

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.

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The author would indeed prefer to answer these three questions together. Q (1) 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 be discerned from the other is obscure.

Q (2) 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?

Q (3) 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?

Answer:

In general, there is broad agreement on the appropriateness of MFDFA in studying multifractal scaling behavior of non-stationary time series, but we wish to draw your attention to the paragraph "III. ANALYSIS OF SUNSPOT TIME SERIES" of Movahed (et al., 2005) which explains the reasons why further attention must be taken in the analyzing the stationarity of signals in the pre-processing step, before feeding them into the cycle of Fractal analysis. In connection with this point, we MUST make it absolutely clear that inherent non-stationarity of signals should never be confused with the concept of non-stationarity made by external perturbations. Regularly, fractional Gaussian noises (fGn) are involved in the inherent stationarity process, in contrast, fractional Brownian motions (fBm) are linked to the inherently non-stationarity process. The stationarity of fGn signals can be characterized by two parameters, σ^2 , the variance, and H, the Hurst coefficient, while a fBm process has a time-dependent variance. Hence, on the basis of the class to which signals belong, different techniques may be required for processing. Failure to match signal class with the appropriate method of fractal analysis results in serious error in the estimating H. For instance Dispersional analysis (Disp) is recommended to the analysing the fractionality of fGn signals, while bridge de-

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trended scaled windowed variance analysis (bdSWV) is suitable for fBm signals (Eke et al., 2000). These classes might not be a-priori known, so a preliminary interpretation in this regard may be available by fitting a straight line of slope $-\beta$ on a log-log plot of the periodogram. Based on this method, signals can be categorized according to the value of β , but Eke et al., (2000) placed emphasis on this point that this method is only applicable if β falls into the category $-1 < \beta < 0.38$ (for an obvious stationary case) or if falls into the range of $1.4 < \beta < 3$ (for an obvious non-stationary case), but there is no certainty that this method fully comply with the complicated characteristics of signals in the range of $3.8 < \beta < 1.4$ where stationary and non-stationary mixed into each other. Based on this assumption, it is essential to provide another reliable framework for a regular monitoring of stationarity of signals. Signal summation conversion method (SSC) is advised to use as a discriminating method (Eke et al., 2000). An alternative approach for distinguishing fGn signals from fBm signal was proposed by Movahed et al., (2005) who experimentally attempted to examine the feature of Sunspot Time Series by analyzing the behavior of standard deviation of its time series as a function of timescale. There is a growing consensus amongst researchers (Zhong, et al., 2015; Wang et al., 2014) that Time-Frequency Surrogate Analysis (TFSA) proposed by Borgnat et al. (2010) can provide a complementary view for testing stationarity of seismic signals in an operational sense. This method is statistically characterized on the basis of a set of surrogates which all share the same average spectrum as the analyzed signal while being stationarized. With this short introduction, the answers to the above-mentioned question can be summarized briefly: We, therefore, agree with this statement in your question: Stationarity and non-stationarity are different characteristics, and this is exactly what we need. In other words, this difference enables us to discern a stationary fGn from non-stationary fBm. We want to underline the point that nonstationarities from our point of view are those corresponding to inherent non-stationarity which should be known at the pre-processing step before making our choice between fGn and fBm process. TFSA is a rigorous and sufficiently flexible method which not only provides an opportunity for assessing the inherent stationarity/non-stationarity of a signal but

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also further it may bring information from their state of stationarity. Therefore, another strength of TFSA lies in its capability to constrain the length of stationary of the signal by quickly and reliably validating tasks. This would be of utmost importance if our data encompass the microseismic range of frequency (0.1-0.3 Hz) or if data were acquired from the surveys at the vicinity of dome-looking topography features seismic signals appear mostly in the quasi-stationary state. In those cases, reproducible seismic signals could fall into one of the following states: macroscale, mesoscale or microscale state (Fig 1), therefore, more accurate method is needed in order to properly assess the state of this quasi-stationarity. In this paper, we do make a point that the length of the signal directly impacts on the reliability of Long-range autocorrelations assessment. The importance of this factor was previously the subject of other investigations such as Delignieres et al., (2006) and Warlop et al., (2017), but this point has been hidden from view in analyzing the fractality of seismic signals. The importance of this issue is strikingly apparent for seismic signals since the stationarity of seismic records at various frequency ranges or temporal length are different. The experimental estimates obtained by Gorbatikov & Stepanova, (2008) shows that, at the microseismic range of frequency, signals are mostly quasi-stationarity, but this stationarity may not be preserved for very long periods of time. For instance, this interval might be lengthened to the several days or be shortened to the 1–1.5 h, while Wang et al., (2014) showed that for frequencies above than 1 Hz signals, the stationarity range of the signal is just in the range of several seconds. This is where we think we have to focus much of our attention. Obviously, by choosing an extreme short window length users might thereby misunderstand the true origin of non-stationarity, in such a way that lead to results that are non-informative, and potentially misleading. Accordingly, we will be able to adaptively adjust the length of processing incompatible with the stationarity length of the signal, if we take full benefit of the advantages of TFSA.

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Q (4) The authors state “Existence of a self-affine long-range persistence in the seismic noise time series evidence 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 Discussion section it was ever strengthened such statement on the base of the obtained results, leaving it suspended and without a clear connection with all the performed analysis.

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Answer:

Generally speaking, a local perturbation may continuously fluctuate system over time in a complex manner, such that consecutive cycles of a signal exhibits an interdependency spanning over long time intervals. Indeed, the existence of this type of long-range correlation (or self-affine long-range persistence) in the seismic noise wavefield is mostly associated with the existence of coherent signals (e.g. P, S, and Surface waves). Matcharashvili, et al. (2013) showed that the dynamical features of ambient noise undergo essential changes during preparation, and also after triggering the activity of strong local events. At distance far away from a seismic source, the generated waves bounce on several heterogeneities and gradually enter in the multiple scattering regimes (see Fig. (2)). Therefore, the diffused scattered coda waves overwhelm the direct wave, gradually. In this case, the strength of persistence of signal diminishes through time, due to the attenuation of the coherent wavefront. Therefore, the footprint of a perturbation should be tracked in the ambient noises and the existence of a self-affine long-range persistence in the seismic noise time series evidences that this system is still in the process of transition, that is, transition from a complete coherent state to a pure diffusive state. In this point of view, the scattering mean free time, τ , indicates the characteristic time after which such transition is happening. The scattering mean free time is the key feature of a medium since it allows knowing in advance the degree of heterogeneity of that medium. A medium with low levels of heterogeneity is no longer considered as a candidate site for executing the advanced process such as seismic interferometry (see, for instance, see Wapenaar, 2012 a,b), since the existence of a diffusive wavefield is a key precondition for this analysis (Pilz & Parolai 2014). With this short introduction, the answers to the above-mentioned question can be summarized briefly: In short, this sentence “Existence of a self-affine long-range persistence in the seismic noise time series evidence that the current state of system is not in the pure diffused regime and transition from coherent to incoherent motion is still on progress” is evident from recent studies not directly the conclusion of our research but we MUST give the specific reference for this argue. Our paper is actually

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the continuation of the effort made by Pilz & Parolai (2014), much of our focus was on improving processes, with the enhancing the quality of fractal analysis.

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Q (5) The title of subsection 3.1 seems not appropriate since the fractality of a signal can be detected or identified and not learned.

Answer:

Thanks, we agree. This will be revised definitely.

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Q (6) 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

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refers to one direction of the sensor by which the seismic noise was measured, one vertical and two horizontal. Actually, it would have been much more useful and informative of the underlying geophysical process to analyze each direction separately. Probably it would have been better to first calculate the total displacement combining the three-time series and then apply the MFDFA on such total displacement.

Answer:

Actually, we followed a process similar to the one outlined in the "Casetra, et al., 2007" provided in Eq. (2). The similar approach has been taken by Pilz & Parolai (2014) in Eq. (2). The rationale behind this suggestion was given by Casetra, et al., (2007) as:

"Moreover, we consider the 3D soil displacement instead of its three components because we are interested in studying the global soil motion under the effect of seismic noise; considering and comparing the motion in each component separately (H/V spectral ratio, etc.), could be done in a next paper (Casetra, et al., 2007, p. 259)".

In any case, we feel that your comment is well-founded and that there needs to be the reflection on the matter in the advanced processing.

Q (7) Eq (6) is not correct because the zero-values are of the parabolic fitting function and not of Dq , calculated from the data.

Answer

We exactly do the method introduced by Eq. (9) in Padhy (2016), or by Eq. (21) in Shimizu et al. (2002). Maybe we don't quite understand the question of the referee. We would appreciate if you could notify the problem of this method to us.

Q (8) At page 7, the authors say that the "phase structure, which controls the non-stationarity. . ." this is not correct because the phase is responsible for 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 in the phase.

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I appreciate for pointing out the mistakes I have made. It will be revised.

Q (9) In response to questions and comments from referees concerning the TFSA, we try to restate the method in the clear way in the attached supplement.

Q (10) At page 9, N/6 samples correspond to 600 seconds and not 6 seconds. Answer

Signals have been recorded at 50 sample per second so each one of these signals has $N = 3600 \times 50 = 180000$ samples. By limiting the size of segments into the $h \leq N / 6$ samples, that is, $h \leq 600$ s. Sorry for this mistake in typing.

The authors mention that they calculated the modified F_q to compensate the effect at small scales, but at the end, they showed in Fig. 3a the un-modified F_q . Fig. 3a shows that the fluctuation functions by approximately two different 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 it 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 multifractality are absolutely wrong. 4) It is lacking a convincing geophysical explanation of the link between the tectonics of the area and the found multifractal parameters.

Please also note the supplement to this comment:

<https://www.nonlin-processes-geophys-discuss.net/npg-2017-16/npg-2017-16-AC3-supplement.pdf>

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