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Nonlinear Processes in Geophysics
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Interactive comment on "Detecting dynamical anomalies in time series from different palaeoclimate proxy archives using windowed recurrence network analysis" *by* Jaqueline Lekscha and Reik V. Donner

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First of all, I would like to apologise for this delayed review.

The purpose of the manuscript is to apply a method developed and documented by the authors elsewhere (the windowed recurrence network analysis (wRNA)) on artificial time series simulating the output of speleothem, lake, tree archives and isotopic water concentration in ice cores. The purpose is to test to what extent the 'proxy process' (transformation of the climate signal by the natural archive) could mask anomalies that

would have been otherwise detected in the original time series. Different time series have been tested, including Gaussian white noise, an autoregressive process of order 1, output non-linear dynamical system model, and data from the reanalysis project of Hakim et al.

The main diagnostic is 'network transitivity', and the application of the method to the different input datasets generates Figures 2 to 6. The reader is invited to concentrate on the 'area-wise' significance test, which is supposed to indicate a signal of significant change in network transitivity, to be interpreted as a change in the effective dimensionality of the system.

Some results appear a bit disconcerting, especially the test on the AR(1) signal, because it shows, on the one hand, a large patch of area-wise significant anomalies in network transitivity which was — if I understood correctly — a priori not expected in this signal. In addition, this large patch disappears in the natural archives simulated with this model, while another patch emerges in the speleothem simulation. Simulations with other inputs tend to confirm that the speleothem model is prone to create or destroy areas of significant network transitivity anomalies seen in the original time series.

The study is quite systematic in its approach, to the point of being slightly repetitive, and yet, one might argue that it falls short convincing the reader about the robustness of the conclusions. Basically, up to p. 12 the manuscript consists in an exposition of the methods, which for their greatest part have been described elsewhere (the significant area test is in press, and the proxy models have been published elsewhere). The output of the wRNA analysis follows a show-and-tell description running until p. 18, and even though some main conclusions are correctly outlined, the discussion does not really help identify mechanisms or key conclusions that would actually help to 'improve the interpretation of windowed recurrence network analysis' as announced in the abstract. For example, the authors have observed that the speleothem model modifies the significant patches, but we do not really which process, in the speleothem model, is responsible for this behaviour. Do we expect this to be an idiosyncrasy of the particular speleothem model used here, or do we expect it to be a general result? Which aspects of the 'nonlinear filtering' should be incriminated? The presence of a large significant area patch on the AR(1) time series, along with its the quasi-absence of significant changes in network transitivity in the last-millennium reanalysis data is also disconcerting, because we no longer know how to reasonably interpret the output of the wRNA for understanding climate dynamics. Is the last millennium actually the right test bed for this study?

To sound hopefully a bit more constructive, I would suggest the authors to seek for more general aspects of the filtering process which may destroy or generate spurious changes in network transitivity. Is this caused by non-linearity in the instantaneous response (what would an 'exp(x)' filtering generate?) Is this the temporal smoothing process? Is it the amount of noise? What would this analysis tell us about how to find proxies that would preserve the wRNA signal, beyond the particular example chosen here? Which are the desirable characteristics for such proxies? Answering these questions would provide some more general and perhaps valuable hints for the interpretation of the wRNA, which could then be summarised in the abstract.

Perhaps the reader will also better appreciate the interest of the wRNA if more clues are given about how to interpret it: can one get a more or less adequate intuition of what a change in wRNA implies about the dynamics of climate. What, physically, does an increase or a decrease in network transitivity mean? Would this be associated with a form of 'global synchronisation' ? Are we expecting it when we approach a form of bifurcation (a "tipping point")? What is the wRNA telling us that is not obvious from visual inspection of the time series?

Finally, the choice of an embedding dimension $m = 3$ was, to this reader, difficult to reconcile the quote that "The embedding theorem of Takens guarantees that, when choosing the embedding dimension larger than twice the box-counting dimension of the original attractor, the reconstructed and the original system's attractor are related by a

smooth one-to-one coordinate transformation with smooth inverse, independent of the choice of the delay". Wouldn't we have expected, on this basis, a much larger embedding dimension? This may invite some discussion, perhaps available in Lekscha and Donner, (in press). In 1984 (Nature, vol. 311, p. 311), Nicolis and Nicolis published an estimate of the 'climate attractor dimension' but subsequent authors (including Grassberger, 1986, Nature, 1996, vol. 323, p. 609, and Vautard and Ghil, 1989, Physica D, vol. 35, p.395) pointed the difficulty of actually getting a meaningful estimate of "a climate dimension" from a 1-dimensional, finite record. Could the authors clarify their position in this respect?

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