



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

**J. Guiot (Editor)**

guiot@cerege.fr

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Version 2 is a significant improvement of version 1 and a part of the criticisms of the three reviewers have been taken into account. Nevertheless, it remains some points which need to be clarified.

Point 1) It is written “ Note that the correlation coefficients between these signals and time series of orbital parameters during the last 420 kyr exceed the value of 0.7 ”. : R1 has requested the graphic of these time-series. I think that it should be important to have them. As the analysis of the decomposition of deuterium curves into three components related to orbital parameters is the key to demonstrate that climate variations are caused by the combined unidirectional influences of these parameters, it is absolutely necessary to plot these parameters in regards with D3, D5, D6. As it, the authors continue to fail in that demonstration.

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Point 2) R3 has pointed two major problems with this paper : - there is no real discussion of the effect of irregular data sampling on the spectra, in particular with the choice of the mother wavelet and the fluctuation of the periodicities - in the marine cores, the age models are tuned to orbital cycles ; there is then a risk of circularity I am not sure that the new version of the paper is a clear progress in that respect. A good way to solve these problems, in my sense, is to check the robustness of the method as regards as the time sampling fluctuation. Concerning the marine time scale, the tuning is done on specific periodicities (essentially 20 and 40kyr), it must be kept in mind when the results are discussed.

In the discussion, the authors protect them against criticisms from the paleoclimate point of view with sentences as this “ present paper is aimed at the searching for orbital indicators in paleoclimatic records really, rather than at the revision of present opinion on the Milankovitch theory of Ice Ages ”. I am afraid that finally the paper is not convincing neither in paleoclimatology or in methodology. Indeed, I may think that NDW is interesting, but why is it better than other methods?

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

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