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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.

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Some notes on "Multi-periodic climate dynamics: spectral analysis of long-term instrumental and proxy temperature records" by H.-J. Lüdecke, A. Hempelmann, and C. O. Weiss

The authors (LHW in the following) present results of an analysis that may lead to identifying certain periodicities in the regional temperature record for Europe if the earlier comments by Mudelsee (2012) are implemented. Following Mudelsee, I recommend that the author extend the discussion of their results in view of the work of Feigenbaum (see references in original manuscript).

Here I am going to present only some further thoughts that may help to improve the manuscript.

I would advise caution in making inferences about global temperatures and the importance of certain drivers from the periodicities found in the temperature series for one region. Indeed, the analysis only considers Europe, although LHW present the temporal evolution of one d18O record for one Antarctic ice core (note, the abstract mentions multiple ice cores). Thus, it would also be welcome to see the analyses extended to the d18O record. That is, I am concerned that it is not fully justified to draw conclusions from one location and one regional series about similarities between hemispheric temperature evolutions.

Furthermore, the manuscript lacks a discussion of the quality of the early instrumental series as presented. I am aware that this may be found in the noted references, but at least a qualitative discussion of such issues is necessary.

A rather large question concerns the authors' empirical reconstruction. In my understanding, it is a foreclosed conclusion, that I'll obtain a reasonable fit to a smoothed version of my original time series, if I reconstruct it by using those periods with the largest power densities. But I may be wrong. This implies further that if there is an anthropogenic trend component in the temperature record, it is a feature in the record and not external. By transforming and inverse transforming the series the authors include the potential anthropogenic trend component in their analyses and thus are not able to draw conclusions on the existence or the lack of such a signal. This holds especially if we follow Forster et al. (2007) and assume that atmospheric CO2 has increased by about 36% over the last 250 years (the period studied by LHW).

With respect to the reconstruction and the performed projection, I also think the author's missed an opportunity so far by not showing a hindcast assessment. Obviously, they wouldn't be able to use the full 250 years of their record for such an exercise, but it would be interesting to see how well the projection is able to represent late

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20th century warming or, as a second test, the first 50 years of the studied period. Such a study could only base on about 200 years of the data. If LHW only used 150 years, they possibly could include the larger number of station series of the BEST data (http://berkeleyearth.org/) in their assessment.

Another point to consider is that the study title mentions “climate dynamics” but includes no result on climate dynamics. That is, the presented fitting exercise does possibly state something about the periodicities of the regional temperature series for Europe but nothing about the dynamical implications. Indeed the authors explicitly state on page 4501 line 12: “The cause of the periodicities is unknown.” However, the literature includes a number of possible candidates for low frequent variability.

I have to admit that I find the discussion section of the manuscript slightly confusing. While the authors draw conclusions from the intrinsic fluctuations in the regional series on the evolution of the global temperature, they on the other hand assume that the global temperature acts as control parameter for the Feigenbaum transition responsible for the intrinsic fluctuations.

On the same topic: While I find the idea interesting to interpret the orbital configuration and resulting changes in solar insolation and seasonality as control parameter, I don’t think that the Esper et al. (2012, reference see manuscript) study has been correctly quoted. Esper et al. (2012) confirm earlier results (see references of Esper et al., 2012) that the boreal summer temperature has cooled, over the last 2000 years. They do so by revealing this trend in long regional temperature-sensitive proxies. While this is essentially a regional result, the larger scale details of this cooling may be inferred from climate simulations and are not homogeneous (see supplement of Esper et al., 2012 or also Fischer, 2011 and Servonnat et al., 2010). A literature search may reveal additional references concerning the effects of late Holocene orbital forcing changes on regional climates.

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