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Interactive Comment

Interactive comment on "Late quaternary temperature variability described as abrupt transitions on a $1/f$ noise background" *by* M. Rypdal and K. Rypdal

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Response to Lovejoy

Some of the comments by the reviewer are important in the sense that they point at the difference between the approach we have adopted in this paper and the approach advocated by the reviewer. Others are continuations of an ESDD discussion on another paper. For our reply to those comments we refer to our response to Lovejoy in that discussion (Nilsen et al., 2015). The referee's tendency to divert attention from the actual issues of our paper is problematic. In order to keep focus have decided to attend to those comments that are relevant and important. These are concerned with the multifractal characterization of paleo time series of quaternary climate.

Are the authors old-fashioned and illiterate?

The reviewer claims that our analysis is based on an old-fashioned theoretical framework, implicitly suggesting that we do not master the multifractal formalism. He may not be aware that we have published a significant number of papers employing multifractal analysis to solar, magnetospheric, and financial time series. We know by experience that a multifractal characterization does not always make sense. In particular, an intermittent (in the meaning "bursty") time series is not necessarily multifractal. Multifractality means that there is burstiness and clustering of bursts down to the smallest resolved scales. This is not true for for quaternary ice core data, and in general not for surface temperature data. In the revised paper we add a subsection where we demonstrate that these data are not multifractal.

DO events are not an expression of multifractality

The DO-events featured in the Greenland ice-core data during the last ice age do not lead to multifractality, but to a shift in the scaling exponent at millennium time scales. The simple reason is that DO events and the glacial-interglacial transitions invoke amplitudes, durations and waiting times with characteristic scales. The referee writes that if we "allow the process to be multifractal then the DO events may be expected as necessary manifestations of the intermittency (multifractality)!" In our analysis we find no evidence that supports this claim. Surely a multifractal model will give rise to bursts, but nothing that looks like DO events. The DO event phenomenology requires something more specific than a multifractal scaling model. This is the reason why we did not address the DO events as such in this short paper, and the reason why we did not find it very relevant to discuss the ideas of Lovejoy and Schertzer on multifractality. Nevertheless, in the face of this criticism by the referee, we have decided to include a multifractal analysis to prove the point.

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Discussion Paper

Second-order statistics is not quasi-Gaussian and monofractal

The referee describes our scaling analysis in the original manuscript as quasi-Gaussian and monofractal. This is not correct. By considering only second-order statistics we just avoided using an over-detailed map that does not fit with the terrain. *Power-law scaling of the second-order structure function, which is equivalent to power-law scaling of the spectral density, implies neither Gaussianity, nor monoscaling, nor multifractal scaling.* The reviewer seems to forget that multifractality is not a generalization of second-order statistics. On the contrary, multifractality is infinitely more restrictive, since it requires that structure functions of all orders are power-laws, while scaling in second-order statistics only requires that the second-order structure function is a power law.

The "subjective" identification of DO events

In the paper we isolate the DO events and transitions between stadials and interstadials as determined by quantitative phenomenological criteria in Svensson et al. (2006). These criteria are not subjective, as claimed by the referee. Nevertheless, in the revision we show by multifractal analysis of the entire glacial ice-core series, without excluding the stadial-interstadial transitions, that the effect of the events is to introduce a higher scaling exponent for time scales larger than a millennium than for smaller scales. In each of those scaling regimes the structure for a range of q's are power laws, and the scaling function is linear. The latter is not what is expected from a multifractal signal.

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Selection of scope is not “ignoring”

The paper was never intended to be a critique of Shaun Lovejoy’s ideas. In fact, we have used the same estimators as employed in many of Lovejoy’s recent papers on scaling in paleoclimatic data. In these papers structure functions and multifractality are usually superficially mentioned, but only second-order statistics is put into practical use; namely the power spectral density and the Haar-fluctuation (e.g., Lovejoy, 2014). Thus, it is a bit difficult to understand Lovejoy’s outrage when other authors do exactly the same. In fact, our idea is not so different from the “macroweather” scaling concept of Lovejoy and Schertzer. The main difference is that according to our analysis the macroweather scaling is not limited to time scales up to a century, but is present as a background noise on all scales in all stages between the abrupt transitions.

Revision

With this paper we wanted to forward some simple ideas without being dragged into an endless discussion about Shaun Lovejoy’s work. We realize that we did not quite succeed, so in the revision we have added a multifractal analysis of all the data sets for which we presented power spectra and wavelet variance spectra in the original manuscript. These results confirm the second-order statistics results, and quantifies what a trained eye can see directly from the data; the time series exhibit abrupt transitions, but they are not multifractal.

The discussion of the use of the parameter H belongs somewhere else, but we have made a comment on it in our reply to the editor’s comment. We don’t need this parameter in this paper, so in the revision we stick only to the exponent β. On the other hand, there *was* an issue with the use of the “climacogram” for non-stationary time series, which also was mentioned by reviewer Ditlefsen. We discuss this issue in our reply to him. We don’t put this estimator to use in the paper, but our reference to it in the original manuscript was misleading. We correct this in the revision by replacing it with the Haar fluctuation.

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Please also note the supplement to this comment:
http://www.earth-syst-dynam-discuss.net/6/C1067/2016/esdd-6-C1067-2016-supplement.pdf

Interactive comment on Earth Syst. Dynam. Discuss., 6, 2323, 2015.