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Atmospheric Chemistry and Physics Discussions

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

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We agree with the Reviewer's general point of view. The existence of astronomical cycles and the non-linear response of the different climatic sub-systems suggest dynamical scenarios that are richer than that represented by the ubiquity of scaling. In the pure fractal picture, external forcing and interactions drive a continuum of frequencies randomly organized in scaling regimes, where scale invariance implies the absence of dominant frequencies. In theory, it would be impossible to single out specific oscillatory mechanisms (e.g., El Nino) because any oscillation should be blurred in a continuous scaling wave-packet without having a showy identity. On the other hand, if such slow stochastic oscillations there exist instead, then they are expected to be a source of correlation on the long range that can trick fractal tools. We think that long-range persistence is a strong claim that cannot be supported by careless time series investigations.

Detailed points:

(1) Our "very detailed inspections" refer to the investigation of the power-law functional form carried out by looking at "variance details", since the magnitude of the correlated fluctuations beyond meteorology is small. The analysis of such details can be performed by investigating residuals from best fits and/or by assessing the dependence of the scaling coefficient on the scale.

This may require non sophisticated tools. In fact, the sentence cited by the Reviewer "Although the simple naked eye inspection of our results was sufficient for drawing conclusions, we applied the Runs Test" enhances the striking evidence of non linearity in the log-log plots of the best fit residuals. The statistical test was performed just for making the procedure clean, but non-linearity in our figures is extremely clear even at a quick look.

As far as acf and spectra, the second part of the work follows a quite different philosophy. Once established the weakness of the scale invariant paradigm, we try to evaluate if "one only" time scale beyond meteorology is sufficient for justifying the observational results reported above. We specify that the selected model is the "simplest" one (see page 5181 lines 11-15; page 5184, lines 16-19.) and not necessarily the best. Therefore, the discussion reported in this second part aims at demonstrating that a simple dynamics generates persistence characteristics that: a) are mixed up for scale invariance if analysed perfunctorily; b) reveal instead their non-scaling nature (similarly to the observational data) if analysed minutely. The 95% confidence level for acf is already included in the caption of figure 4 and within the text: it does not exceed the size of the symbols used in the plot. In order to better enhance the statistical goodness of our model, we can also precise that the standard deviation of the differences between the "true" autocorrelation function and that obtained from the theoretical model (s=0.0067)

ACPD

9, S1509–S1512, 2009

Interactive Comment

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is comparable to this 95% level (s=0.0070). Thus, our model is able to pick up the sample autocorrelation within the limits imposed by statistics despite its simplicity. This result, together with the previous ones, suggests that the right modelling has very likely to be searched for in a scale dependent context rather than in the scale invariant one: this is our conclusion.

(2) Since in the first part of the paper we show that temperature is a false persistent fractal, whatever mechanism that is able to generate infinite memory can be considered improbable. This excludes a priori the existence of significant feedbacks.

By the way, the only data on atmospheric temperature are not sufficient for obtaining the best modelling in a multivariable context, since we should have data on the other variables involved. When the feedback is included, the use of a non-observed dummy variable in a coupling makes the parameterization quite dependent on the initial conditions; many local minima can be found because of the parameters redundancy. As explained in the paper, we started from a verified model (Mosedale et al, J. Climate, 18, 1086, 2005 and reference therein), which includes the feedback term too. The coupling coefficient (a21=0.0046) is one/two orders of magnitude less than the other "memory" parameters estimated by Mosedale and co-workers for wintertime (a11, a12, a13) = (0.790, 0.092, 0.995). We decided to eliminate it for making our model as simpler as possible (see page 5184, lines 16-19; page 5185 lines 9-10) so as to evidence the mechanism that allows to mix up "one pure" long scale for scale invariance (figure 8).

Finally, we think there is still much information stored in atmospheric temperature time series. Nevertheless, if we are right, they do not pass detail fractal analyses. Therefore, why to use fractal models? More in general, the comparison between fractal and multi-modal simulations could be not particularly useful for a better understanding of climate, if too much modes are used. The superposition of "many" modes is just the strategy some researchers use for simulating fractals. Long range correlation is often regarded as the outcome of many independent AR1 (see Caballero et. Clim. Res., 21, 127-140, 2002) and this comparison could be not conclusive. A "low" number of

ACPD

9, S1509–S1512, 2009

Interactive Comment

modes per frequency range makes the difference instead. This is why the simplicity of our model makes our results strong and confirms a posteriori the wisdom of our approach. We think that non linearity could be the key dynamical concept to improve our understanding of Climate.

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9, S1509–S1512, 2009

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