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Climate of the Past Discussions

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 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 non-linear 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.

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. 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. 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?
   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.

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 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).

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 biturbation. 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.

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. 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 CO_2 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. 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. 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.

1. p. 4 l. 5: follow THE convention
2. equation 2: what is the meaning of H^3?
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.
4. p. 7 l. 1: "Therefore, we make minimum assumptions and use a coupling coefficient that only corrects for a frequency of $W_(f1, f2) = (f_1 + f_2)$". 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 ll. 6-11).
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?
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).

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).
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 artifact?
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. 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). Next, are we speaking of interactions with precession, i.e., are we comparing interactions between precession and obliquity, vs precession and eccentricity? And, again, some more intuitive meaning of "interaction" in the present context (perhaps with a simple example) would be really helpful.
10. p. 15. l. 2: Their 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).
11. Figure 1: what are the contours on the continuous wavelet transform plot?
12. Figure 5: the first bit is cryptic: "Input → "black box" climate → output"

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 !

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

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