

Interactive comment on “Natural periodicities and north–south hemispheres connection of fast temperature changes during the last glacial period: EPICA and NGRIP revisited” by T. Alberti et al.

Anonymous Referee #2

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This paper studies the behavior of the changes in temperature of the last glacial period, using temperature proxies located in two hemispheres. The authors use the series of EPICA and NGRIP projects to analyze the Dansgaard-Oeschger (DO) events with the technique called Empirical Mode Decomposition (EMD, Huang et al. 1998). This technique allows them to characterize climate variability in time domain, instead of other methodologies, like Fourier analysis, which give the results in the frequency domain. From the results they obtain using the EMD they use a Langevin model to test if the different DO events can be the result of excitations of the system in the same climate

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state or a transition between different states. This is a very interesting research work that uses a relatively new and powerful methodology analysis (EMD).

Major content issues:

- 1) There should be a more detailed justification on why the authors use EMD formalism in this case, specially compared to other methodologies like FFT calculation for typical frequencies of time-series.
- 2) To compare time-series of paleoclimatic data there is already some bibliography on the subject, also using EMD formalism to compare GRIP, Vostok and EPICA time-series, see Solé et al. 2007a. The authors should revise this existing previous work and compare their results with the already published ones.
- 3) About the data sampling resolution. The authors state that there is a constant resolution of 50yr for both data sets. It is known that the resolution of data in the original ice-core time-series does not have a constant sampling rate, please clarify that.
- 4) There is no explanation about the data error in the time-series used (in both the value of proxy and the time in which this value is dated) and how this error can be propagated by the EMD method used here, and then can affect the results the authors found.
- 5) The authors should also comment the problem of the IMF mixing frequencies that appears in some of the obtained IMFs. It is a phenomena well known for this method, as the EMD is empirical. For example, in fig. 2 the frequencies in some time periods in mode 2 vs. 3 and 3 vs. 4. That is not of crucial importance for the purposes the authors use the IMFs but it should be nice to have a comment on that, it will give a more documented perspective of the method used.
- 6) The authors should write a little bit more about the physical basis of the DO events (Rahmstorf, 2002) and clarify the difference between the cooling phases of the DO events and the so-called Heinrich events and how these cycles are defined previously

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(Broecker,1994; Burroughs, 1992, Solé et al. 2007b).

7) About the Langevin model used, as far as I have understood, the model is used to ascertain what kind of climatic transition states the DO events should represent. There should be clarified the notation on page 1135, $N(t)$ and $S(t)$ eqs. 4-7, which one corresponds to EPICA and which one to NGRIP?

8) Fig. 6 plots error bars, from where they come? And how are they calculated? It should be mentioned in the text, not only in the fig. Caption.

9) The authors should explain what differences/similitudes they obtain in his analysis compared with the results of the paper of Livina et al. 2010 who also analyze the DO events and use the same methodology of Langevin model.

10) The study of what time-series is leading the change, and the results shown, must be justified in terms of what physical mechanisms should be implied to produce such behavior.

11) The result that the Antarctica leads the processes is obtained from a cross correlation analysis, but no robustness of this analysis is provided. For instance, the sensibility of the results to the time resolution of the series or to an irregular sampling. The same that I was wondering for the EMD analysis and the model applies here: how the error in the time series values can change the results obtained?

Major organization issues:

1) Mainly this work has three methodological parts: IMF calculation, model potential adjustment and cross-correlation analysis. So, I would suggest to organize the paper accordingly, to clarify this methodological approach.

2) It appears that, conceptually, the manuscript has two aims. First, to find what kind of transition states DO events represent. And, second, the leading time series in the climatic changes. Maybe the organization of the paper could be changed with less sections reflecting this two main objectives.

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3) I miss a discussion section with comments on what is new in both perspectives, methodological and conceptual. This will give more clarity to the reader on what is the novelty and the major findings of this paper compared to the already published works in this subject.

Minor questions:

1) there is a typo in the lines 57, 81 and 86 (and soon...) for the periods of data sets it should be – instead the sing Å.

2) In the fig. 5 the offset should be specified, I suppose it corresponds to the last IMF amplitude, but it should be clarified in the manuscript.

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