Atmos. Chem. Phys. Discuss., 9, S1243–S1245, 2009
www.atmos-chem-phys-discuss.net/9/S1243/2009/

Atmospheric Chemistry and Physics Discussions

ACPD

9, S1243–S1245, 2009

Interactive Comment

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

Anonymous Referee #2

Received and published: 2 April 2009

The Authors present several fairly strong claims which I would like them to address in the ensuing discussion. In some of the points below I assumed the role of the "devil's advocate" to encourage the discussion.

In the manuscript the Authors present an analysis of a number of real meteorological series consisting of removing a trend using DFA followed by an application of a Markov type model to the residuals.

Their main result seems to be the questioning of scale invariance in the context of the analysed data sets, thus questioning several current theories regarding the fractal nature of meteorological time series.

Why would I tend to agree with this? From my pragmatic engineer's point of view and some acquaintance with meteorological and other environmental, econometric and industrial time series, I do appreciate the need for modelling on different time-scales, and also of models that describe the system's behaviour efficiently. However the trivial fact that there are several well defined astronomical cycles in the "inputs" to our Earth System indicates that can be a much simpler phenomenological explanation of these different time scales. These periodic inputs of relatively high amplitude excite an inherently non-linear system with its own dynamic modes, and so generate harmonics and modulation effects, which may give a false impression of fractal nature. And while a fractal model will often fit the data with several frequencies present, it will not always provide useable forecasts or a structural "physical" interpretation in terms of real processes such as transfer of mass and energy.

In my view there are two major methodological issues I would ask the Authors to address prior to publication.

(1) However, while I agree in principle with their view I feel that the Authors need to firm up the methodology of the present work in order to make these results more convincing. For instance the Authors make a claim of very detailed inspections, yet they repeatedly show plots of similar acf or spectra and state that they are clearly in a good agreement. Their statement "Although the simple naked eye inspection of our results was sufficient for drawing conclusions, we applied the Runs Test" seems in contradiction with the claim. A detailed inspection would, in my view, include at least an assessment of sensitivity of these results. It is a fairly well known fact that acf and spectra are not that sensitive measures and that they carry large uncertainty on their estimates. For instance Fig.4 might be telling a different story should the commonly used significance level for the acf be included in it. In general any conclusions from similarity of spectra (or acf) between models and data should come with a grain of salt, if not a detailed sensitivity analysis.

(2) To a control engineer, the Markov model the Authors apply to the detrended data

is a clear model of a feedback process and these can and do incorporate various time scales. For the model the Authors estimated, the eigenvalues of the system indicate time constants of approx. 3 and 50 days. In this context I could not help noting how the Authors got rid of the feedback coefficient with quite vague explanation. If this coefficient is non-zero, the resulting feedback system can easily have extremely long time constants, including a unit root (infinite memory). I strongly suggest that this "elimination" is made objective, that is based on data: estimated and not resulting from the Authors' preconceptions.

Finally, I also have a suggestion of a numerical study which might throw some light on the questions the Authors raise. I support the Authors' claim that the fractal tools may well be "tricked" by the presence of slower dynamic modes in the series. It would be interesting how the DFA is affected by "stiff" character of the time series dynamics. Perhaps a simulation study attempting to re-create the data series characteristics using both fractal and multi-modal mechanisms might serve as some supporting evidence?

My conclusion is that the manuscript, while valuable in terms of providing solid and questioning material for scientific discussion, should be revised to firm up the methodology, in particular addressing the two issues highlighted above.

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