Dear Editor,

We thank the reviewer for his useful comments and annotations regarding the manuscript, they have been considered in this revised version.

"This is an interesting paper and I certainly recommend publication. It is speculative and I am not sure that I completely agree with all of their methods or conclusions - but that is not important in a paper like this. The ideas are well presented, the data are explained, the methods are clearly described and the rational for their conclusions are clearly stated. In searches for causative relationships between different phenomena - - in this case water level changes and earthquakes - - a single case may not be convincing, but multiple cases can build confidence in the significance (or lack thereof) of the relationship. Good documentation of case histories is important. Even when individual cases may be questionable, publication and distribution of well-presented documentation is important. Otherwise, how can common features be identified?"

Some specific comments:

Comment 1:

"It is not clear how much area is covered by the earthquakes considered in this study. Of course, since they are based on felt reports, the exact epicentral locations are difficult to determine. However, it would seem that the events considered could extend a considerable distance (100 km?) from the Dead Sea itself. It should be noted that this would be significantly farther from the lake than experienced in other cases of triggered earthquakes."

Response:

In the present study we specifically focus on earthquakes that were reported to have toppled houses at or very near Jerusalem. The distance from the historical city to the main Jericho fault is about 30 km. The "felt" level of local intensity corresponds to $2 \leq I \leq 6$, whereas fallen houses correspond to $I \geq 7$. As shown for reservoir-induced seismicity, water level changes can generate earthquakes over very long distances from the reservoir (e.g. up to 40km Durá-Gómez and Talwani, 2010). It is explained by the diffusion along the faults. In our analysis we associate all the historical earthquakes presented (Table 1A,2A in appendix chapter) with rupture of the strike-slip faults, which agree with our modeling approach. Hence, the major strike-slip faults (Lower Jordan fault, Dead Sea Lake fault and Northern Arava fault) constituting the plate boundary could be affected by Dead Sea water level changes. Therefore, our study covers the area within

this distance. The corresponding text was included in the final version, please see lines 46-47 on pp.3 and lines 92-96 on pp.5.

Comment 2:

It would be very useful to provide a figure showing the time history of the induced stresses, tectonic stresses and failure criteria for the synthetic earthquake catalog developed along with the water level data. This would make it easier to understand the process used to develop the link between water level and seismicity and also provide a better understanding of the relative magnitudes of the stresses involved. Without this information, I find it difficult to assess the significance of the lake induced stresses relative to the naturally occurring stresses and failure criteria.

Response:

Please see the required figures and the corresponding explanation in the revised version of the manuscript (Figure 3, line 219 on pp.14, and corresponding text lines 212-218 on pp.13).

Comment 3:

My oversimplification of the results of this study is that three episodes have been identified in the water level and seismicity rates - - one from 0-600 years CE with high water level and shorter recurrence intervals; the second from 600 – 1200 years CE with low water level and longer recurrence intervals; the third from 1200 – 1900 years CE with a return to higher water level and shorter recurrence intervals. In this regard, the authors should make note of Figure 5 in Ambrayses, 1971 (Nature v 232 pp 375-379, "Value of Historical Records of Earthquakes") which shows a similar cycle in the rate of seismicity. Although the Ambrayses paper is a comparison of seismicity rates between the Anatolian fault zone and the "Border Zone" (northern extension of the Dead Sea Zone), he does make the following tantalizing statement:

"A similar cyclic pattern, but with longer periods of overlapping activity, was noticed for the Border Zone and the Dead Sea System. At this stage, however, a more detailed study of the interaction and correlation of activity of contiguous units is not warranted." These long-term changes in seismicity rates, without a link to induced stresses, should be noted as a counter to the mechanism proposed in this paper.

Response:

Ambraseys' paper from 1971 has guided our research for the last two decades: In Migowski et al., 2004 (cited in the manuscript), we explored (Fig. 8) Ambraseys' statement for the Dead Sea Fault. In Agnon et al., 2006 (Geol. Soc. Am. Special Paper 401, 195-214, "Intraclast breccias in laminated sequences

reviewed: Recorders of paleo-earthquakes"), we refined the picture (Fig. 13). In Agnon, 2014 (cited in the manuscript) we show (Fig. 8.17a) that the transition noted by the reviewer for 600 CE is not warranted for the entire Dead Sea fault. Yet the reviewer is correct: a transition appears in our data filtered for the Dead Sea Basin per se at 600 CE. As for the second transition, our filtered dataset indicates 1100 CE.

We have been exploring coupling across plate boundaries for some time, see e.g. Braun et al., 2011 (Israel J. Earth Sci.; 58: 257–273, "Dating speleoseismites near the Dead Sea Transform and the Carmel Fault: Clues to coupling of a plate boundary and its branch"). We find that the millennial-scale cycles, modulated by large prehistoric earthquakes, contiguous strands and branches seem to be coupled.

We are inclined to think that the coupled systems of contiguous plate boundaries are modulated by the Dead Sea level fluctuations. Why would the 100 km long Dead Sea basin affect the entire plate boundary? Likely because this unique basin is the only one where such fluctuations are permitted by the hydrogeology. However, please keep in mind that our "hard" dataset comprises only 16 points, so the results are tentative and sensitive. This limitation brought us to use a random-number generator for a kind of bootstrapping in order to test correlation between lake levels and recurrence intervals. Such an exercise for testing the correlation with the Anatolian Faults is beyond the scope of the paper. or, in Anbraseys' own words: "At this stage, however, a more detailed study of the interaction and correlation of activity of contiguous units is not warranted."