Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2017-16-RC1, 2017

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on “Constraining on the Stationarity of Signal with Time-Frequency Surrogates to Enhance the Reliability of Singularity Spectrum Attributes of Random Seismic Noise Wavefield” *by* Amir Ali Hamed et al.

Anonymous Referee #1

Received and published: 15 July 2017

Abstract: 1) the authors state “Existence of a self-afïňĄne long range persistence in the seismic noise time series evidences that the current state of system is not in the pure diffused regime and transition from coherent to incoherent motion is still on progress” however not in the Results section nor in the Disussion section it was ever strengthened such statement on the base of the obtained results, leaving it sospended and without a clear connection with all the performed analysis. 2) It is not clear (in the worst case, not correct) that the long-range correlation of a stationary time series (with $0 < h(2) < 1$) can be discerned from a non-stationary process. Stationarity and non-stationarity are

different characteristics and such relationship that makes one to be discerned from the other is obscure. 3) The authors say “changing in the fractal properties in the crossover time scales in time series don’t permit us to ascribe a single amount for h(2)”, probably the authors link the multifractality to the crossover time scale in the fluctuation functions. This is not what multifractality means. The crossover in a fluctuation function indicates just the co-existence of two different dynamics at different time scale ranges. However, in the paper no crossover timesclaes have been mentioned; so such sentence does not reflect any aspect of the topic approached in the paper. 4) Most important: the authors claim that their pre-processing (related with the identification of stationary segments) improve the accuracy of the results. But, no comparison with other methods has been provided, to assess that their pre-processing not only is original but also effectively improves the accurary of the results.

Introduction: 1) It is not clear the concern of the authors in selecting the stationary intervals of signals before using the MFDFA, if the MFDFA is already capable to deal with nonstationarities. Moreover, the authors have not clarified, or not mentioned at all, what type of nonstationarities would affect their data, so that the application of the MFDFA directly would produce misleading results. If the nonstationarities of their data are among those types that MFDFA would be able to deal with, why the pre-processing is proposed in the next sections?

Methodology 1) The title of subsection 3.1 seems not appropriate, since the fractality of a signal can be detected or identified and not learned 2) The authors say that “a short part at the end of the time-series remains in the most cases, which we have ignored them from the cycle of MF-DFA”, practically they do not perform the MFDFA in the reverse side of the time series in order to not disregard the last part of the time series that is necessarily left out. This is not well-done procedure, because even the last part of the signal could contain information, in principle, that could be useful for the overall calculation of the fluctuation function. Actually, you don’t know if including such small part that is left at the end of time series would change significantly your results or not.

So, it would be much better to apply the MFDFA a sit was proposed by several authors, so, forward and backward. 3) It is obscure at all, the summation the authors did in eq. 3; practically they sum over the three different time series, so instead to consider one set of fluctuation functions (depending on q) for each time series, they summed for each q the three fluctuation functions obtained for each time series. I suppose that each time series refers to one direction of the sensor by which the seismic noise was measured, one vertical and two horizontal. Actually, it would has been much more useful and informative of the underlying geophsyical process to analyse each direction separately. Probably it would has been better to first calculate the total displacement combining the three time series and then apply the MFDFA on such total displacement. 4) Eq (6) is not correct, because the zero-values are of the parabolic fitting function and not of Dq, calculated from the data. 5) At page 7, the authors say that the "phase structure, which controls the non-stationarity. . ." this is not correct, because the phase are responsible of the non-linearity of a time series. The stationarity/nonstationarity of a time series can be simply verified looking at the power spectrum and its power-law shape, which depends solely on the amplitude of the Fourier transform and not on the phase. 6) The Eq. 10 seems misleading if compared with the analogous Eq. 3 of Borgnat et al. (2010), because the multitaper spectrogram that the authors use is evaluated only at time positions tn, with n=1,..N, with a spacing which is an adjustable fractionâĂŤtypically, one halfâĂŤof the temporal width Th of the K windows Hk(t), which are the Hermite functions. In Eq. 10, the authors use to vary a continuous parameter t from 0 to T. So, in the eq. 3 of Borgnat et al. (2010) the averaged spectrum is done on N local spectra. It is not clear on how many local spectra the average is done in Eq. 10 of the present paper. 7) Eq. 15 seems not correct (compared with eq. 16 of Borgnat et al. (2010)) 8) At page 8, the authors mention the parameter Tn as the window length of spectrogram, but they never used or defined it previously. 9) The equation (20) is introduced but never used.

Results 1) At page 9, N/6 samples correspond to 600 seconds and not 6 seconds. 2) The authors mention that they calculated the modified Fq to compensate the ef-

fect at small scales, but at the end they showed in Fig. 3a the un-modified Fq. 3) Fig. 3a shows that the fluctuation functions by approximately two differente regimes, at small scales until approximately and at long scales from 1 until the end of the investigated timescale range. The authors do not do any comment on such apparent double regimes and seems that they have calculated the scope of the lines fittings each fluctuation function in the entire range of scales (actually, the authors have not clarified/specified if they used the entire scale range, or part of it). If they used the entire range, the results obtained about the multufractality are absolutely wrong. 4) It is lacking a convincing geophysical explanation of the link between the tectonics of the area and the found multifractal paramters.

Interactive comment on Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2017-16, 2017.