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

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It seems the Editor Comment was coincidentally posted by Dr. Huber at the same time as I was uploading a revised manuscript. Therefore a reply in this interactive discussion is likely to be in place.

Our main claims of novelty are methodological only for the part that we use averaging of spectra, and also this in the climate context, not time series analysis in general. See for instance such 'clean' figures as Figs. 3-5 in the manuscript. By 'clean', we mean that it can be clearly seen to which frequency ranges power laws fit, especially in Figs. 3a, 4 and 5. Alternative frequency ranges (e.g. multidecadal to Nyquist frequency) would not allow for equally good linear fits, something that is easily seen in our results and that might be missed in unaveraged spectra. It is this averaging and the advantages it

brings that is the main novelty of the paper.

It should be noted that it would also be possible to perform the same study as we did using some other, e.g. the multitaper method, but averaging the spectra would be the important then too. In the revised manuscript, such checks have been performed in Section 4 in response to a comment by Anonymous Referee 3. The checks show an advantage (narrower confidence intervals) and a disadvantage (best-fit betas outside each other's confidence intervals) of the multitaper method as compared to ours, and we have opted for the more simple method to allow for easier understanding also to readers without specialization in time series analysis.

We disagree having danced around the issue of novelty of the spectral method. Firstly, we mention our own previous article in press for Climate Dynamics using the same method and are thus not introducing the method in the present article. Secondly, we have responded directly also to the critical comments, admitted e.g. that we missed the (Welch, 1967) reference in the first version of the manuscript and added it to the revised version.

The Blender/Fraedrich studies referred to by Dr. Huber are surely important contributions to the field, but instead of having established the nature of power laws in atmospheric temperature data to all important parts, they have received controversial responses (Vyushin et al., 2004 and especially Rybski et al., 2008). Therefore we think further research, such as our manuscript, are welcome contributions to this discussion.

In summary of this reply and the interactive discussion, good arguments requested by Dr. Huber can be listed as follows:

1. Averaging over spectra allows for improved identification of power-law ranges, improved fits and also narrowed confidence intervals.

2. Further results and illustration of the important impact of selection of frequency range in the context of spatial distributions. This impact also illustrates why point 1. is

so important.

3. Good agreement with measurements (although it turns out that for global mean temperature, such comparisons have been made before, the agreement for the CET record between model and measurements is new and encouraging).

4. We have dealt with the issues brought up in the reviews.

Naturally, we are more than willing to discuss any of the issues further to reach common understanding.

Respectfully,

Svante Henriksson

Vyushin, D., I. Zhidkov, S. Havlin, A. Bunde, and S. Brenner (2004), Reply to comment by R. Blender and K. Fraedrich on "Volcanic forcing improves atmosphere–ocean coupled general circulation model scaling performance", Geophys. Res. Lett., 31, L22210, doi:10.1029/2004GL021155.

Rybski, D., A. Bunde, and H. von Storch (2008), Long-term memory in 1000-year simulated temperature records, J. Geophys. Res., 113, D02106, doi:10.1029/2007JD008568.

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