Answer to review of a manuscript for NPG (RC2)

Detecting dynamical anomalies in time series from different palaeoclimate proxy archives using windowed recurrence network analysis by J. Lekscha and R.V. Donner

First of all, we would like to thank the Referee for thoroughly reading and commenting on our manuscript. In the following, we provide our answers to the comments.

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.

Indeed, the AR(1) signal shows a patch of areawise significant anomalies that is a priori not expected. As argued in the manuscript (lines 398-400), such artefacts appearing in single realisations of the stochastic input time series should be excluded in future work by considering ensembles of realisations of the processes. Unfortunately, this was not possible within the scope of the original manuscript.

With respect to the results of the different proxy model output time series, we generally found that the tree ring and the lake sediment model tend to miss, while the speleothem model both misses and creates additional areawise significant patches of the network transitivity, which is in agreement with the above observations. We will further disentangle the description of these results in the revised manuscript in order to make these points clearer.

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?

We thank the Referee for pointing out the weaknesses of the presentation of the material and will work on the mentioned points in the revised manuscript. This particularly concerns the presentation of the results and the corresponding discussion and conclusions, while for the (admittedly long) description of the methods and proxy system models, we consider it useful to be included in the manuscript to provide a complete picture and make the contribution self-consistent. Still, we will attempt to shorten the detailed description wherever possible and potentially move parts of it to an appendix or supplementary information document.

Also, we want to stress that this study has been meant to improve the interpretation of wRNA results in terms of highlighting not only the potentials, but also the limitations of the method when applied to palaeoclimate data. That is, in particular for the speleothem model, our results show the need of further studying the effects of different filtering mechanisms on the results of the wRNA in order to draw reliable conclusions when analysing real-world data. (Note that there have been quite a few papers reporting the results of windowed recurrence analyses of real-world speleothem data, e.g., Donges et al., Clim. Past, 2015; Eroglu et al., Nat. Comm., 2016.) In this regard, we argue that the last millennium is a good test bed because for this period, highly resolved proxy data are available from various different archives. We will clarify the direct conclusions from the obtained results with respect to the interpretation of the wRNA and outline further work that will help to increase the robustness of the conclusions. For the speleothem model results, for example, we expect the conclusions to hold, independently of the actual model used, but potentially depend on the choice of the model parameters.

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.

We thank the Referee for these suggestions and fully agree that including the results for a set of general filtering functions and noises on the wRNA results will help interpreting the obtained results by providing insights into the mechanisms that are responsible for them in the different proxy system models. We will further elaborate on these more general aspects in the revised version of the manuscript.

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?

We agree with the Referee that including a paragraph on the interpretation of wRNA with respect to the climate system is a very good idea. Still, general statements will hardly be possible as climate-related interpretations vary depending on the location and thus, local boundary conditions have to be taken into account. Also, the network transitivity calculated from a single time series cannot be associated with a 'global synchronisation' as only information from a single location is taken into account. Instead, the network transitivity has rather been related to the dynamical regularity of the variations in the analysed time series (e.g., Donges et al., PNAS, 2011) with higher values of the network transitivity corresponding to less irregular variability and vice versa. This is in accordance with the interpretation of the network transitivity as an indicator of the dimensionality of the system's dynamics. In this regard, detected anomalies in the network transitivity could be related to some tipping point, but do not have to be. We will attempt to provide a more concise description of possible interpretations of the wRNA results in the (palaeo)climate context in the revised version of the manuscript.

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?

We are well aware of the problem of choosing an appropriate embedding dimension when the available data are univariate, finite, and subject to noise. The embedding theorem of Takens is a sufficient condition and not a necessary one; thus, embedding dimensions smaller than twice the box-counting dimension of the original system's attractor may lead (at least approximately) to an appropriate embedding, which might possibly, at least partly, reconcile the quote and the choice of the embedding dimension m = 3. Also, it should be noted that the theorem is strictly valid only for perfect data. Thus, for finite and noisy data, the estimation of the embedding dimension has to rely on some more or less heuristic approaches. Many of these approaches however either have problems of distinguishing deterministic chaos and noise or systematically underestimate the embedding dimension.

In this spirit, we fully agree with the critiques of the cited papers that it is difficult to get a meaningful estimate of an embedding dimension from univariate, finite and noisy data. For the particular case of a 'climate attractor dimension', we agree that the climate system is not a low-dimensional dynamical system. Still, we think that lower dimensional embeddings can be used to obtain meaningful information about a system. Furthermore, in the palaeoclimate context where available time series are often rather short, we do not think that high-dimensional embeddings are useful as the limited amount of data points will in most cases not be sufficient to sample the attractor in a high-dimensional embedding space.

In the majority of the paper, we do not directly analyse climate data but rather synthetic data representing different kinds of underlying processes. For convenience, comparability, and with respect to the time series length and the increasing computational effort for larger embedding dimensions, we chose m = 3 for all analysed time series, thus, also for the last millennium reanalysis data which indeed represents more closely the actual dynamics of the climate system. In general, when analysing real-world data, we think that the problem of choosing an appropriate embedding dimension is best tackled by employing one of the estimation methods that can distinguish deterministic and stochastic signals and do not require too many subjective parameter choices (such as the one presented in Cao (1997), Physica D, 110, 43-50), and additionally slightly varying the obtained embedding dimension when analysing the data to check the robustness of the results. In the revised manuscript, we will add a more detailed comment on our choice of m = 3.