Nonlin. Processes Geophys. Discuss., 2, C344–C346, 2015
www.nonlin-processes-geophys-discuss.net/2/C344/2015/

Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "An inkling of the relation between the monofractality of temperatures and pressure anomalies" *by* A. Deliège and S. Nicolay

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Received and published: 23 August 2015

This is a too short paper, devoted to a field which is well studied, but here using new methods, which are not well documented, and the link with available literature is not satisfactory. I must propose to reject the manuscript because there are too many weaknesses.

- The method proposed to test for monofractality is not convincing. It is applied to two deterministic functions. It should be applied e.g. to a fBm. Furthermore, a good test of monofractality is simply to check if a moment function is linear. The most classical form is the structure function, for non-stationary data with stationary increments. There is no need for a complicated phase shuffling as proposed here. Or if this method is a powerful one to test for monofractality, the authors should prove that it is better than simply to see if a moment function is linear.

- The data considered are not mean quantities, there are the mean between daily minimum and daily maximum. This is a rather strange mixture and it seems more realistic to estimate a better defined mean value, if available.

- There is a strange sentence, line 17 p. 1343: "For the purpose of reducing the noise, the data f(t) were replaced by their temperature profiles". What noise? Why is it called a noise, is it a stochastic variability? Scaling methods are indeed devoted to characterize stochastic variability and averaging in order to smooth out fluctuations is not recommended, at least prior to any analysis. What is t in the proposed equation (line 18)?

- Some portion of the time series are not displayed. The scaling range of the data is not commented nor shown. Figures showing the scaling range are needed before any plot of the scaling exponents. Some power spectra for some stations should be provided.

- Most studies of temperature fluctuations show that it is a multifractal quantity. The monofractal claim here is an important point which is not enough documented. Most likely, it is false.

- The link with available methodologies and data analysis studies is not satisfactory. There are many other methods to test for monofractality or multifractality and these are not discussed. There are many studies in the topic of scaling analysis of climate temperature, which are not cited nor discussed. A quick google scholar search gave me the papers below (and many others are certainly existing):

Yu Zhou and Yee Leung J. Stat. Mech. (2010) P06021. doi:10.1088/1742-5468/2010/06/P06021

R. O. Weber and P. Talkner, JGR Atmospheres 106 (2001) 20131. DOI: 10.1029/2001JD000548

R.D. Valdez-Cepeda et al., Fractals 11 (2003) 137. DOI: 10.1142/S0218348X0300163X

N. Yuan et al., Theor. Appl. Climatol. 112 (2013) 673. doi 10.1007/s00704-012-0762-3

J. Alvarez-Ramirez et al., Physica A 387 (2008) 3629. doi:10.1016/j.physa.2008.02.051

G. Lin and Z. Fu, Physica A 387 (2008) 573. doi:10.1016/j.physa.2007.10.011

D.M. Sonechkin and N. M. Datsenko, Pure Appl. Geophys. 157 (2000) 653. doi 10.1007/978-3-0348-8430-3_11

H Millan et al., Atm. Res. 88 (2008) 355. doi:10.1016/j.atmosres.2007.11.030

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1339, 2015.