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Interactive comment on "A multi-centennial record of past floods and earthquakes in Valle d'Aosta, Mediterranean Italian Alps" *by* Bruno Wilhelm et al.

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We thank the reviewer for his comments. A point-by-point reply to these comments can be found below, as well as the marked-up manuscript version. Our response to the review comments is marked in yellow. In addition, we have indicated all changes in the annotated version of the revised manuscript in yellow.

Response to the main comment:

I have a broader concern around the scope and impact of the paper. The authors and their collaborators have published a series of papers on this theme from various lakes in the European Alps over the last few years. If the authors are pitching it as a

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further case study, that's OK, and it meets the criteria of NHESS by presenting new data. On the other hand, the authors state on line 316 that Lago Inferiore has the "highest Earthquake Sensitivity Threshold Index of any studied Alpine lake". This is a much stronger statement than is made in the abstract. I urge the authors to consider re-framing the paper so they sell its novel aspects.

This aspect is already clearly stated in the abstract (l.20-22). We just avoid technical term (ESTI) in the abstract to make the statement clearer to a broad audience: "Compared to other lake-sediment sequences, Lago Inferiore de Laures sediments appear to be regionally the most sensitive to earthquake shaking, offering a great potential to reconstruct the past regional seismicity further back in time."

Important interpretational queries (i) The sediment accumulation rate is surprisingly high if the majority of the catchment is inactive, owing to the high-elevation lakes, and it is frozen for half the year. This will leave an active catchment in the order of 1-2 km2. I suggest you elaborate further on the sources of sediment, especially how much may be glacially-derived material. Will this not have a very different sedimentological signal to the floods and mass movements?

Sedimentation rate in INF is as high as for many other high-elevation lakes of the Alps (see Fig. 8B). In addition, we explain in the section 2 (l. 84-86) that the glacial material is mostly trapped by the two lakes located upstream. Only a rock-glacier is present in the 'real' catchment of the lake but there is no fine sediments that might be transported by the temporary stream and deposited in the lake. Finally, deposits triggered by floods or mass movements are all mainly made of detrital materials coming from the catchment that is uniformly made of eclogitic micaschist. That's why there is here no clear sedimentological or geochemical differences between them.

(ii) It is unclear how you associate the lamination thickness with the grain size measurements sampled continuously at 5-mm intervals. In the Passega-type diagrams, what did you do if a maximum D50 or D80 value was derived from a 5-mm slice that

overlapped into another distinguishable lamination?

For the great majority of layers, there is no problem of overlap (i.e. we generally got 1 measure of grain size per layer). For the very few thin layers that may be sampled together, the same grain-size parameters are assigned as we are not able to reduce the sampling step below 5mm.

(iii) The suggestion that the 137Cs spike associated with AD 1963 weapons testing has been diluted by the Chernobyl signal seems unlikely, considering the rate of sediment accumulation. AD 1963 should occur within a band at 12-18 cm (1σ), which corresponds with GB-IIIa. Is it more likely that this mass movement may have redeposited older material and diluted the atmospheric 137Cs signal?

As the hypothesis of the dilution effect is indeed uncertain and not necessary, we have removed this mention. The hypothesis of the reviewer on the role of the mass movement seems also unlikely because the Cs signal below the mass-movement deposit is null, whilst in this case we should expect low values (not null) corresponding to the beginning of the atmospheric nuclear weapon test in the 1950's.

(iv) The extrapolation of the age-depth model is a concern, although I appreciate this cannot be easily resolved without substantial effort e.g. acquiring radiocarbon ages. I presume the authors looked for earlier metal signals reflecting earlier industrial emissions and/or mining/smelting? Further, sediment density is higher below 20-cm. Could this point to greater input of clastic material? I suggest the authors make a convincing case that sediment accumulation rates are likely to have remained constant through this time window. In particular, would the SAR have remained constant as the glacier(s) in the catchment retreated after the mid-19th century maximum (assuming it followed the regional pattern)?

Because of the uncertainties of radiocarbon ages (around a century or even more), this would not improve this chronology. In addition, there is no older Pb (or metal) contamination. The increase of density is mainly related to the compaction effect that

triggers a lower porosity and then a lower water content (classic in recent sediments). As indicated above, the glacier fluctuations are not expected to significantly influence the sedimentation rate (SR) because of the two lakes upstream that act as sediment traps.

(v) The sedimentological evidence of mass movements is convincing but can anything be inferred from the different depositional characteristics of the four mass movement layers? Is the likelihood for one depositional mechanism to occur sensitive to earthquake intensity or distance from epicentre, for example? Or does the lake and/or catchment evolution influence which type of mass movement deposit occurs in response to an earthquake? I've seen little on this in the literature and it would be an interesting point to try and make.

Indeed, this issue is an exciting research perspective in palaeoseismology. We have previously explored this approach based on a comprehensive review of earthquake-induced mass movements in similar high-elevation alpine lakes (Wilhelm et al., 2016, JGR). However, this appeared unsuccessful for those lakes.

(vi) The role of glacial input and/or snow avalanches has not been considered fully. The former could make a significant contribution to the basal sediments because the active catchment from the eastern stream is so small. There is potential for snow avalanches to deliver a characteristic deposit – see some of the work by Eivind Støren and colleagues. This could a factor in the discussion on lines 332-334. Are there any records of avalanches in those years or local meteorological data that suggest particularly warm springs, which could have triggered widespread snowmelt? This notion of snowmelt applies more broadly, as the lake is frozen for 6 months of the year. Do the historical data (as referred to on line 372, presumably derived from Mercalli et al. 2003) suggest any regional floods triggered by snowmelt?

About the glacial inputs, see comments above. About the avalanche and the work of C. Vasskog et al. (colleague of E. Støren), this is an issue we know well. Avalanches

generally deliver unsorted debris (e.g. coarse sand and gravels mixed with fine sediments) that form particular deposits that we do not observe here. In some cases, this may exceptionally trigger mass movements. However, this requires an avalanche strong enough to break the ice cover and then disturb the slope sediments (e.g. Wilhelm et al. 2013). From the review of mass movements recorded in many alpine lakes, we observed that the earthquake is a much more probable trigger than such a strong avalanche (Wilhelm et al., 2016, JGR). In addition, we do not have any information related to past avalanches in the catchment, or in the region, to support even more this aspect. Mercalli et al. (2003) do not describe the hydro-meteorological conditions that induced floods.

(vii) On Figure 8 there appears to be two earthquakes that plot above the sensitivity threshold. In terms of fully understanding the process sedimentology, I suggest the authors offer some explanation as to why those earthquakes did not leave a preservable imprint.

Actually there is only 1 earthquake that does not let a visible imprint (AD 1905, see also Fig. 7 where this event is highlighted with a question mark). We do not have any plausible explanation of this phenomenon. That's why we are not able to further discuss this aspect.

(viii) I am unconvinced by the argument that grazing facilitated thicker recent event deposits. Did grazing in the catchment really only begin in the 1990s? It would be helpful for the authors to provide evidence.

The relation between grazing and erosion, and the potential influence on the flood record, is not an argument but a fact as shown for instance by Giguet-Covex et al. (2011). You may also see Giguet-Covex et al., 2014, Nature Communications or Brisset et al., 2017, Geology. At this stage, we cannot further elaborate on this aspect in INF because of the lack of data. That's why we conclude that "further work is still required to confirm this hypothesis" (l. 411) and propose a research way to approach this issue:

“studying proxy of grazing activity like coprophilous fungal ascospores” (l. 412).

Figure 1: (i) The colour scheme associated with the DEM ought to be incorporated; (ii) The purpose of panel C is unclear. The lake appears disconnected from the major regional tributaries

The purpose of panel C is to highlight the streams affected by the historical floods documented by Mercalli et al. (2003) that we used in our comparison with the sedimentary record. We add a few words in the caption to highlight this aspect: “the hydrological network of Vallee d’Aosta that is regularly affected by floods as documented by Mercalli et al. (2003). (l. 73-74). We added the connection between the lake outlet and this hydrological network.

Figure 3: (i) Could the horizontal layer stripes be shaded to reflect the different processes? (ii) Explain in the caption what the layer codes represent, or at least point the reader to the relevant section; (iii) The matrix-supported layer is very difficult to distinguish. Could you use a different colour scheme or patterning?

We modified the horizontal grey bars as suggested and we added the meaning of this code in the caption as well as a reference to the sections in the text. The matrix-supported layer is already clearly highlighted with its label (MSB) in Fig. 3 and a zoom on this layer is also presented in Fig. 4.

Figure 4: (i) Change ‘sedimentary’ to ‘sediment’ on the y-axis The modification has been done.

Figure 7: Spell out what “Io” and “d” are in the caption This has been added in the caption.

Figure 9: (i) Spell out “INF” and “LED” or else include these codes in the caption; (ii) What does the horizontal red line represent?

INF and LED are now explained in the caption as well as the meaning of the red rectangle. (it highlights the period dated by the 210Pb method)

Minor comments Lines 34-35: the phrase on ‘robust risk assessments’ is rather vague We slightly modified the sentence to clarify it.

Line 36: Should include a reference We added the reference of the IPCC (2013)

Line 38: “have been” instead of “were” This has been modified.

Line 46: “In the case of earthquakes. . .” This has been modified.

Line 49: “centuries” This has been modified.

Line 61: remove “it” This has been modified.

Line 120: replace “during” with “from” We did not find this reviewer’s proposition appropriate.

Line 140: remove “the” This has been modified.

Line 150: “. . .deposits, representing. . .” This has been modified.

Line 150: come up with a better technical word than “interrupted” We kept this word as it well highlights the sudden occurrence these deposits in term of sedimentological features and processes. In addition, this word is often used in the literature.

Line 197: add “down-core” or similar at the end of the sentence This has been added.

Line 207: “cannot be as clear defined.” This has been modified.

Global change: the word “decennial” is odd. I suggest a global change to “decadal” This has been modified all along the manuscript.

Line 379 and section 5.2.3: I suggest the authors insert additional references to Mediterranean climate in this section (some of which are listed in the bibliography and referenced elsewhere) References have been added as suggested.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2016-364/nhess-2016-364-

AC3-supplement.zip

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