Submitted on 19 Apr 2018
Anonymous Referee #1

First of all let me again express my sincere gratitude to reviewer 1, for fair and highly professional reviewing of our manuscript. With no regard of final decision, I appreciate importance of most of his/her remarks in the improving of our manuscript.

Below are answers to the reviewer's remarks.

Reviewer 1. Even though the authors took large effort to respond to all comments, they could not dispel my major concern about the appropriateness of the introduced IDT measure to describe the degree of randomness. The complete paper is based on the assumption that for random sequences sum_[i=1..N] DT(i) → 0, for large N. This needs to be proofed.
Referring to a paper in preparation or stating " logically, for any random sequence, the sum of the deviation times should approach zero, when " (revised version, page 2, last paragraph) is not sufficient. The sentence needs mathematical background. I assume the authors have something like the law of large number in mind, but this is not valid in their application, since it requires independent identical distributed data. Yet, DT(i) is not independent from DT(i-1).

I respect opinion of reviewer 1, but as it was pointed many times we base present analysis on the strong empirical argumentation of used approach. This do not exclude possibility that we come back to theoretical proofs in future but here we believe that our work should to be evaluated as it is, i.e. from the point of view of correctness of presented empirical results. Again, in this work we aimed to present idea how interesting dynamical features of complex data sets can be discerned through the simple method, effectiveness of which is clearly demonstrated empirically for both modeled as well as real data sets from original seismic catalogue.

Reviewer 1. Also the presentation of a matching example (as the analysis of colored noise) is not sufficient to proof the concept. In fact, Fig. 5a) contradicts the authors assumption, since IDTs for shorter time series (e.g. N = 5000) are smaller than IDTs for longer time series (N=34020) and consequently do not converge to zero for large N.

We agree with reviewer that former version of Fig.5 is better to be corrected. Apparently, it is better to show results normed not only to the span of window (i.e. sum of data in window) but also to the number of data in each window. In this case it is easier to understand that IDT for longer windows goes closer to zero. Thus, results in present Fig.5 do not contradict to our working assumption.

Reviewer 1. Moreover, a deterministic time-series with equidistant time steps contradicts the authors assumption that the IDT for more random-like time series is closer to zero, than for deterministic time series (even though a equidistant time series is not in the interest of the study). I could think of several other deterministic time series, where this is the case.

We have already explained that comparison with equidistantly distributed markers has no sense in our analysis. Thus, we clearly stated that IDT analysis in its present form should be used for non equidistantly distributed marker sequences. The question why we need to avoid equidistant marker sequences is not easy to answer and is related with the more general problem of what really random process is and what is relation between randomness and order. In other words it is *equal if you pose the question – could we regard order as a limit of randomness or not? I can not remember sources which directly answer such fundamental questions. Moreover I do not think that this article about the simple empirical analysis really necessitate to be expanded to such fundamental discussions. At the same time, I agree that problem is of immense general importance and hope will be able come back to this question later. As for reviewers remark on deterministic time series, I think here we face misunderstanding. In general, each time series of any kind is equidistant by the definition (otherwise it would be unevenly sampled sequence). From this point of view equidistant sequence, what we mean in manuscript, is rather time series consisted of equal values. At the same time is not clear why deterministic processes should produce data sets of this kind? From thermodynamics point of view this will be the condition of thermodynamic equilibrium for which the term "deterministic" is questionable, at least in the present connotation of deterministic processes.*

Reviewer 1. revised paper, page 7: "From this figure [fig. 6] we see that IDT values goes closer to zero when the extent of order decreases. Besides, it also becomes clear that even for short data sets IDT calculation is useful to detect differences in considered data sets."

Figure 6 only shows, that the distributions for low orders are more narrow than for higher orders. Yet, the mean seems to be the same for all distributions and differs from zero (~0.8?). Since sequences of high order magnitude also have good chances to be close to zero, the usefulness of IDT to detect differences is questionable. Several changes will be undetected.

On top, neither Figure 6 nor any other analyses shows, how IDT reacts to changes in the regularity of the time series. For all synthetic data sets the spectral exponent stays constant.

Here we, again, apparently face misunderstanding of the meaning of curves in Fig.6. In manuscript we speak that "IDT values goes closer to zero when the extent of order decreases". Indeed, for the case β*=0.001(triangles) corresponding curve crosses ordinate axis at about 45 % while for*β=1.655*(cross signs) at about 15%. How this fact can be interpreted otherwise if not the indication that at lower extent of order (*β*=0.001) larger part of IDT values is closer to zero (at least three times) comparing to more regular sequences (other than* β*=0.001)? This is why, at the same length of windows, distribution becomes narrower for less ordered sequence. It should not be forgotten that we deal with finite length (even very short) data sets (windows) for which closeness to zero, of IDT values, can be regarded just statistically. I can not agree with reviewer and would like to underline again that results in Figures 5 and 6 exactly show that changes in the extent of regularity of used synthetic data sets, lead to decrease IDTs closer to zero value. Spectral exponents, beta values, are already shown in figures.*

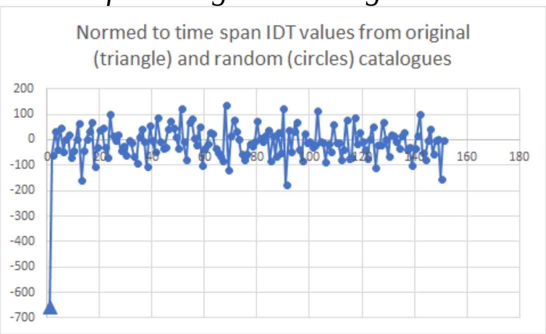
Reviewer 1. Revised paper, page 9: "Increasing the threshold to M3.6, M4.6, and M5.6 leads to following IDT values: 71.7, 6.7, -0.87 accordingly. Two important things can be underlined here: first, the increase of the magnitude threshold makes the time distribution of remained EQs more random and second, according to our conjecture to the more random EQ distribution should corresponds the closer to zero IDT value, what indeed is shown above. "

The IDT values depend on the number of events in the time series. Consequently the IDTs for the different threshold magnitudes are not comparable and can not be used to support the given conclusions.

I am very grateful to reviewer 1, for this very important remark and completely agree that "IDTs for the different threshold magnitudes are not comparable". I regret that we normed IDT values only to the time span, while it was necessary to norm also to the number of events (in that cases when number of events in windows have been different). Now we add norming to number of data *and see that stronger events do not differ too much from smaller ones by their time distribution features.*

Reviewer 1. Revised paper, page 10: “Thus, comparing the average of integral deviation times, calculated for the entire length of randomized catalogues, with the IDT value of the original SC catalogue, we see that the last one is two orders of magnitude larger.”
Comparing the average IDT of randomized catalogues to the original catalogue is not meaningful comparison, since the average value can be expected to be closer to zero in any case. It would be reasonable to instead show the IDT of the original catalogue in Fig 7, which gives the distribution of IDTs of randomized time series.

Here I can not agree with reviewer 1, though can not exclude that he misunderstood what was done with randomized catalogues. So let me once again explain that for testing purpose we compiled randomized catalogues in which the time occurrences of earthquakes from original catalogue, were randomly shuffled. For each of such catalogs we calculated IDT values and normed to the time span (norming to the number of EQs was not necessary because was the same as in the original catalogue). Than we compared all these 150 IDT values (-7.2 ± 57.5) with original one (-659.15). Z score =11.2, corresponding to p=0.001 convince that each IDT from randomized catalogues is significantly smaller than for original. Figure below will additionally help to understand situation that IDT values from each of random catalogues is essentially smaller than ID from original catalogue and thus effects of averaging cannot play any role.



Reviewer 1. Revised paper, page 11: “the strongest earthquakes never occurred in periods, when IDT curve comes close to zero value or crosses abscissa line” …
This is not very surprising, since only 6 of the 3500 time windows correspond to the large magnitude earthquakes. It is quite unlikely to hit one those time windows.

Can not agree with Reviewer's statement. In response I could say that the probability that certain earthquake will hit the certain time window is unlikely, exactly in the same way. Yes in Fig. 8b, we are focused on 6 windows (with main earthquakes far from zero crossings) out of 3500, but it is noticeably that all 6 windows are located at the rising branches of IDT curve (this is indeed unlikely to be happened by chance) and usually are very far from zero crossings (even first strong event M6.4) occurred after 100 events from crossing. All these, i.e. deviations from zero crossings and location on rising branches can not be explained as something caused by blind chance, though definitely needs further deep analysis, what we plan to do in nearest future.

Reviewer 1. Revised paper, page 12: “As we see in Fig. 9, analysis carried out on shorter catalogues, confirm the result obtained for the entire period of observation (1975-2017) and convinces that the curve of IDT values crosses abscissa at periods of relatively decreased seismic energy release.”.
Still it is unlikely to hit one of the high magnitude EQ windows. Fig. 9 also shows that the crossing of the abscissa strongly depends on the starting point of the studied period (e.g. the points for 1980 differ strongly compared to 1985). Consequently, I doubt there is a meaningful interpretation of the crossing points.

In order discussion to look more scientific let we speak about testable facts but not about what is likely or not. Exactly, our analysis clearly shows that windows with high magnitude EQs almost never coincides with windows with closest to zero IDT values. In present version we changed figures 9, 10 and 11 and added tables where are indicated windows with strong earthquakes and windows when IDT is closer to zero. From table 1 we see that for threshold value M2.6 no strong earthquakes (M6.4 -M7.3) occurred in windows with IDT closest to zero. In tables 2 and 3, for M3.6 and M4.6 representative thresholds we generally observe the same. Only in one case we found that strong earthquakes occurred in windows with closest to zero IDT values. This may be somehow is caused by specificity of seismic process in considered time periods and apparently do not in principle contradict the supposition that strong earthquakes rarely occurred in periods when calculated IDT value is closer to zero.

Reviewer 1. In general, it is difficult to compare the IDT values you provide, since some are not normalized, some are normalized to the span of window (e.g. Fig. 5), some to number of events (e.g. Fig 9). A uniform presentation should be used. E.g. it is difficult to compare the IDT of the original SC catalogue (not normalized) with the distribution of IDTs for randomized catalogues (normalized to span of window).

In present version in all cases we show results normalized to number of events and span of windows. Exclusion are figures 12 and 13 where we compare results in similar 100 data length windows so here normalization to the number of events was not necessary.

Reviewer 1. The results of Fig. 5b do not match with Fig. 6. The mean values for high orders are larger in Fig 5b compared to the means in Fig. 6.

This is because in Fig.6 we present PDFs, not frequencies.

Reviewer 1. I still don’t understand the authors' concept of randomized time series, since a shuffling of occurrence times will not change anything. Maybe the authors mean a shuffling of waiting times.

As it was already explained we consider randomized catalogue in which time locations of earthquakes from the original catalogue have been randomly changed.

Submitted on 31 Jan 2018
Anonymous Referee #2

In opposite to remarkable work of reviewer 1, I am deeply disappointed by human and scientific irresponsibility of reviewer 2.

Indeed, we have answered all his/her remarks in revision 1, and seems he/she has no further arguments against.

In spite of this fact, reviewer 2 recommends rejection without any arguments. In the same way, not providing any arguments, I also can state that reviewer2 is incompetent in the subject of our manuscript and do not deserves to be reviewer of respected NPG.