

Interactive comment on “Orbital changes, variation in solar activity and increased anthropogenic activities: controls on the Holocene flood frequency in the Lake Ledro area, Northern Italy” by B. Vannière et al.

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The study presented in the paper “Orbital changes, variation in solar activity and increased anthropogenic activities: controls on the Holocene flood frequency in the Lake Ledro area, Northern Italy” provides a reconstruction of intensity and frequency of flood layers based on a laminated lacustrine record from Northern Italy. This reconstruction is mostly based on sedimentological proxies, including colour, physical properties and geochemical composition of the sediments, but some analyses on the organic components (organic matter characterization and pollen data) help to interpret the lacustrine

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record. In addition, counting annual laminae in the sediment, together with ^{14}C and ^{137}Cs dates, allow constructing a reliable chronology for the Holocene.

Overall, this is a very interesting paper that merits publication in *Climate of the Past*. However, there are a number of issues that need to be addressed before publication. So, I recommend major changes on the manuscript. I detail below the main reasons that support this statement and also some suggestions that may help the authors to improve the manuscript.

1. Lack of detailed sedimentological description. In spite this paper can be considered a “sedimentological paper”, the description of the lacustrine sediments is very poor. This is the main limitation of the study of Lake Ledro sediments presented in this manuscript. Several issues should be improved:

a. First, to assert the annual (or seasonal) character of the lamination a monitoring study (installing sediment traps, for instance) or/and a petrographic characterization (SEM, microscope) of thin slides are mandatory. For example, it is said that white laminae correspond to the carbonate precipitation in the lake, that occurs mostly in summer. allochthonous origin for the carbonate is also frequent in a limestone-rich catchment area, as this one, so the presence of detrital carbonate must be ruled out before giving a biogenic interpretation to the white laminae. In addition, the sedimentation of the gray layer is attributed to winter season, but without any proven evidence. A sedimentological and geochemical characterization of pairs of laminae, in a similar way than presented in other papers from laminated sequences (Brauer et al., 2008; Corella et al., 2012; Martin-Puertas et al., 2012) is recommended.

b. Second, more explanations are necessary regarding the identification of flood layers. The method proposed here to identify flood layers is based on the color of the sediment. However, the authors state several times in the manuscript that geophysical and geochemical data make possible to distinguish events. Why don't they use those proxies (with more compositional meaning than the color) for the statistical analyses

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of frequencies? Or at least, to check the results obtained from the color analyses? In fact, XRF core scanner measurements were already obtained with a sampling of 0.2 mm, likely detailed enough to resolve the presence of laminae. Again, more detailed sedimentological description is necessary to see if there was erosion or not within flood layers and “background” laminations.

c. Third, the presence of slumps must be considered carefully. From the number and intensity of Cs peaks, it seems evident that there was a slump duplicating the sedimentary sequence in the uppermost part of the record. However, are there any other evidences to detect other similar slumps that may happen in other sections? How can the authors rule out the presence of other slumps?

2. Forcing mechanisms. In the manuscript the authors discuss climate mechanisms at different time scales and also consider human influences on the catchment to explain some changes in the flood frequency or intensity. Although this discussion is particularly profuse and correct, I add some ideas that the authors may want to include:

a. The main hydrological change in the record is observed after 4500-4000 cal BP when a sudden increase in flood frequency is observed; this change must be regarded as a change in the trend rather than a particular event associated to the 4200 yr event. This change is associated to a response to orbitally-driven insolation (see also (Magny et al., 2012)) while human influence is discarded (in spite of the presence of a Bronze settlement near the lake and evidences of deforestation from the pollen data). The main arguments put forward by the authors are (1) that there was a clear hydrological change marked by an increase in lake level and (2) since a similar change is observed in many other Southern Europe locations the common cause must be climatic. It is always difficult to tip the scale favouring climate or anthropic impact but in this case, there are enough data (from the sequence and in other nearby records) to discuss in depth this issue. Other changes in the catchment area (any variation in the tributaries??) should also be also discussed in relation to this large hydrological change. To me, more emphasis on this important and clear inflection point in the hydrological pattern is

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missing.

b. In the discussion of Early to Mid-Holocene events, some confusion is produced among “abrupt events” and “general trends” in the record. For example, the authors consider three events (three clustering of flood events) at 8000, 7500 and 7000 cal BP but they are compared to reconstructed summer or sea surface temperature trends. Those concepts can not be mixed.

c. The increase in floods during the LIA is linked to the persistent negative mode of the NAO, as reconstructed in Trouet et al. (2009). I suggest the authors to plot the Trouet’s data to clearly mark the similarities with the Lake Ledro flood frequency reconstruction. In addition, an explanation about the influence of NAO in Spring (or a reference) for the studied area would be desirable.

Minor comments and suggestions:

Although English is not my mother-tongue, I recognize some sentences that are not fully clear (although readable). I would suggest the authors to consider the revision of the manuscript by an English native speaker.

Abstract

- It is too long and with too much detail. There are some sentences similar to the ones written in the Methods section. As an example, lines 8-11 are not necessary.

- The last third of the Holocene. . . . change by Late Holocene or After 4500 cal BP. . .

- When talking about “centennial time scale” (line 21), reference to longer periods such as Neo-glacial or Little Ice Age should be avoided in relation to “events” (ca. 4500 and 500 cal BP). Authors sometime mix “events” with “longer periods” (see above).

Introduction

- Line 14, Page 4703: In contrast to this period. . . . Why in contrast? Not clear, please rewrite

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- Lines 7-10, Page 4704: Not always. Please, rewrite.

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Material and methods

- line 11-12, page 4705: a detailed description of the sediments is necessary in this manuscript, not only about the rim

- 2.4. Geochemical analyses:

o what about the other elements not presented here? Was the XRF signal too low, noisy, not significant? Please, explain

o Was it possible to sample for ICP-AES analyses at the scale of the lamination? The authors may need to explain why did not use "traditional" XRF instead of ICP-AES.

- The procedure followed to count laminae is not explained in methods.

Results

- Fig. 3: I don't understand the differences among "auto" and "naked-eye" differences in the construction of the age model. Please, improve figure caption with more clear explanation.

- line 15 in page 4713: the comparison among "auto" and "naked-eye" counting methods is not clear to me in Figure 5. Specially, for core LL08-2 differences in both methods seem pretty important.

Discussion

- Figure 6: consider to add marks for the four time periods considered in the text

- Line 26, page 4717. Should be read 20th century (not 19th).

- Line 26-28, page 4717 (last paragraph of 4.2 section). The most recent part of the record was not used for paleoenvironmental reconstruction due to the presence of a slump. Add this fact

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- Figure 8 is cited before Figure 7
- Lines 9-13, page 4720: incomplete sentence.

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