

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-126

Anonymous Referee #2

Referee comment on "Milankovic Pseudo-cycles Recorded in Sediments and Ice Cores Extracted by Singular Spectrum Analysis" by Fernando Lopes et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-126-RC2>, 2022

Initial note: I should emphasize that I read the comments of reviewer #1, as well as the authors' rebuttal, only after drafting my own report (see below). From both documents, I am strongly concerned whether the authors' own opinion on the scope of their analysis and the type of information that this analysis can provide to the paleoclimate community is compatible with that of the vast majority of climate scientists and, thus, the readership of Climate of the Past.

Despite I do not necessarily agree with all details of the report of reviewer #1, I share their opinion that SSA is used here as a statistical approach, even if the authors strictly oppose to this view. (In the end, the decomposition is related to autocorrelation coefficients unfolded into a matrix, i.e., a basic statistical quantity that is estimated from the data under study.) Hence, I agree and insist that the findings reported need to be evaluated using standard scientific measures applying to any kind of statistical method, i.e., combining a descriptive statistics (identified oscillation modes) with an associated significance assessment. The authors argue in their rebuttal against this view in a way that is worrying me to a large extent, since they appear to miss the essential points of criticism raised by our colleague.

This being said, let me come to the results of my own reading of this manuscript:

The authors present an analysis of periodic components embedded in several paleoclimate records, namely different proxies from the Antarctic Vostok ice core record and the LR04 Plio-Pleistocene benthic oxygen isotope stack, together with some insolation variations and orbital cycles during the last 250 Myr. For this purpose, they utilize a variant of the celebrated singular spectrum analysis to decompose the different time series based on

lagged trajectory matrices and associate the corresponding pairs of eigenvectors(?) with oscillatory modes with well-defined frequency content. The results are then compared among the different time series to highlight the presence of certain orbital cycles and combinations thereof in the paleoclimate records.

From their publication history, the authors appear to be well familiar with the used statistical methodology, which is probably more than they should expect from the average readership of *Climate of the Past*. They may have the feeling that it is sufficient to introduce the iSSA technique by referring to their expressive own publication list; I personally do not think that this is appropriate. To familiarize the readership with the specifications of the method, the method needs to be thoroughly explained as part of the manuscript. I may belong to a minority knowing well about SSA in general and even having it applied in my own previous work, but I am confident that the vast majority of possible readers will not have this background. However, even I do not know what exactly makes up the special feature of the used Iterative SSA without further explanation. The authors are kindly requested to add more details on the statistical methodology to familiarize their readers with the used approach.

Somewhat related to this, my understanding of SSA has always been that it allows to identify periodic components in time series without the normal restriction of Fourier analysis assuming a harmonic shape of oscillations. However, to my best knowledge, it still requires a fixed repetitive pattern (especially with fixed oscillation frequency) for each oscillatory mode. If this is correct, I am worrying about justifying the known non-stationarity of Milankovich components in paleoclimate records, which is well visible in the LR04 dataset in terms of ice age cycles changing from approximately 41 kyr to 100 kyr along with the mid-Pleistocene transition, as barely more than amplitude changes of those (non-harmonic) oscillations. A more explicit discussion of this aspect (along with the restriction to oscillations with a fixed frequency) appears essential for making the presented analysis useful for the paleoclimate community. In essence, this point is important for the motivation of using (i)SSA in this study. Why not employing any other time series decomposition technique that might more flexibly cope with nonstationarity not only in amplitude, but also frequency? The authors motivate the use of iSSA exclusively by the fact that they have used this method successfully to other datasets in the past – in my opinion, a proper motivation would need to start with some (justifiable) working hypothesis on the type of oscillations to be identified.

Even when neglecting the aforementioned issues, I would still need some clarification about the identification of the different oscillatory modes. I suppose that they correspond to pairs of SSA eigenvectors/singular vectors that are in quadrature, yet many of them do likely not only lack physical origin, but may also have low magnitude making them practically indistinguishable from noise. From a statistical perspective, I may argue that one should only be interested in components whose variability amplitude clearly exceeds a certain significance threshold. However, I did not find a word about any significance assessment related to the reported oscillatory modes. If there has been some corresponding assessment that was simply not (yet) reported: what has been the underlying null model? (White noise, correlated noise, etc.?)

Finally, if I accept that the list of oscillatory modes identified for each time series is useful

for some scientific purpose and considers certain significance criteria, I still wonder about the interpretation. In this regard, I found the wording/terminology used by the authors pretty vague or even somewhat confusing and inconsistent – periodicity, cycle, pseudo-cycle, quasi-periodicity, etc. Some of these words appear to have a precise meaning (and especially “quasi-periodic” typically means something distinctively different than what it is used for here), others are more colloquial. I would warmly welcome a more “mechanistic” discussion on which of the cycles are actually “relevant” and of physical origin, which rather occur due to (nonlinear?) combinations of other (non-harmonic) “cycles”, and which may be barely more than statistical artifacts (e.g., affected by the time step of the considered series). Uncovering and explaining this thoroughly would be a strong contribution that could actually be helpful for the readers of this manuscript and the whole community. To this end, I found every “attempt” of providing corresponding hints in this work very vague, to say the least.

In summary, I am not convinced that the paper as it stands now should be published in *Climate of the Past*. I have strong sympathy for this type of analysis, but the way it is reported here it is very technical, not very appropriate for the readership of the journal, and leaves open many important aspects.

Specific comments:

The abstract should reflect the main messages, not a detailed enumeration of all specific results. Please focus on the main insights of this study.

Line 15: What do you define as a “Milankovic periodicity”? Typically, I would associate this term only with the classical major oscillation frequencies of the Earth’s orbital parameters, while the authors seem to generously expand this term to everything they find in their analysis.

The Vostok and LR04 data have undergone a sequence of more or less heavy preprocessing steps (interpolation, stacking) that may have modified their spectral characteristics especially in the higher-frequency part beyond simple age uncertainty effects (e.g., any interpolation to a different time axis necessarily induces correlations). This important source of uncertainty (and potentially error for the presented analysis) needs to be discussed.

The authors report that LR04 is the richest series in terms of the number of embedded oscillatory components. To me, this is not quite surprising: other than the orbital solution by Laskar, the series is not strictly deterministic and not smooth, which likely introduces additional components into the decomposition “just by chance”. Moreover, since it is

considerably longer than the Vostok core, it can also contain more lower-frequency modes than the latter. Both features are in my opinion well traceable in Tab. 1.

What is the insolation time series used by the authors? Summer insolation at 65°N? Please specify!

Line 43: What does plate tectonics have to do with magnetic reversals?

Line 80: What do you mean by "ratio planktonic to ice"?

Figure 2: Why do you use a reverted time axis as compared to Fig. 1?

Line 104: "but not for atmospheric" – atmospheric what?

Line 119: Does Laskar et al. 2004 really use nine planets? Rather eight planets? (Plus Pluto? Other large bodies with larger gravitational effect on the Earth's orbit than this one?)

Lines 135-144: I do not see any relevance of this paragraph for the present analysis, despite elevating the number of self-citations.

Generally, the wording of "a line" for referring to the frequency of any SSA mode is not quite useful.

Line 281-282: This sentence provides an important hint on the nature of the secondary/pseudo-cycles found in this analysis. This should be further detailed.

Line 302: The phase of an oscillation is continuously varying, so this does not need to be mentioned. The fact that the amplitudes of SSA modes can vary with time is important for the present analysis, but the corresponding variations are not really exploited in detail for the reported analysis. What seems to be relevant to me are slow modulations of the amplitudes, which could indicate some kind of cross-frequency coupling between different modes/processes.

Line 308: What do you mean by "response functions of the various components"?

Lines 336-343: This is not a result of the present work and therefore completely irrelevant for the conclusions.

In general, the conclusions section leaves the same impression as the abstract, being full of technical details making the reader lost with trying to identify the key findings of the paper. In any case, this part reads very repetitive.

Can the differences between the Fourier analysis of Laskar et al. and the iSSA results for the same time series reported in this work be simply explained by the fact that one method explicitly assumes harmonic oscillations while the other does not?

Line 371: "Eccentricity has the main influence on insolation." I would strongly doubt that the physical effect (in W/m^2) of eccentricity does actually exceed those of the other orbital components.

Lines 384-386: I think it is statistically meaningless to discuss apparent cyclic components with long periods that only fit two or three times into the total length of the studied time series.