach, and section 5 suggests possible climate mechanisms.

As pointed out by the authors, this is not the first time that bispectral analysis is being applied to palaeoclimatic time series. Earlier attempts are due to Teresa Hagelberg in the early 1990s and it is nice to see here an up-to-date application of this technique, illustrated by carefully prepared figures (key Figures are 6, 7, and 9). I have, however, a number of comments which I believe pertain to quite fundamental issues, but which nevertheless may be addressed by the authors.

We find it interesting to learn that R1 considers Figures 6, 7, and 9 as key. We consider Figures 4, 5, and 8 most informative.

2. Major Comments

1. First, the concepts of "energy" and "energy conservation" need to be clarified. In wave theory, the Fourier energy (square of amplitude) is directly interpretable as kinetic energy. The concept of energy conservation therefore has straightforward meaning. In palaeoclimates, the amplitude of a precession beating is not an energy of that form.

This is a very fundamental point that R1 raises. Energy and energy conservation for paleoclimatic case studies are dependent on the record that is being analysed. Here, we solely focus on the LR04 benthic foraminiferal oxygen isotope stack and "energy" is given as a function of the variability in benthic foraminiferal $\delta^{18}O$, i.e., expressed in ‰^{3} kyr^{-2} in the bispectrum, and as ‰^{3} when integrating over the bispectrum. However, when other palaeoclimatic time series are considered, the "energy" units will change accordingly.

With respect to the LR04 record: this time series is a globally averaged signal of land-ice volumes and deep-sea temperatures combined. Variability in benthic foraminiferal $\delta^{18}O$ is largely the result of a nonlinear response of the climate-cryosphere system to the changes in the distribution of Earth's incoming solar radiation (given in the energy units W m^{-2}), which is often represented by insolation for a particular latitude (e.g. 65º N).

How energy is transferred from Earth's total energy budget (in W s^{-1} , i.e., Joule) into variability in the globally averaged $\delta^{18}O$ record (in ‰ Vienna Pee Dee Belemnite, VPDB), is depending on "…many climatologic and oceanographic, biologic, sedimentologic and lithologic processes…" (see last line of Section 4.2). It is not the purpose of this study to quantify these Earth-internal processes further. We describe "energy" and "energy conservation" merely qualitatively (i.e., translate asymmetry into a loss and a gain at particular frequencies present in the LR04 stack), without scaling them to the power spectrum of the LR04 stack, to the power spectrum of insolation at e.g. 65º N, or to Earth's total energy budget over a given time. These may be objectives for follow-up studies.

> Therefore, why energy transfers should be conservative is not immediately obvious. If I understood correctly, the specific choice of the weight (p. 7, line 2) enforces conservation, but again the physical justification is unclear.

Within the climate system energy losses (i.e., ultimately to space) can occur at numerous stages. These losses will lead to the formation of a particular palaeoclimate record. However, in bispectral analysis and the calculation of the transfer term, these earlier energy losses are not resolved because the bispectrum only considers the available time series, and not what happened beforehand.

When performing bispectral analysis on a specific palaeoclimate record, the conservation of energy during exchanges is assumed (i.e., enforced, obtained, implied) by correcting the energy gains and losses of a particular triad interaction by the values of the frequencies that are involved. This correction is similar/comparable to the Boussinesq scaling used for computing energy exchanges among ocean waves (e.g., (Herbers and Burton, 1997; Herbers et al., 2000)), and allows for qualitative interpretations (i.e., energy gains and losses across frequencies are scaled to one other) (see Section 2.4.1., Fig. 5b, and Fig. 8).

When subsequently making the step to scale these energy exchanges to absolute energy exchanges and make them directly comparable with the time-evolutive gradient of the power spectral density within this specific paleoclimatic record being analysed (i.e., to be able to explain the changes observed within the record through time), the bispectral exchanges have to be corrected for physical processes that play a role in the strength of these exchanges. The dissipation term itself is a completely separate term from the energy conservation that we enforce during triad interactions as documented in the bispectrum (see also Eq. 1 in (Herbers et al., 2000)).

We forego the scaling to absolute transfers here, because of the many unknown/poorly constrained physical, chemical, biological, sedimentological and lithological processes that affect absolute $\delta^{18}O$ values of the globally integrated LR04 record (see Section 4.2). Further research is needed to advance on this point and obtain estimates of the absolute energies that are exchanged.

To further clarify this point, about the assumed energy conservation in nonlinear triad interactions, we have added text ("if we assume a simple coupling coefficient between frequencies") to Section 2.3.1., added "assumed" to Section 3.4.2. and to Section 6., replaced "using" by "assuming" in Section 4.2.

> Similarly, the authors follow the state-of-the art literature and focus on the imaginary part of the bispectrum, but as I understood it the physical rationale for focusing on the imaginary part is in fact grounded in wave theory. Why would we focus on the imaginary part in the present context?

The focus on the imaginary part of the bispectrum is not grounded in wave theory, but in bispectral theory. At equal amplitudes, more energy is transferred among frequencies for time series characterized by asymmetric (imaginary part) than for skewed (real part) wave forms/cycle shapes (approx. an order of magnitude difference). Despite this strong focus on the imaginary part of the bispectrum, we do not rule out a (probably much smaller) contributing role for the real part of the bispectrum in describing (even more) energy transfers. This is a potential topic for future research. (See the first point in the Outlook, i.e., Section 6, and Supp. Fig. 1 and 2).

We agree with R1 that the physical rationale for why nearshore waves are asymmetric is much better understood than why climate cycles are asymmetric. See e.g. the comparison of model to flume/beach data presented in de Bakker et al (2016). In this study, for palaeoclimatic interpretations, we speculate that the asymmetry in the LR04 time series is mainly due to nonlinear (positive) ice feedbacks (albedo, inertia of large ice volumes, land-ice mass-loading threshold) that causes a phase-lag with respect to precession and obliquity, and a phase-coupling with respect to ~110-kyr eccentricity.

We have clarified this point in the text by adding "if time series are dominated by asymmetric wave forms/cycle shapes" to the introduction, and by rephrasing the first bullet point of the Outlook (Section 6).

> Perhaps the reader would be reassured to see the bispectral analysis for typical transformation known to be relevant for palaeoclimate dynamics. What happens with bioturbation (which one might intuitively see as a form of non-conservation, or dissipation)? How does bispectral analysis identify demodulation (precession beating being transformed in a response at the period of the beating). What is happening at a period doubling bifurcation? In other words, we need a user's guide, a reading key of the bispectrum that is well suited to the phenomenology of Pleistocene dynamics. Perhaps these simple examples will also help the reader understand why the focus should be set on the imaginary part of the bispectrum.

The bispectrum is an accepted method in research fields ranging from nearshore waves, neurology, cardiology, to economics, etc. The extension of these (advanced) techniques to palaeoclimatic problems is one of the latest for bispectral applications in this sequence. It is not the purpose of this study to fully educate the reader in bispectral theory, and some background reading/studying may still be required.

We note that the phenomenology of Pleistocene dynamics is highly proxy record dependant (benthic $\delta^{18}O$ in this case). Hence, there is no single user's guide that will suit all palaeoclimatic purposes. How the bispectrum is precisely affected by the issues raised above (bioturbation, demodulations, period doubling bifurcations), falls outside the scope of this study. In general, many of these processes will lead to lower signal-to-noise ratios, and hence, more biased results.

We would like to point R1 (and the interested reader) to the "palaeoclimatic" user's guide provided by Hagelberg et al. (1991) and King (1996), who show synthetic examples of frequency and phase (de-) coupled time series and their bicoherence spectra.

2. Still in relation with the specific phenomenology of palaeoclimate dynamics, it is important to distinguish 'cycle' and 'frequency'. A saw-tooth signal of 100-ka long is the manifestation of one cycle, that is, a succession of events that form a phenomenon (e.g.: the ice-age cycle). Yet the Fourier decomposition of this signal will feature multiple frequencies (an infinite, countable number of them). Hence, a Fourier peak does not necessarily correspond to what we would like to call a 'cycle' or a 'cyclicity' in palaeoclimate dynamics. I am a bit worried about the numerous references to a 28-kyr cycle. Wouldn't it be the main merit of bispectrum analysis to show how frequencies appear in the spectrum and how they are linked to other? In other words, isn't it precisely the purpose of bispectrum analysis to help one distinguish a frequency from a cycle? (if two frequencies are strongly linked, they are part of a same cycle).

We agree with R1. However, we choose to use "cycle", "frequency" and "periodicity" more loosely and interchangeably, mainly for textual purposes. We understand that a single frequency (identified in either spectrum or bispectrum) is not necessarily the same as a cycle (identified in a time series), because many cycles are composites of multiple frequencies, most notably skewed, asymmetric, and kurtose cycles.

Despite the (small) differences in the meanings of cycle and frequency, we prefer our less strict semantics to keep the text varied, readable and accessible. However, to also acknowledge the point of R1, we have re-evaluated the usage of "cycle", "frequency" and "periodicity" throughout the manuscript, and in a few instances changed the wording to make specific references to either time (i.e., cyclic phenomena) or frequency domains clearer. We have also added an explanation to Section 2.3.1., clarifying our intended usage of these words.

3. It is fine in an exploratory paper to focus on one record, here the LR04 stack. However, the possible pitfalls associated with the way the record for this specific application need be better discussed. The chronology of the LR04 stack was established by tuning the record on the output of a simple ice-age model driven by mid-June insolation (the Imbrie and Imbrie 1980 model), with different time constants for the early and late Pleistocene. By design, this approach tends to concentrate power on astronomical bands, with consequences on the bispectrum which are hard to fully anticipate. On the other hand, the process of stacking different records may have unintended effects on the relative weights between the precession and obliquity components (precession being harder to detect, it may be damaged by a stacking process that favours the visible obliquity signal), and, again, consequences on bispectrum hard to anticipate. Precession signals are also relatively more affected than obliquity's by mixing processes such as bioturbation. Hence, I found a bit hasty and not entirely convincing the author's conclusion that

stacked records are the best material for their application (p. 20). Splicing high-resolution, carefully chosen records might in fact be an equally attractive choice.

The main rationale to use the LR04 stack in this study is the high signal-to-noise ratio and relatively accurate and precise ages, given the abovementioned tuning assumptions, of which we are fully aware (see caption to Figure 1). We agree with R1 that this does not solely result in benefits, but also in some loss of signal, especially at the higher (precession) frequencies (see also (Huybers and Wunsch, 2004)).

Therefore, we have deleted "Therefore, we argue that for these purposes data stacks are preferred" from Section 6 (last bullet point). In fact, application of bispectra to individual records may prove fruitful for future studies.

4. I must confess being quite critical about section 5. The mechanisms for the explanation of the findings are unnecessarily speculative and slightly misinformed, and seem to me to do more harm than good to the credibility of the paper. A word about the "precession motor", first. Clearly precession has various possible effects on ice ages dynamics, via the local insolation forcing, possibly the hydrological cycle, why not the carbon or methane cycles. Hence, focusing on monsoon dynamics is unnecessarily reductive. The simulations presented by Werner et al., 2001 suggest that less than 10 % of the precipitation falling on Greenland on in Eastern Canada is of tropical origin. The article is a bit dated but the order of magnitude must be valid. Hence, monsoon might have a direct effect on ice accumulation balance, but the results presented here provide no argument to see it as a dominant one.

The suggested modelling paper by Werner et al, (2001) is mainly concerned with Greenlandic land-ice isotope composition, and does not seem too relevant to our study. During the current interglacial (and those of the past million years or so), Greenland is still largely glaciated. Hence, the moisture source for Greenland is not very relevant in explaining large land-ice volume fluctuations of the past million year (de Boer et al., 2012). The largest land-ice volumes during Middle and Late Pleistocene glacial maxima were located on the North American and Eurasian continents. Hence, moisture sources for these regions during glacial inceptions and maxima may well be largely temperate to (sub-) tropical in origin. Further evidence for the strength of the precession motor comes from lower latitudes (e.g. the sapropels in the Mediterranean), which show the latitudinal migrations of atmospheric (and oceanic) fronts and associated hydroclimate on these shorter, precession time scales (Bosmans et al., 2015).

However, to acknowledge the uncertainty that remains in the understanding of precipitation sources for large land-ice volumes, we have added a reference to Werner et al, (2001), in addition to references to other, more recent modelling studies.

Likewise, the reference to a "resonance of crustal sinking" is, again, unnecessarily sophisticated. Physicists and glaciologists working on ice ages broadly agree that terminations are the manifestation of some 'non-linear effect' expressing the instability glacial maxima, and the debate is about the mechanisms of instability (ice-sheet dynamics, ocean and carbon cycle, tectonic CO2 release). Again, the contributions or relative importance of these mechanisms cannot be investigated on the basis of a single record, whatever analysis technique is being used.

We agree with R1 that 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 modulated precession (e.g. following (Pisias et al., 1990; Abe-Ouchi et al., 2013)).

Finally, the point 5.2.3 about the "climatic and tectonic boundary conditions" is a bit verbose. A quick glance at the LR04 immediately reveals an evolutionary process, which indeed, is being attributed to tectonic changes with perhaps some evolutionary contribution. The authors are citing many references but the context and the purpose of these references is not always clear, and do not relate to an information that bispectrum analysis would have specifically enlightened.

For contextual purposes, we thought to briefly (one paragraph only) address the long-term climatic evolution during the Pliocene and Pleistocene, mainly to set clear boundaries on what the bispectrum can–and what it cannot–help to understand better. Especially the comparison of the LR04 spectrum to the LR04 bispectrum is of relevance. The time-evolutive spectral analysis show long term frequency evolutions that are absent in the time-evolutive bispectral analysis (compare Fig. 5b to 5c), suggesting shifts in the response of the climate system that are unrelated to the nonlinear processes described by the bispectrum.

In summary, how the bispectrum analysis may contribute to the identification of ice-age dynamics needs to be thought of better. It seems that the main (and really nice) contribution of bispectrum is to act as a powerful test of dynamical system models of Pleistocene climate dynamics.

We agree with R1 that the bispectrum may serve as a powerful test of dynamical system models (GCMs or conceptual) and help the understanding of the Pliocene and Pleistocene climate system. However, we disagree with R1, and do not find it "unnecessarily speculative" and "misinformed" (see R1s point 4) to then also interpret the bispectral results in terms of dynamics (i.e., mechanisms), despite the fact that these interpretations are speculative.

Given the most thorough higher order spectral description of nonlinearities during the Pliocene and Pleistocene to date, which we present, some speculation on the mechanisms is in place. In fact, we would perceive it as a missed opportunity not to at least attempt to (speculatively) link these new observations and descriptions of nonlinearities to mechanisms that have been proposed in the literature. Throughout the Discussion, we have made it very clear that these interpretations are speculative at best.

1. p. 4 l. 5: follow THE convention

Corrected.

2. equation 2: what is the meaning of H^3?

This is indeed a mistake. We have changed $As(x) = \frac{\langle H^3(x-\bar{x})\rangle}{\langle (x-\bar{x})^2\rangle^{3/2}}$ into $As(x) = \frac{\langle H(x-\bar{x})^3\rangle}{\langle (x-\bar{x})^2\rangle^{3/2}}$. This typo was unfortunately not noticed and corrected in in the text of Liebrand et al. (2017).We checked, and the computations that were performed in MATLAB use the latter (correct) formula. *H* stands for the Hilbert transform.

3. p. 6 l. 4: the reference to Fig. 4a is not straightforward. Perhaps say in more plain language what the reader is supposed to look at on the Figure.

Reading bispectra is not straightforward indeed, which is why we focus the interpretations of the bispectra on the integrations, which transpose the frequency-frequency domain into the time-frequency domain. We did want to show a few clear examples of bispectra, to familiarize the reader with the analysis underpinning the results that are presented later on in the manuscript.

To aid the understanding of Figure 4, we have restructured Section 2.3.2., and moved last paragraph upward. By reordering this section, we now first explain how to read sum frequencies. This should make the reference to Figure 4a also more accessible. All frequencies and periodicities were already given in bispectral notation, which point the reader to the correct "blue area" in the bispectrum.

4. p. 7 l. 1: "Therefore, we make minimum assumptions and use a coupling coefficient that only corrects for a frequency of W(f 1, f 2) = (f1 + f2)". This seems to be a key passage, of which the implications are not immediately clear to the non-expert. Why does it enforce energy conservation (perhaps this can be explained simply if we consider that the rate of energy loss is counted by cycle), and why having energy conservation allows for "qualitative interpretation"? Again, this links with major comment 1. above, the need to explain in simple term what is "energy", and how the imaginary part of the triad interaction is an interesting qualitative indicator of energy transfers (comment applies also to p.4 l. 6-11).

See our rebuttal to R1's Main Point 1 above.

5. p. 8, l. 19: "Nonsinusoidal cycle shapes are generally a good indicator for the successful application of higher order spectral analysis". Ambiguous sentence. If nonsinusoidal cycles are in the record (quite evidently, late Pleistocene cycles are asymmetric), in what sense does it tell us something about the "successful application" of whatever technique?

Higher order spectra describe nonsinusoidality. However, we agree with R1 that nonsinusoidality on its own, is not sufficient for the successful application of higher order spectra.

We have rephrased this sentence to remove the ambiguity.

6. p. 12: "We only document very minimal direct fuelling of eccentricity-paced climate cycles by precession-paced climate cycles in this zone." Can we imagine that

this result is influenced by the fact that individual precession cycles are poorly resolved? (bioturbation, undesired effects of stacking).

This may well be the case. The LR04 stack is indeed a globally integrated, land-ice volume dominated record, which may have attenuated precession variability compared to other proxy records, and especially compared to insolation variability at any particular latitude. However, despite the likely bias of this proxy to variability at the lower frequencies, we do see energy transfers from precession to obliquity periodicities (e.g. see Zone 5, OOP), and of obliquity to eccentricity periodicities (e.g. see Zone 2, EEO). Therefore, we argue, that the lack of direct "fuelling" of eccentricity variability by precession (see Zone 6, PEP) is a valid observation,

In the Results Chapter, we prefer to observe and describe without too much interpretation, and have therefore left the text as is. In the Outlook (Section 6) we argue that further higher order spectral analyses, on climate time series that are less land-ice volume dominated, may well be insightful.

7. p. 13: The purpose of the reference to Ahn et al., 2017 is not very clear since it seems that the authors have used the original LR04 stack (hence, Lisiecki and Raymo, 2005).

We have rephrased this sentence by replacing "are" with "may be".

8. p. 13: Section 3.4.2: another confusing point for the non-expert. Given that weights where chosen such that energy is conserved, how could energy not be conserved? A numerical artefact?

See our rebuttal to R1's Main Point 1 above.

9. p. 13, l. 17: "A comparison of conservativities indicates that approximately similar amounts of energy are exchanges in interactions involving obliquity, as in those involving eccentricity". typo: exchanges -> exchanged.

Corrected

The meaning could also be clearer. First, conservativity is a non-standard noun which is not defined in the manuscript (the word appears also in legend of Figure 5).

We have now defined "conservativity" (Section 3.4.2.) and rephrased the figure captions of Fig. 9 and Fig. 10 (N.B. not Fig. 5).

Next, are we speaking of interactions with precession, i.e., are we comparing interactions between precession and obliquity, vs precession and eccentricity?

This sentence (starting with "A comparison of conservativities…") refers to Figure 10d, in which we compare conservativities of the recombined zones, that contain at least one precession, obliquity, or eccentricity component (See the first sentence of Section 3.4.3). The answer to R1's question is no.

We have rephrased the text to make this point clearer.

> And, again, some more intuitive meaning of "interaction" in the present context (perhaps with a simple example) would be really helpful.

We added a definition of "triad interaction" to Section 2.3.1. However, Figure 10 shows the recombined zonal integrations over the imaginary part of the bispectrum. Frequencies participating in multiple triad interactions are summed and may therefore no longer be visible.

We have added "(triad)" to this particular sentence, to remind the reader of the link to the bispectrum that underpins these computations of energy exchanges.

10. p. 15. l. 2: There may be some confusion between the notion of "reproducibility" (ability to "reproduce" the results based using the data and methodology printed in the manuscript), and "robustness" (insensitivity of results to methodological aspects seemingly unimportant).

Throughout the text we have replaced "reproduce" with "robust/robustness".

11. Figure 1: what are the contours on the continuous wavelet transform plot?

The black contours represent 95% significance. We have added this information to the figure caption.

12. Figure 5: the first bit is cryptic: "Input → "black box" climate → output"

To clarify this figure caption, we have added the relevant panel call-outs ((a), (b), (c)). The meaning of the figure of well-explained in the rest of the caption.

We are aware that strictly speaking bispectra are an "output" analysis, however, our framing here, as a window into the "black box" response, corresponds to the framing of the paper; namely that bispectra 'show how' ice ages are fuelled.

Again, the application of bispectrum analysis is promising and interesting and I would definitely encourage the readers to revise the manuscript. Not much revision may be needed in fact. Focus on the methodology, provide a good 'reading key' so that the naive reader understands better the meaning and implication of the notion of 'energy transfer' in the specific context of palaeoclimate dynamics, and downplay the mechanistic interpretation, which is too speculative and out of scope. Good luck!

Energy and energy transfer do not have a specific context that relates to "palaeoclimate dynamics". These are bispectral properties specific to each proxy time series. See also our rebuttal to R1's Main Point 1 above.

3. References

Werner M., M. Heimann and G. Hoffmann (2001), Isotopic composition and origin of polar precipitation in present and glacial climate simulations, Tellus B: Chemical and Physical Meteorology, (53) 53–71 doi:10.3402/tellusb.v53i1.16539

We have included this reference in 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., Drijfhout, S. S., Tuenter, E., Hilgen, F. J., and Lourens, L. J.: Response of the North African summer monsoon to precession and obliquity forcings in the EC-Earth GCM, Clim Dynam, 44, 279–297, https://doi.org/10.1007/s00382-014-2260-z, 2015.

de Bakker, A. T. M., Tissier, M. F. S., and Ruessink, B. G.: Beach steepness effects on nonlinear infragravity-wave interactions: A numerical study, J Geophys Res-Oceans, 121, 554–570, https://doi.org/10.1002/2015jc011268, 2016.

de Boer, B., van de Wal, R. S. W., Lourens, L. J., and Bintanja, R.: Transient nature of the Earth's climate and the implications for the interpretation of benthic delta O-18 records, Palaeogeogr Palaeocl, 335, 4–11, 10.1016/j.palaeo.2011.02.001, 2012.

Hagelberg, T., Pisias, N., and Elgar, S.: Linear and nonlinear couplings between orbital forcing and the marine □^{18}O record during the late Neogene, Paleoceanography, 6, 729–746, https://doi.org/10.1029/91PA02281, 1991.

Herbers, T. H. C., and Burton, M. C.: Nonlinear shoaling of directionally spread waves on a beach, J Geophys Res-Oceans, 102, 21101–21114, https://doi.org/10.1029/97jc01581, 1997.

Herbers, T. H. C., Russnogle, N. R., and Elgar, S.: Spectral energy balance of breaking waves within the surf zone, Journal of Physical Oceanography, 30, 2723–2737, https://doi.org/10.1175/1520-0485(2000)030<2723:SEBOBW>2.0.CO;2, 2000.

Huybers, P., and Wunsch, C.: A depth-derived Pleistocene age model: Uncertainty estimates, sedimentation variability, and nonlinear climate change, Paleoceanography, 19, https://www.doi.org/10.1029/2002pa000857, 2004.

King, T.: Quantifying nonlinearity and geometry in time series of climate, Quaternary Science Reviews, 15, 247–266, https://doi.org/10.1016/0277-3791(95)00060-7, 1996.

Liebrand, D., de Bakker, A. T. M., Beddow, H. M., Wilson, P. A., Bohaty, S. M., Ruessink, G., Pälike, H., Batenburg, S. J., Hilgen, F. J., Hodell, D. A., Huck, C. E., Kroon, D., Raffi, I., Saes, M. J. M., van Dijk, A. E., and Lourens, L. J.: Evolution of the early Antarctic ice ages, Proceedings of the National Academy of Sciences of the United States of America, 114, 3867–3872, https://doi.org/10.1073/pnas.1615440114, 2017.

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.

Werner, M., Heimann, M., and Hoffmann, G.: Isotopic composition and origin of polar precipitation in present and glacial climate simulations, Tellus B, 53, 53–71, https://doi.org/10.3402/tellusb.v53i1.16539, 2001.

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