Earth Syst. Dynam. Discuss., 3, C363–C368, 2012
www.earth-syst-dynam-discuss.net/3/C363/2012/

Earth System Dynamics Discussions

ESDD

3, C363–C368, 2012

Interactive Comment

Interactive comment on "Power-law behavior in millennium climate simulations" *by* S. V. Henriksson et al.

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Received and published: 10 September 2012

The review of Anonymous Referee 1 was very critical. Nevertheless, we thank him/her for comments. We replied to the hard critique regarding novelty and significance of the results in a Short Comment before and focus here on replying to the more detailed comments. The original comments are in italics.

This paper presents power spectra of global and regional temperature time series from millennium climate simulations and observed Central England Temperature. The authors make local regression fits of the power spectra and compare the slopes obtained from the various times series. They conclude that their results are similar to those found by various authors.

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Interactive Discussion

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General comments: The specific methodological details (not shown in the manuscript) are certainly fine, but I do not see how this paper increases any scientific knowledge that is already known (the authors already cite the literature reaching similar results). Hence the significance of the paper is extremely low (although I acknowledge that serious work was done). I cannot recommend its publication.

Our main conclusions are listed in the abstract, and the consistence of some results with earlier studies is only a small part of our results. See also our Short Comment in the Interactive Discussion. In response to Anonymous Referee 2 we added the reference (Vyushin et al., 2004) that indeed contains conclusions similar to some of ours (comparing simulations with measurements and concluding that external (volcanic) forcing improves correspondence). Nevertheless, we consider our method to be a novel way of improving the spectral estimates and identifying power-law ranges through averaging. We also consider that many aspects of the spatial distributions and the comparison with measurements bring important new insight.

Specific comments: The term "power-law behavior" refers to the limit when fre- quency tends to 0. Regression fits of the spectrum over regions of finite frequenc intervals have nothing to do with power laws. Hence, the terminology used by the au- thors (and many others for what matters) is abusive.

Anonymous Referee 1 probably means the limit when frequency tends to infinity and not zero. Although we can understand the point of view, that some authors might wish to call only asymptotically valid power law relationships power laws, we are not aware that this requirement would be a general convention and have clearly stated that the power laws are only valid in certain frequency ranges. In the introduction, we now also mention the alternative term of multifractality, referring to different frequency ranges having different power-law exponents.

The introduction presents a review of the subject, but the scientific challenges or objectives are not listed. As it stands, the paper looks like a report on "random" statistical *analyses, with no clear motivation. I do not understand how the methodology is an alternative to what has already been done.*

Our spectral method has not been applied to studying the full spectrum of temperature fluctuations in climate data before. The introduction now highlights the reason why the method to some parts improves on previous research: statistical averages allow better for identifying frequency ranges where power laws are valid. The introduction has also been modified, partly in response to the review of Anonymous Referee 2, to better illustrate the motivation of the study.

Please mention the space resolution of the model simulations. Are there biases in the model simulations? Trends? The times series of temperature that are going to be analysed should be shown.

The resolution is now mentioned in Section 2. There are some known biases in the model, like in any other global climate (earth system) model. Trends and detrending for the forced simulation are discussed in Section 3. The unforced simulation is essentially free of any trends, as the external forcings are independent of time (no solar variations, no volcanoes). Plots of the global annual mean temperature time series have been added to Figure 1.

p. 394, l. 4: What is meant by "we can assume the internal variability to be homogeneous throughout the 1201 simulated years"?

It means that when we split up the time series into different segments to produce spectra, we can average over the different spectra assuming that the fluctuations are different realizations of the same underlying climate dynamics. This follows from the lack of external forcings.

p. 395, l. 14: what am I supposed to "see"?

The fits are obviously better than those in the previous figures in the sense that the points are closer to the line. With confidence intervals added in the revised manuscript,

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also the confidence interval can be seen to be more narrow than in the previous figures.

p. 396, l. 14: p. 396, l. 28: why is it self-similar? How is this please define the goodness of fit. assessed? What does this imply

Self-similarity means that the spectral power decreases in a constant proportion when multiplying frequency by a certain factor. The sentence has been removed after modifications in response to Anonymous Referee 2, but the first sentence of the introduction still states the same fact. We have not defined a quantitative metric for goodness of fit, but as we can see visually and from the rather narrow confidence intervals, the simulated spectral powers are very close to the fits in many cases.

p. 397, last para. before section 4. This is a strange statement: the premise of the paper is that climate is a complex system and hence has power law spectra (whatever that means). Then the authors seem surprised to find properties relating to power laws. The reasoning seems rather loose to me.

The fact that the temperature fluctuations seem to follow power laws is a conclusion, not a premise, of the paper. The last paragraph before Section 3 has been modified (also in reply to the other referees' comments) to avoid making possibly vague statements.

P. 398, l. 1: who do you expect to study Fig. 3a? The following paragraph does not make much sense to me.

Interested readers of the article may study Fig. 3a for inspiration for future research. The idea for further studying how well the spectrum follows the power-law form is connected for instance to observed spectral peaks at 20-30 year periods in the North Atlantic (e.g. Frankcombe et al., 2010 and Figure 7 of the present manuscript). The rest of the paragraph describes in what sense the fits for the highest frequencies are not as good as for the multidecadal to El Nino frequencies.

P. 401, top: comparing the relative contributions of volcanic and solar forcings only makes sense if the way they are taken into account in the model simulations is dis-

cussed.

A brief description of the volcanic and solar forcings has been added to the section describing the model. More information can be obtained in (Jungclaus et al.?, 2010) and references therein.

I am very puzzled by the horizontal axes of the spectra, which are never expressed in standard units (e.g. cycles per year), so that figures can hardly be compared.

Units of the figures have been changed to cycles per year in the revised manuscript.

I am also generally confused by the values of the slopes. A regression to obtain beta should mean that beta is positive when the slope is negative in a (S(f), f) diagram. This does not seem to be the case in the manuscript.

We define the power-law relationship as $S(f) \sim f$-β. Beta is positive when the slope is negative and vice versa. In the context of spatial distributions, Anonymous Referee 1 is right in that there was a contradiction between beta in the text and exponent (negative in most places) in the figures showing the spatial distributions. This has now been corrected.

The histogram in Fig. 6b does not reflect the range of values in Fig. 6a. Why? The maps hardly allow one to locate Central England.

Figures 6a and 6b describe different residuals. They were selected to have a clear picture of variability from one frequency to the next in Fig. 6a and more statistics in the histogram in Fig. 6b. It is true that it is hard to visually distinguish the local features around Central England; therefore the data used in Section 6 are local measurements and data selected from a local gridpoint in the simulation.

Frankcombe, L. M., von der Heydt, A., and Dijkstra, H. A., North Atlantic Multidecadal Variability: An investigation of dominant time scales and processes, J. Clim, 23, 3626-3638, 2010.

Interactive comment on Earth Syst. Dynam. Discuss., 3, 391, 2012.