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## **Milankovitch forcing is about SUMMER insolation, not global ! - Detection is about significance, not profusion !**

Anonymous Referee #1

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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.,  
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I have difficulties to understand the main topic of this manuscript: Either the main message is that paleoclimatic records contain astronomical periodicities, something which is well-known for almost 50 years. Or, more probably, the aim is to present a "new" spectral analysis method that could detect extremely faint periodicities in geological records. In the first case, I would reject the paper on the ground that it brings nothing new. But the authors are not even citing the famous foundational Hays et al (1976) paper in their reference list, which is, to say the least, very peculiar and which suggest that they may not be familiar with this topic and its extremely abundant literature. Besides (see below), it appears that the authors have even missed the main point of Milankovitch theory, which is based on summer insolation, and not global mean insolation... In the second case, I would also reject the paper since it does not show any information on the key question of spectral analysis: statistical relevance. Indeed, the authors seem to be unaware that white noise contains "all frequencies". Therefore, finding frequencies in a signal is NOT (has never been) the key point of spectral analysis. The only interesting question is to estimate their statistical significance. But there is simply no mention at all of statistics in this paper. On these two main grounds, and several others listed below, I do not recommend publication of this manuscript.

### **Major comments on SSA.**

SSA is now a very classical spectral analysis method. It was largely developed in the 1980s (Broomhead and King, 1986) and was used for paleoclimatic studies since then (Vautard and Ghil, 1989). It was even applied to the Vostok record as early as 1994 (Yiou et al., 1994). Again, it is strange not to find these references in a scientific paper on SSA applied to paleoclimate records in general and to the Vostok data in particular. In fact, SSA has been applied to Vostok and to LR04 in many previous papers over the last 20 years : it is so classical that this analysis is now even part of some student textbooks (eg. Martinson 2018, page 531, Figure 15.6 showing the SSA analysis of LR04). In any case, it

is difficult to find any novelty in this paper.

Spectral analysis is a branch of statistics. Finding many frequencies (real or spurious) in a signal is known to be extremely easy, since the discovery of the Fourier transform. In particular, finding astronomical frequencies and their numerous harmonics in paleoclimatic signals is certainly not surprising. Actually, the opposite would be much more unexpected. This is even more true for the insolation itself !... Not finding the main and secondary astronomical periodicities that are used as input of the insolation computation would be a sure signature of some methodological error. I therefore do not understand the value of applying a spectral analysis method (in particular on insolation), without discussing the "detection level" problem. Indeed, the key mathematical difficulty is to decide whether it is "noise" or "signal" using some statistical test. There is no mention of any statistics in this paper, which certainly disqualifies it entirely. This is strange since standard SSA tools used in the climate community (eg. SSA-MTM toolkit) do provide some statistical tools and safeguards. The harmonics and combination tones found by the authors have often a very small amplitude (below 1% or 0.1% of the variance) and to prove their significance is certainly not an easy task, given that the records are rather "short" and "noisy". This is precisely the main difficulty of spectral analysis, and a problem that climate scientists are usually facing. But the authors of this paper are not even mentioning or discussing this central point, as if they were unaware of the main problem of "spectral analysis estimation".

In addition, it is worth mentioning that detecting several significant frequencies (let alone NUMEROUS significant frequencies) is a much more difficult problem than detecting a single one. Indeed, the null-hypothesis cannot be a simple (red or white) noise hypothesis. A classical work-around is to perform Monte-Carlo simulations of synthetic signals with and without each single periodicity and assess whether the results are statistically distinguishable. But this would probably be a very different paper.

Just to be more precise: Of course, I do not mention the uncertainty in the periodicities as listed in table 1 (obtained after SSA decomposition and resampling of the data), but the significance of the SSA components themselves. In particular, the KEY parameter here is the choice of the embedding dimension. If chosen too high (extremely likely here), then there are a lot of SSA components with dubious relevance. The same phenomenon applies to all spectral methods: the more degrees of freedom you are allowing, the more frequencies you get, but the less significant they are. What is the embedding dimension of the analysis here? This is not even mentioned ! But according to line 62, "SSA was able to extract up to 75 cycles". My guess is that the embedding dimension is a few hundreds, which represents quite a lot of freedom for the method to generate a lot of "spurious" frequencies.

It is very strange to use the old Vostok record (Petit et al. 1999) which covers only 4 glacial cycles while a more recent one (Dome C), covering twice as many cycles, has been available for more than a decade now (see eg: Guo et al (2012) for the SSA analysis of CH<sub>4</sub> at Dome C over 8 cycles, not 4 as here). Obviously, the results are likely to be statistically more robust with a longer time series, if statistics matters of course.

"Finding not only the main expected Milankovic periodicities but also many "secondary" components with much smaller amplitudes gives confidence in our iterative SSA method".

But I would expect standard methods to do the same, with attributing to these periodicities a very low confidence. In any case again, finding many frequencies is easy and NOT the main problem of "spectral analysis". Besides, what is the subject of the paper? Is it about climate or mathematics?

### **Major comments on Climate**

The authors appear unaware that the word "insolation" is not sufficient to describe the astronomical forcing of climate. They are presenting the spectral analysis of "the insolation of Laskar et al (2004)". This is clearly a very insufficient description. When looking more closely at the spectrum (Figure 3), it seems that they are in fact discussing global averaged insolation (linked to eccentricity only)! Otherwise the spectrum would be dominated by the 41k and the 23k periodicities. Since there is no power in the obliquity and precession band, the only explanation is that the authors are using global averaged insolation. But this is a major misunderstanding on the Milankovitch forcing. Indeed, it is well known since the 19<sup>th</sup> century that global insolation changes are much too weak to affect climate and scientists have discussed local seasonal insolation, not mean global one. When climate scientists talk about "insolation" in the context of glacial cycles, they are talking about summer insolation at high northern latitude according to Milankovitch theory. THIS IS NOT what is used in this analysis. In other words, this paper has missed entirely its claimed goal of discussing the Milankovitch forcing, simply by looking at the wrong insolation...

To conclude, it seems to me that the authors have misunderstood both the aim of "spectral analysis" and the foundations of the "Milankovitch theory". I cannot be favorable to publication.

### **Some references**

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