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Climate of the Past Discussions

Interactive comment on "Strong indications for nonlinear dynamics during Dansgaard-Oeschger events" *by* H. Braun

Anonymous Referee #2

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The paper presents a statistical test for nonlinearity in NGRIP data for the time interval 60–11 kyr before present based on the method of surrogate time series. The author uses the mean absolute increment between two consecutive values in the ice-core record as test statistic and the null-hypothesis of a linear Gaussian stochastic process is rejected beyond any reasonable level of doubt. As a consequence, the paper advocates the use of nonlinear rather than linear time series analysis methods for interpreting Dansgaard-Oeschger events and raises a couple of specific issues where linear methods or assumptions may lead to misinterpretations or false conclusions.

In my opinion, the study is generally sound, competently carried out and well written. The main result (DO events cannot be described as a linear Gaussian stochastic process) is not at all surprising; the notion of a nonlinear switching between two different

climate states during glacial times is common sense in the palaeoclimate community. Nonetheless, it has not been demonstrated so far in the literature purely based on data using a systematic statistical test and thus, in principle, merits publication. Moreover, linear concepts and techniques are still customary in palaeoclimate; the full consequences of a nonlinear dynamical framework have not yet been recognised. This makes the present study of value. However, I am disappointed about the very limited level of analysis in the paper. The manuscript is clearly too thin and not acceptable in its present form. I have the following major concerns which I would like to be addressed by the author before considering approval for publication:

1.) The use of only the M-statistic is a serious limitation. What is the motivation behind this choice? In particular, the M-statistic seems to be more a measure of non-Gaussianity in the ice-core data and/or the increment time series, which is well-known and already evident by eye, rather than genuine nonlinearity. Prediction errors or suitably chosen higher-order moments would be more convincing measures of nonlinearity (cf. Kantz and Schreiber, 2004).

2.) The author may have a look at the improved nonlinearity test proposed by Schreiber and Schmitz (1996). It tests against the null-hypothesis of a nonlinearly observed/distorted linear Gaussian process rather than just a linear Gaussian process. This is a more interesting null-hypothesis as it distinguishes between pure non-Gaussianity and genuine nonlinear structure. Observational distortions in the probability distribution may well play a role in the ice-core data as the isotope record does not only represent temperature but is also influenced by other factors.

3.) What is the influence of the smoothing procedure applied to the ice-core record (running mean)? It probably reduces the noise level of the data and thus facilitates the detection of nonlinearity. Is nonlinearity detectable also in the raw data?

4.) Table 1: Do the results refer to a single realisation of a surrogate time series or is it an ensemble mean? What about the statistical significance of the differences in the

distributions?

References

Kantz, H. and T. Schreiber: Nonlinear time series analysis, 2nd ed., Cambridge University Press, Cambridge, 2004.

Schreiber, T. and A. Schmitz: Improved surrogate data for nonlinearity tests, Phys. Rev. Lett., 77, 635–638, 1996.

Interactive comment on Clim. Past Discuss., 5, 1751, 2009.