Review of revised version of Hintersberger et al. 2017 "**Implications from palaeoseismological investigations at the Markgrafneusiedl Fault (Vienna Basin, Austria) for seismic hazard assessment**"

I read the three reviews and subsequent major revision of the manuscript, including the response to the reviewers and the revised paper and supplement. I think that the authors have addressed the issues raised by the three referees in a comprehensive manner and documented these changes accordingly.

Below, I will briefly comment only on the main arguments and follow up with a few minor suggestions to be addressed before publication. To do so, I summarize the reviewer's statements in black and my evaluation of their treatment by the authors in blue font color.

Three referees who are all experts in paleoseismology and familiar with trenching, earthquake histories and associated geochronology have seen the original paper. They all agree that this is an important and appropriate study to be published in *Natural Hazards and Earth System Sciences*, that it provides new paleoseismic results and will be an important contribution to the neotectonic and seismic hazard communities, including emergency managers.
To push the article to it's full potential, all reviewers basically agree that the following issues need to be addressed:

Data & Discussion

(1) The event history and the event correlation between the sites, which is based on limited age control of bracketing units, needs some reworking to clearly outline the events by their confining units and to provide a common framework for their timing including uncertainties. To do so, two of the reviewers suggest using a probabilistic analysis with OxCal - that is good practice in many paleoseismic studies worldwide.

The authors have adapted their model using the suggested OxCal model. The modeled results are comparable to the previous "hands-on" calculation, which seems to me a good indication of a robust interpretation. The new approach and results are incorporated in the associated tables, method section, and figure and the code is added to the supplement. → Point seems fully addressed in this new version

(2) The anticipated fault behavior needs some redefinition or clarification. While the authors aim to differentiate between potential *characteristic* and *super-cycle* earthquake occurrence, the discussion and review revealed that *periodic vs. clustered* scenarios would be better end member scenarios to evaluate. Two of the referees additionally suggest to use to the coefficient of variation to statistically discriminate between the two.

The new manuscript has clarified terms and used only quasi-periodic vs. clustered behavior. → Point seems fully addressed in this new version

(3) All reviewers wish to see a discussion (or data) on a potential linkage between the MF and the more active VBTF with the effect of such a scenario on earthquake timing and maximum magnitude for the Vienna basin.

Although only briefly, this possibility is now discussed and the consequences of potentially larger magnitudes for combined ruptures mentioned. → Point is briefly addressed in this new version; authors give good arguments in the rebuttal letter that the data so far don't allow for a more detailed discussion of this issue.

(4) Reviewers would like to see an improved discussion on the preservation potential of the earthquake evidence, which also includes alternative interpretation (or discussion) for origin and/or overprint of colluvial wedges. This is an important issue in low-strain areas, especially under influence of geomorphic consequences of glacial-interglacial cycles.

This issue got a new paragraph in the discussion chapter → fully addressed there and referred to also when discussing smaller ruptures in between clusters.

Presentation & Style

(1) The reviewers suggest that a methods section before the results will help to build a framework around the study without interrupting the text flow within the results by such information.

The new manuscript has a method chapter in a comprehensive and well-structured format → point fully addressed.

(2) A detailed site description (in figure) needs to supplement the trenching information to provide the reader with the geomorphic understanding of the area.

The new manuscript has a new figure (Figure 4) to account for these issues. Here, however, I feel that the points are not fully taken, see also my additional comments below. → needs revision

(3) The original (uninterpreted) photomosaics should be provided (at least as supplement).

→ Point fully addressed by incorporating the photomosaics into the supplemment

(4) The reviewers suggest a relabeling/restructuring of events to avoid misunderstandings about the number/sequence of earthquakes at or between the sites.

The event-labeling was changed accordingly. → Point fully addressed.

Additional comments

(1) Response to RC4 (Ice loading). While this may not necessarily need to find its way into the paper, I would like to emphasize that there is some discussion in the literature suggesting significant rebound of the Alps after LGM deglaciation (e.g., Norton and Hampel, 2010; Mey et al., 2016). Without knowing the exact location of the Alpine extent in Austria, the wavelength of isostatic compensation might be larger and includes the previous forebulge area, where vertical adjustment might have a different effect on normal faults than known for reactivated thrust faults that have been located below the Ice. In Switzerland, indications for extended glaciations also exist for MIS 6, so a time period just pre-dating the time interval of the earthquakes dated in this study.

(1) Please add references for the statement in the first paragraph of the introduction (i.e., the Sumatra, Tohoku, Haiti and Christchurch EQ-cases).

(2) Same paragraph, pg. 2, lines 5-8: There is much doubling, I suggest merging these sentences.

(3) It might be worth citing the introductory paper of the book *Landgraf, A., Kuebler, S., Hintersberger, E. & Stein, S. (eds) Seismicity, Fault Rupture and Earthquake Hazards in Slowly Deforming Regions. Geological Society, London, Special Publications, 432, http://doi.org/10.1144/SP432.13* that addresses many of the mentioned issues (E. Hintersberger is one of the editors).

(4) Section 3.3: Did you follow the approaches of DuRoss et al., or Personius et al., as suggested by the reviewers? They might be cited here.

(5) Figure 4: I'm not sure, if this selection of field photos provided here adds much to the understanding (i.e., 4 C, D, F). 4 E is good, but could benefit from some annotation (e.g., fault scarp) – same for B. Also, perhaps a hillshade, instead of the DEM (in 4 A, but perhaps could be even considered for Fig. 3)) would bring out the geomorphology better. As a geomorphic map, it would also benefit from some annotation (perhaps having (A) as hillshade and (B) next to it as interpreted geomorphic map within the same extent (see Meriaux et al., 2014/2015 for nice examples of geomorphic mapping). Focus might be the fault scarp(s), but also incision. As the reviewer was asking for topographic profiles, such complementary profiles, derived from the DEM next to the trenches and annotated to highlight the trench dimensions and vertical separation at each site, would help the reader to better acknowledge the geomorphology of the sites. Additional comments on Fig. 4: Please provide orientation at field photos; consider moving the scale bar in A to the bottom so you can leave the coordinates in place. Indicate position and viewing direction of field photos on the overview map.

(6) "collapsed free-face" vs. "colluvial wedge": I understand the difference from the description, but find this terminology confusing. An earthquake-driven colluvial wedge also at least partly derives from collapse of the free face (until the angle of repose is established and the diffusive

processes take over). Maybe, you can call it “nontectonic wedge” or similar?

(7) Page 17, Line 1: Typo! Slip should be 1.8 m instead of 10.8 m – also, I did not quite follow the calculation. E1 –slip (based on B1 event) was measured as dip-slip between offset markers, so I guess, you assumed a vertical fault dip to translate the 10 cm of vertical slip for E1 that you added to the 0.8 m of B2 wedge height? Then, this amount (0.1 m for E1) should not be doubled for the second scenario, adding to 1.7 m only instead of 1.8? – Please clarify.

(8) In general, did you account for the listric shape and fault-dips of about 70° in places when calculating magnitudes from slip? I see that you consider vertical slip only, so the actual dip-slip might be slightly higher.

(9) Figures 1,3, and 4: give data source for the DEM