**Climate
of the Past
Discussions**

Interactive comment on "Multi-periodic climate dynamics: spectral analysis of long-term instrumental and proxy temperature records" *by* H.-J. Lüdecke et al.

Anonymous Referee #1

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This is an interesting paper, applying various forms of signal analysis to long climate records, and arguing about possible future expectations on the basis of the analysis. The authors appear to be statistical experts in this form of analysis, although not mainly publishing in climate science, and it is refreshing to see new approaches to an old problem.

The paper is in general well-written; the methods are fairly well described, given space-constraints; the conclusions are relevant to the general field of signal analysis applied to climate science. However, I think the authors have made an error in how they argue for their main point, so I call for some modifications/additions of the manuscript before

I can accept it. Nevertheless, the paper is welcome, in that it does an unusually good job of estimating the significance of results based on auto-correlated data.

I have a few minor comments and notes on the paper, and one major objection (near the end), and I give them here in the order they appear:

Minor points:

p. 4495, line 8: It is not entirely clear what is being used as the standard deviation here - of the whole record? i.e. including all trends and low frequency variability?

p. 4495, lines 27-28: This statement seems to be rather optimistic. Why can the future climate be predicted from what is essentially an addition of harmonic oscillations? I understand what the authors are saying but feel that they may want to demonstrate by example or at least by references why they think that it is a good idea to extrapolate from superposed harmonics.

p. 4498, lines 1 and 2: I was not aware that the Hurst exponent guarantees a good description of the auto-correlation of a series - it may well be so, but perhaps the authors can remind the reader why this is so: The usual method in climate research - when applied - is to use surrogate generation methods that conserve the autocorrelation spectrum, such as 'scrambling of the phases'. Looking at the Hurst exponent may be a better thing to do and I suggest that the authors tell us why that is.

p. 4498 lines 16-22: The procedure seems to be the same as "low-pass filtering". That would be the common phrase to use in the climate literature. The use of '6 best out of 8' frequencies is novel, I believe - is there a description in the literature of why this is a good idea?

p. 4499, lines 6-11. It is shown that there is a somewhat similar long oscillation in the SPA record, and the presence of this is used to support the analysis of the shorter time series. The phase-agreement between that periodicity in the shorter and corroborating longer records is important here, and could perhaps be commented on or shown in a

graph somewhere?

p. 4500, section 6: Is very well described - well done!

p. 4501, Discussion section: The comments near lines 20-25 on the Feigenbaum concept are interesting. To me it seems the first time someone has pointed this out in the climate literature. Can the idea be tested on other data - i.e. can a scientific test of the principle be proposed that perhaps could be applied in the future when e.g. long proxy climate reconstructions become available; or can the ideas be applied to shorter periodicities that would be easier to see in the numerous short instrumental climate records that exist?

Major point:

p. 4502, lines 3-7. I do not agree with this. It seems wrong to say 'Look, we have done well recreating the time series so if we add anything more it will not be as good'. What has been demonstrated is really that Fourier decomposition works on an arbitrary time series, yes? I simply do not see how what the authors have done has any relevance on excluding GHGs as a major factor on the climate. Technically, since the effect of greenhouse gasses on the climate is 'trend-like' in terms of temporal evolution I am worried that the use of various low-pass filters in the reconstructions are presupposing that any GHG influence would be shorter-lived than the cutoff of the filter. GHG have always had an influence on the climate - but the amount of GHGs is accelerating and it remains to be shown if the time-scales implied by the choices of the filters are important in that argument, surely? I would say that at the very least the authors need to explain their reasoning better here, giving references to estimates of GHG time-scales and so on.

Further suggestions:

In general, the use of harmonic decompositions of the climate record has been discussed before and I think it would be interesting to see the authors discuss some of

the results in the various papers by N. Scafetta.

The present manuscript's results may perhaps also be compared to those of Mann, M.E., Lees. J., "Robust Estimation of Background Noise and Signal Detection in Climatic Time Series", Climatic Change, 33, 409-445, 1996.

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