Comments on
"Mahalanobis distance based recognition of changes in the dynamics of seismic process"
by Teimuraz Matcharashvili, Zbigniew Czechowski, Natalia Zhukova

A novel method is proposed, based on well-established Mahalanobis metric, to quantitatively assess significance of the changes (i.e. switches from randomness to non-randomness) in the seismic process, by means of a simultaneous (multivariate) analysis of the so called increments of cumulative time, distances and energy (ICT, ICD and ICE) between consecutive earthquakes in the catalog.
The manuscript illustrates the results obtained by the method's application to real data, from the Southern California earthquake catalog, as well as to synthetic sequences of events, generated by two different non-linear models, one non-Earth specific (i.e. the Lorenz model for an incompressible fluid) and one more closely connected to earthquakes (i.e. the Crack Fusion model).
I believe the proposed approach is interesting and pretty general, and the method might be further expanded to explore other possible patterns in earthquakes occurrence. Therefore in my opinion the manuscript is definitely worth publication, after some necessary revision, taking into account following comments and suggestions to the authors.

General comments:

1) Language, as well as text organization, should be significantly improved throughout the manuscript. Specifically, the organization of the manuscript in sections and sub-sections could be improved as follows:

- Used data and methods would be better split in two different sections. Data description (i.e. text from line 100 and up to line 120) could be slightly expanded (see specific comments below);
- Method description (i.e. text from line 121 to line 201) should be included in a separate "Method" section;
- Method testing on synthetic data, generated by Lorenz and Crack-fusion models, should be included in a separate section. The section should include text from line 202 to 250, plus lines from 264 to 290 (including current figures 4 and 5) and could be titled "Testing the method on models";
- The section "Results and discussion" should be focused on results obtained from real data only;
- The part of the text describing the analysis performed for different representative magnitude thresholds, namely M3.6 and M4.6, from line 468 to line 512, should be included in a separate sub-section (possible title: "Testing stability of results with respect to minimum magnitude").

Careful proofreading would make the text more readable and understandable in several parts: some terms seem not to be used properly, some parts are unnecessarily repeated and some quite obvious statements could be removed (see also specific comments below).

2) The method description is quite general. I would suggest the authors to add, if possible, an Appendix explicitly explaining how the method is applied to real earthquake data, namely:
- How the "derivative quantities" ICT(i), ICD(i) and ICE(i) are computed and how they are "normed" to their standard deviation (i.e. which data are used to compute the standard deviation)? How it looks like the distribution of these quantities (e.g. is it Gaussian)?
- How D^2 is computed from the three quantities ICT(i), ICD(i) and ICE(i)? Which equation is used? How it looks like the covariance matrix Si (see lines 174-175) in terms of ICT, ICD and ICE?
- How it is estimated the number of degrees of freedom for the specific F-test?
These might appear obvious aspects; however in my opinion such detailed information would help the reader to better understand the method, and would make the obtained results replicable.

3) A critical point in the analysis could be related to the duration of the temporal windows associated with a fixed number of events. In fact, considering a fixed number of events (i.e. n=50 events), the related time span is longer during periods of low activity ("quiet" periods), while it is shorter during periods of high activity, such as during aftershock sequences (particularly after large earthquakes). The variability of the temporal window might have some influence on the obtained results. This is suggested also by the results presented in figure 17, where a smaller number of events is included in the analysis; in this case it is shown that the smoothing for n=50 does not allow to appreciate the low value of MD preceding some large events, while for n=30 (corresponding to a shorter time span) the pattern is visible again. In my opinion it could be interesting to perform a similar analysis over time windows of fixed length. I would suggest the authors to add a comment about this aspect.

4) Based on results illustrated in figures 13-15, the authors conclude that during periods of relatively small earthquakes, with magnitudes not exceeding M=4.6 and far from strong events, seismic activity is close to random. On the other side, after larger events a switch from random to more regular behavior is detected. Thus it seems that a different behavior is detected depending on magnitude. The specific magnitude threshold M4.6 considered in this study has been proposed by Hough (1997). Would this "critical" magnitude threshold depend on the considered region? How this threshold could be determined for other areas? I feel authors should expand a bit the comments about this difference between relatively "strong" and "small" earthquakes.

Specific comments:

- Consider replacing "exactly" by "specifically" throughout the text.
- Abstract (lines 19-20): the sentence should be reformulated; it is not clear that the " *different representative threshold values*" refer to the completeness magnitude threshold of the data.
- Consider replacing "normed" by "normalized" in data description.
- Introduction (lines 71-73). The sentence *"In common parlance...replaced by disorder."* is not clear. It should be reformulated or removed.
- Used data (lines 117-120): Data completeness for M2.6 and above is assumed by the authors, relying on earlier studies and analyses. However the meaning of

the sentence: *"we declare that take responsibility on the trustworthy of our analysis*" is not clear. Data description could be expanded, e.g. showing the frequency-magnitude distribution of events, commenting on the number of large events, etc.

- Method (lines 157-159): The sentence *"To be more precise... of the investigated process."* does not seem to add any information about the relevance of the considered data sets. The sentence should be removed or reformulated, so as to explain why the considered features are adequate to this specific analysis.
- The definition of "three dimensional system" and the related use of abbreviations (e.g. 3D) should be used consistently throughout the text.
- Figure 3. Top panel: authors may wish to consider providing seismic energy in logarithmic scale, if appropriate. Bottom panel: the meaning of white circles and bars should be explained in caption.
- Results from real seismicity (lines 300-306). I would suggest the authors to consider the recent paper by Kossobokov and Nekrasova (2017 - "Characterizing Aftershock Sequences of the Recent Strong Earthquakes in Central Italy", Pure Appl. Geophys.. 174: 3713–3723), and include the related reference in their comments, if they feel appropriate.
- Figures 7-9 are similar and can be grouped into a single figure (panels a, b, c). The caption would be the same, except for the time span to be provided for each of the three panels This should also facilitate the comparison and avoid repetitions in the text.
- Results (lines 362-366). The sentence *"Here need to be underlined that... becoming again dominant with respect to the rate of aftershocks' occurrence [Godano, C., Tramelli, 2016]."* is not clear and should be reformulated.
- Results (lines 386-387) Consider replacing "*assessing it by the variation of ICT(i), ICD(i) and ICE(i) data*." by: "assessing it by MD variation." Similarly, at lines 395-396, replace: *"according to distribution of its ICT(i), ICD(i) and ICE(i) characteristics*" by: "according to MD values".
- Results (lines 400-402). The sentence "*It can be added here...(e.g. M6.0)"* is not clear. It should be reformulated in a more specific way, avoiding statements like "*strong, but not strongest*".
- In figure 13 the line of MD threshold is missing. In figures from 6 to 17, the title of axis should be the same (e.g. "n (first event in window)"; the information in brackets can be eventually given in the caption).
- Results: line 426. The sequential number for M5.12 earthquake is missing.
- Testing stability of results with respect to minimum magnitude threshold (line 473). As in the abstract, I feel it should be explained the meaning of "higher representative threshold values". In fact, magnitude is not clearly mentioned in this paragraph, and the completeness magnitude (i.e. the representative threshold) has been assumed to be M2.6 in the section data description.
- Discussion (lines 523-525). The authors state that *"The period, for which such deviation from the random behavior can last, depends on the amount of seismic energy released by the strong earthquake*." Was this correlation analyzed formally? Did the authors check whether the duration of periods of non-random behavior actually correlates with the seismic energy released by the strong earthquakes? It would be interesting to see how such durations compare with the time windows widely considered for aftershocks identification.