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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 paper presents several flaws in the organization, in the methodological approach and in the presentation of the results. Unfortunately the English is not fluent and the clarity of the sentences and concepts is not always achieved. Even the abstract, which in principle has to outline clearly and synthetically the main findings of the study, seems quite obscure and does not convey clearly the information on what is the outcome of the presented research. For instance the starting paragraph of the Abstract "Existence of a self-affine 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. Rate of this evolving transition can be indirectly linked to the degree of heterogeneity of medium. . . ." seems not well explained and it would be difficult for a reader to understand what is exactly its meaning. Please, be aware that at least the abstract should be developed in a manner that even a reader not strictly familiar with the topic of the paper can capture the general information. Unfortunately, the whole abstract fails in the characteristics of clarity, synthesis, clear explanation of the obtained results.

Such lack of clarity is also evidenced in the description of the dataset. It is not mentioned how many stations have been analysed, although one can guess them from Fig. 1; but probably an explanatory table indicating name, geographic coordinates, elevation, and maybe some simple statistical characteristics, would have been useful to add to make the text clearer. However, the authors say that after removing mean and trend (which trend? linear trend? a figure with the raw data would have been useful), they merged all the different length segments; but how such merging was performed? Then since the data present gaps "stemmed from the zeroed out spikes and overlaps" (what overlaps?), this gaps were filled with linear interpolation; but this interpolation is not clearly explained, and the number and the length of gaps is not specified: these details would be important to mention especially in a journal like NPG, where a relevant focus is given on the methodological aspect of presented study.

Some flaws also exist in the methodology. For instance it would have been more correct to link the persistence/antipersistence of a signal to the succession of the increments rather than of the signal values. It is correct the observation of the referee about what the authors did, ignoring the small part of the signal at its end that remains out during the calculation of the fluctuation function, since in most of the studies such small part at the end of the signal has not ignored but included recalculating the fluctuation function starting from the end of the signal. Also the use of the multitaper spectrogram (Borgnat et al., 2010) seems not correctly performed or at least not clearly carried out, raising issues on the correctness of the obtained results.

The authors apply a complex signal pre-processing for searching the stationary windows to apply MFDFA. Besides the logical observation of the referee that the algorithm of the MFDFA is already developed in a way to remove the non-stationarities (thus making probably quite unuseful or unnecessary that pre-processing), it would have been, instead, much more useful, to apply the MFDFA directly to the signals (as obtained after the procedure described in section 2) and then to such stationary segments (and thus, after the pre-processing) to check if any difference would have been existed and to see if an improvement would have been obtained in the results, especially in relationship with the geophysical implications. I am also skeptical about the obtained results, because it seems that the calculation of the slopes of the fluctuation functions in Fig. 3 was performed considering all the available shown scales; if so, this is clearly wrong, because the fluctuation functions for any q are not linear in log-log scales. So, if the geophysical interpretation of the results are based on such wrong calculations of the slopes of the fluctuation functions, also all the geophysical implications, rather poorly described by the way, would be not convincing.

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