

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

**Anonymous Referee #1**

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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.

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.

Specific comments – The term “power-law behavior” refers to the limit when fre-  
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quency tends to 0. Regression fits of the spectrum over regions of finite frequency intervals have nothing to do with power laws. Hence, the terminology used by the authors (and many others for what matters) is abusive. – 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. – 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. – p. 394, l. 4: What is meant by “we can assume the internal variability to be homogeneous throughout the 1201 simulated years”? – p. 395, l. 14: what am I supposed to “see”? – p. 396, l. 14: please define the goodness of fit. – p. 396, l. 28: why is it self-similar? How is this assessed? What does this imply? – 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. – P. 398, l. 1: who do you expect to study Fig. 3a? The following paragraph does not make much sense to me. – 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 discussed. – 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. – 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. – 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.

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