

Interactive comment on “Discriminating low frequency components from long range persistent fluctuations in daily atmospheric temperature variability” by M. Lanfredi et al.

M. Lanfredi et al.

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The Reviewer is evidently sure of the substantial fractal nature of the climate dynamics (“...the well-established long-term correlations in the atmospheric temperature”). Most of His/Her points are not directly relevant to the issues discussed in our work. No reason is provided for criticizing the reliability of our analysis results and for showing the uselessness of the more careful re-analyses proposed in the paper. In particular, the Reviewer’s issues focus on non-pertinent time scales (paleoclimate, data simulated up to 1000 years), on studies whose approach is already addressed in the paper, and on an erroneous modelling of atmospheric temperature.

Let us to reply to the points of the Reviewer in detail:

1 Reviewer:

First of all, there exists a study by [Pelletier, EPSL, 158, 1998] showing that the scaling of temperature records becomes more pronounced when considering longer time periods, up to random-walk exponent for time scale of 100 kyr (the result was obtained using the conventional power spectrum, without employing the DFA).

1 Authors:

Historical (instrumental) and paleoclimatic (proxy) data cover two non-overlapping spectral bands of the climatic power spectrum, which account for different phenomena. Scaling on the historical band discussed in our work cannot be deduced by the behaviour observed in the paleoclimatic one. Thus, if we focus on the spectrum of historical data (from $f=10^{-2}$ to $f=10^{-1}$ (1/year)) extracted from fig. 7 of Pelletier [EPSL, 158, 1998], we can observe a non linear behaviour that is fitted to a broken line (exponents depend on the selection of the breakpoint). Linearity is particularly questionable on frequencies higher than $f=10^{-1}$, just as in our work. On the other hand, the statistical confidence of any analysis on lower frequencies, around $f=10^{-2}$, is low because historical time series are too short for assessing centennial scales, which should require observations on many centuries. In addition, researchers who use power spectrum should carry out a preventive accurate check for trends, which are very common in temperature data and can severely bias results on long range persistence. Methods such as DFA have been designed just for avoiding the drawbacks of spectral analysis.

2 Reviewer:

On the other hand, one can consider the Central England Temperature (CET) daily time series since 1772 (85K datapoints) to realise that its spectrum is not flat (not white) in time scales above 80 days and can be approximated by a straight line in a log-log plot. The same diagnostics, but as monthly series since 1659, was used in [Pelletier 98], see Figs.2,3 (in Fig.3, the spectrum was obtained using FFT with a Hanning taper, and $S(f) \sim f^{-0.47}$).

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2 Authors:

Our work started just from papers that show straight lines in loglog plots. The example provided by the Reviewer does not add any further information. We do not aim at demonstrating that the long term climatic variability is white. This point has been explicitly specified in our paper (see, e.g., line 25-28, page 5180). The problem is to establish if the straight lines drawn in a loglog plot are statistically realistic or not. Scale invariance and its dynamical implications are not the only possible alternative to the lack of correlation. Why do not perform goodness-of-fit tests concerning the power law functional form as we suggest?

3 Reviewer:

When considering asymptotics of available temperature records, we are dealing with time scales up to centennial, and here works like the one by Pelletier are quite relevant. If the authors prefer non-averaged data, I suggest to consider paper by [Huybers and Curry, Nature, 441, 2006], where they plotted patch-work spectral estimates combining instrumental and proxy records (CET record is shown there, too, see Figure 2, blue curve). Without averaging, the spectra still indicate power-law behaviour at centennial scale (which is too short to be influenced by the Milankovitch cycles).

3 Authors:

The work by Huybers and Curry is very suggestive but there is no substantial difference between the analysis they performed and that carried out by Pelletier. As already specified, instrumental temperature records do not allow accurate investigations of centennial scales. We studied higher frequencies, which are widely discussed in works specifically devoted to instrumental data (see the references reported in the paper for a representative sample of this literature). In addition, spectral estimates can be easily biased by long term trends mimicking persistence.

4 Reviewer:

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In a manuscript dedicated to asymptotic power-law in temperature records discussing these issues is essential, in my opinion. Otherwise, a non-expert reader might have been misled regarding the processes and mechanisms in the climate system: the figures in the authors' manuscript show that something nonlinear happens in short-term scale (Fig.2), but this is not related to power law in asymptotic scale, - and then there are oscillations which can be attributed to finite-size effect or to the DFA algorithm (the last points of the curves are related to the biggest window size, where polynomial fit might be biased), etc.

4 Authors:

In this context, the adjective "asymptotic" in itself has a vague meaning if we do not specify to what process and temporal horizon it refers. All the discussions on daily data reported in literature look at the decay of the meteorological correlation with a superior temporal limit that is imposed by the time series length (it can hardly reach the 100year scale, whatever the analysis technique may be). We limited ourselves to follow those previous works that claim power law on such scales and specified our definition of asymptote in the paper (see, e.g., line 11-12, page 5179). In any case, the discussion of the Reviewer is self-contradictory. He/She has previously affirmed that scaling is detectable in historical data, even up to centennial scales. Here, he/she affirms instead that the shortest time scales are not interesting, whereas the long ones are affected by distortions, such as finite-size effects. In practice, instrumental data should not be able at all to enhance long range persistence. In this case, the comment of the Reviewer would suggest that all the previous literature on this subject is not reliable, and that also the works by Pelletier and by Huybers and Curry are not correct. In particular, their unitary view of the climatic dynamics, obtained by including such data, would have no sense.

5 Reviewer:

Yes, spikes in the data are able to influence the power spectrum at lower frequencies.

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However, the periods of three Milankovitch forcings are far too long to explain all the power around centennial scales. This is the area of further investigation, obviously. And it should be discussed in a paper with such a claim.

5 Authors:

We agree: the three Milankovitch cycles are not responsible for the centennial scales accounted for in the works previously cited. Trivially, the data used for these analyses have been recorded in the last few centuries when none of these events occurred. The accumulation of variance on centennial scales could be more likely the effect of anthropic or natural trends that are widely present in the data. The presence of spikes as well as the occurrence of slow non linear trends within the Milankovitch cycles (<http://cdiac.ornl.gov/trends/temp/vostok/graphics/tempplot5.gif>) influence the paleoclimatic spectrum and not the historical one. Decennial time scales, that are the subject of our work, can be discussed by ignoring paleoclimate just as we can do with meteorology.

6 Reviewer:

Moreover, there exist studies (see, for instance, Rybski et al, JGR, 113, 2008), where similar to observed power-laws are detected in much longer modelled records (1000-yr simulations of ECHO-G general circulation model in the mentioned paper, see Figure 3). This confirms the initial studies claiming the power-law behaviour on shorter observed series, and this is where climate models are very helpful.

6 Authors:

Long lasting power laws in surrogate data cannot be considered conclusive for claiming the existence of power-laws in the real world. Models have to reflect observational results and not the converse.

7 Reviewer:

I cannot agree with presenting the data in the current format in the manuscript. The

authors should provide equivalent figures with artificial power-law data of the same length (not their model data), if they aim to test all the influencing factors. The data should be superimposed with a variable seasonal cycle of about 365 days (because in nature it is not precise), then the seasonality should be filtered out, and the data studied using several scaling techniques.

7 Authors:

The objection of the Reviewer is questionable and, in any case, the suggested simulative algorithm is wrong.

Previous works claimed that temperature anomalies are power-law on monthly and inter-annual scales. Therefore, the right counterpart of our "model data"; on such scales is a "pure" power-law time series. It is not necessary any simulation because any scaling technique should show linearity in a loglog plot, differently from what we observe for real temperature data. Presumably, the Reviewer does not remember that scaling refers to "anomalies" and therefore it should summarize all the variability that exceeds the precise annual periodicity, inaccuracy in the period included. In other words, there are not any other "influencing factors" in the picture of long term persistent fluctuations beyond meteorology. This fact is well-expressed in the work of Kiràli and Jànosi (Phys. Rev. E, 2002). We introduced simulated data from a Markov process just to show that the commonly adopted approach, without any test of goodness-of-fit, suggests (false) scaling also in scale dependent time series. This is a crucial point of our work that is not discussed by the Reviewer.

8 Reviewer:

The power spectrum is informative and should be used to compare results with DFA. There is no single perfect tool, and when claiming strong results, especially contradicting the previous studies, the data should be analysed as rigorously as possible; for instance, showing four power spectra (raw or logarithmically binned) for the named four cities would be very helpful for discussing the asymptotic behaviour.

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8 Authors:

The power spectrum of the Prague time series is already present in the paper and we did not add the other ones because they are very similar to it : no reasonable sign of scaling can be inferred on the low frequency range (frequencies are in the Nyquist range). Of course, we can include them in the final version of the paper.

As a last remark, we would like to emphasize that we contradict previous studies by following the same approach they used. They "claimed strong results"; we merely propose more careful re-analyses.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5177, 2009.

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