



## ***Interactive comment on “Orbital forcings of the Earth’s climate in wavelet domain” by A. V. Glushkov et al.***

### **Anonymous Referee #2**

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#### GENERAL COMMENTS

This paper applies a wavelet analysis on two paleoclimatic datasets (Vostok temperature reconstruction and marine isotopic records). The authors find the usual Milankovitch cycles.

While the application of NWT to such records might be new, the results are not (Milankovitch cycles in paleorecords and their instability ~1Myr BP). The authors list quite a few references in their introduction, but the few important ones are missing:

-Hays, Imbrie and Shackleton (Variations of the earth's orbit: Pacemaker of the ice ages, Science, 194, 1121-1132, 1976) showed almost 30 years ago the presence of Milankovitch cycles in paleorecords

-Birchfield and Ghil (Climate evolution in the Pliocene-Pleistocene as seen in deep sea

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d18O records and in simulations: internal variability versus orbital forcing, J. Geophys. Res., 98, 10385–10399, 1993) showed, with time varying spectral analyses, the transition from 41kyr to 100kyr cycles in marine sediment records, around 1Myr BP.

-Berger, Loutre and Mélice (Instability of the astronomical periods over the last and next millions of years. Paleoclimate Data and Modelling, 2(4), 239-280, 1998) used wavelets and other techniques to assess the variability of orbital parameter and paleorecord periodicities during the past million years.

The authors use an Antarctica record and a composite record from the tropical Pacific. Why those two? Is there a relation between them? A serious discussion of this choice and the climate dynamics information that can be recovered from those two records is lacking.

The publication of this paper is not justified at present, since it does not bring new insight from what is already known.

### SPECIFIC COMMENTS

My comments follow in order of appearance in the text.

1. The English is generally poor and some sentences are rather impossible to understand (which means that I could have misinterpreted some parts of the manuscript).
2. Please use the deuterium data at Vostok (instead of the temperature reconstruction), to be consistent with the use of d18O in marine cores.
3. Please explain how cosmic rays affect climate on millennial time scales (p. 194, l. 23).
4. As far as I know, neither Petit et al. (1999) nor EPICA (2004) mention that "during approximately the past million years various paleorecords are dominated by the ~100 ky cycle, whereas a few preceding millions years are characterized by the ~41 ky cycle.", because those Antarctic records are much less than 1 Myr long.

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5. The paragraph on p. 195 (l. 9-28) is very obscure, and has nothing to do with the authors' study. In the next paragraph (p. 195, l. 29 to p. 196, l. 16), the authors state that "most studies deal with Fourier transforms", and that "many scientists have used wavelet based tools". This is not very coherent.

6. Why are nondecimated wavelet (NWT) statistically adapted, and even more than continuous or discrete wavelets?

7. The authors state that "The amount of energy incoming into upper atmosphere differs in 0.1 percents between the cases of near-circular orbit and maximal eccentricity. This value suffices to change considerably (from climatic point of view) the surface temperature that causes in turn extreme climatic conditions." This is rather hard to believe. Please provide references and a physical explanation.

8. p. 199 l. 9. I understand that the authors are not geochemists (which is fine by me). I doubt that their explanation of the paleorecords they use is very useful to the reader who just needs to know that the paper deals with a temperature proxy and an ice volume proxy. It is the vapour pressure \*at saturation\* that is different from  $H_{218^{\circ}O}$  and  $H_{216^{\circ}O}$ .

The authors should state the what the mean time step is for the two records.

9. The methodology section is extremely obscure, gives rather irrelevant technical details, for instance, what is the problem of not being translation invariant? But the section does not treat any aspect of statistical significance, nor explain clearly the advantages of NWT over other methods. Such advantages are simply assumed, not proved. The paragraph surrounding Eq. (2) is incomprehensible.

10. The Vostok data set is  $T \sim 420$  kyr long. The authors use dyadic scale increments:

$s=2^0=1 \rightarrow \text{scale} = T/s \sim 400$  kyr

$s=2^1=2 \rightarrow \text{scale} = T/s \sim 200$  kyr (not shown)

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$s=2^2=4 \rightarrow \text{scale} = T/s \sim 100 \text{ kyr}$

$s=2^3=8 \rightarrow \text{scale} = T/s \sim 40 \text{ kyr}$

$s=2^4=16 \rightarrow \text{scale} = T/2 \sim 20 \text{ kyr}$

Since NWT is like a bandpass filter around those periods, it is not surprising that the authors "find" them in the Vostok record!

The authors then assume that the periodicities found in the Vostok record represent the insolation forcing, in particular that the 100 kyr component is driven by eccentricity. At this point, this is pure speculation. Quite a few papers have shown that it is possible to get power at 100 kyr without an eccentricity forcing (e.g., Paillard, The timing of Pleistocene glaciations from a simple multi-state climate model, *Nature*, 391, 378–381, 1998). Thus, the assumption made by the authors is quite unfounded, and if the "main outcome" of the paper is that "the fact that the periods of abrupt climate warmings with cyclicity of  $\sim 100$  ky during the last 400 ky were caused by the combined unidirectional influences of three orbital parameters" (p. 202, l. 23), I must point out that such a fact is far from being established here.

11. If "it is \*well-known\* that the late million years is characterized by the so-called Mid-Pleistocene transition of the Earth's climate with a shift characterized towards much larger Northern ice shields at 920 ky BP and the predominance of 100 ky ice age cyclicity.", please provide at least one reference.

12. Please provide confidence intervals for the spectra in Fig. 3, otherwise, comparing two power spectra is rather irrelevant. Are the peaks at 230 kyr and 320 kyr statistically significant? If not, their geomagnetic discussion is irrelevant.

13. The same remark as in my point No. 10 applies to the marine record.

14. The discussion on submillennial solar forcing is rather speculative (and, in my opinion, rather pointless here), with respect to the results obtained by the authors on Milankovitch timescales. The authors state that "The above-mentioned information can

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be considered as indirect evidence that a portion of solar- and orbital-induced climate changes could become apparent some time tion after." If the logic of this sentence is pulled a bit further, I can also claim that life on Mars is not incoherent with the apparition of ice ages, etc.

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Interactive comment on Climate of the Past Discussions, 1, 193, 2005.

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