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Climate of the Past Discussions

Interactive comment on "Changes in high intensity precipitation on the Northern Apennines (Italy) as revealed by multidisciplinary data over the last 9000 years" *by* Stefano Segadelli et al.

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The manuscript by Segadelli et al. uses sedimentary properties such as grain size to reconstruct past flood events at Moo Lake in the Northern Apennines. The authors assume that erosional layers can only be triggered by high intensity precipitation events that have enough energy and duration to mobilize coarse minerogenic material. The Holocene chronology is based on eight 14C dates, the material is most likely of terrestrial origin but may suffer from in-built ages (e.g. wood). Somehow surprisingly an age-depth model is presented but not used to put the data on a time-linear scale, instead depths are preferred throughout the manuscript, which makes it difficult to compare these novel data with existing palaeoclimatic, palaeoecological and palaeoenvironmental records. Summary pollen data with very low temporal resolution are used to identify vegetational changes. The manuscript is well written and very interesting, in particular the authors use independent palaeoclimatic evidence to support their main inferences. The question addressed by the paper is highly relevant and within the scope of CP, novel data are presented in appealing figures and the conclusions are interesting. In most cases the own novel results and the already published data used support the interpretation of the authors, in particular in regard to the main message which is the inferred highest flood activity during the Mid Holocene Thermal Maximum (HTM). In some cases, however, the discussion and conclusions need more caution, in particular in regard to mismatches of temporal scales between the different time-series used. Some factors leading to increased erosion such as human impact and fire are neglected (see detailed remarks) and need attention.

Detailled remarks:

A) The abstract is concise and well written, but some mistakes should corrected ("small ice age" is probably Little Ice Age, LIA).

B) It is not just (Glur et al. 2013) that have observed increasing flood activity in and around the Alps during cold periods. A comprehensive review is provided by Henne et al. (2018) who emphasize the conflict between the available evidence and the hypothesis of the authors: warmer conditions do not always lead to more floods. Flood increases during the Little Ice have been historically recorded and were the consequence of increased precipitation during cold periods (see e.g. Wanner et al. 2000). It might well be that the Alps and the Apennines have different regional flood histories (at least in the northern Alps and the Central European lowlands) and this could be elaborated by the authors.

C) Still in the introduction pollen is declared as a palaeoclimate proxy (line 82). Fortunately, this is not the use the authors make of their (very coarse) pollen record. Actually, pollen is a palaeovegetation proxy that in some cases and under some conditions can be used to reconstruct climate. Since at latest ca. 5000 cal BP vegetation was strongly altered by humans and land use effects were gradually overruling climate as the main driver of plant community changes. For the Northern Apennine this is discussed in detail in e.g. Vescovi et al. (2010a) and Vescovi et al. (2010b) for Greppo and Pavullo. At the remote Greppo site in the mountains, pastoral and fire activities increased at ca. 6000 cal BP. At the less remote and warmer site Pavullo, arable farming started around 5000 cal BP. These activities were associated with deforestation (also recorded in the data of the authors, pollen layers P08, P10, P12 with high herb pollen), a process of paramount ecosystem relevance which can also increase erosion and flood activities. This issue needs more attention given that Glur et al. (2013) correctly emphasize that human impact may affect flood reconstructions deriving from single study sites. However, given the signal observed in the Moo Lake data with reconstructed flood activity peaking during the HTM (when human impact was low) and the post-LIA (when human impact was reduced since ca. 1950 AD), the authors can partly rule out such human effects, which I think is very interesting and should be elaborated in the text.

D) The chronology is likely sufficient to support the inferences and conclusions of the authors, however, the authors may state that they assume that all radiocarbon samples had no hard-water effects (e.g. in particular for peat, what is the material in the peat layers mainly? Cyperaceae?). Indeed, there are limestones and breccias in the catchment and dating of bulk sediment material under such conditions can lead to wrong reconstructions (see Finsinger et al. 2019). Similarly, do the authors exclude inbuilt ages or reworking effects for wood samples (e.g. were they small twigs, see discussions in Gavin 2001; Oswald et al. 2005)?

E) The July temperature reconstruction of Verdarolo is in agreement with that of Gemini and several other proxies, this may be stated at line 197 to increase the representativeness and validity of the Verdarolo record (see discussion in (Samartin et al. 2017).

F) In regard to palynology, did the authors really do all identifications at 1000 magnification? The standard is 400 x (to be efficient), only difficult pollen such as Triticum, Avena or Hordeum types is usually observed at 1000 x (line 216). What is in the pollen sum? Please state (e.g. terrestrial pollen, no aquatics, no spores).

G) The discussion starts with a figure description over 6 lines (332-337), this is not really exciting, is actually very descriptive (and may stay confined to the figure caption). Why not starting more dynamically with lines 345 (and following text), stating that high flood activity occurred during the HTM? This would make the text more appealing.

H) Model simulations confirm that the HTM was more humid during the summer. This outcome is mainly driven by (orbitally forced) summer insolation and the resulting mid latitude (i.e. Mediterranean and European) evaporation. It seems unlikely that enhanced tropical monsoon activity may have led to wetter climate as far north as the Northern Apennines by vapor transport or related phenomena deriving from the monsoons (see e.g. discussion in Tinner et al. 2009). On the contrary, the strong high-pressure field resulting from monsoonal activity north of the ITCZ at ca. 30°N was particularly active during the Early Holocene (causing very dry conditions over the Mediterranean), to become weaker during the Mid Holocene including the HTM, which likely allowed westerlies to reach the Mediterranean, thus creating more humid conditions. Please discuss and refine, Skinner and Poulsen 2016 is about tropical air masses and related humidity changes, not mid-latitudes.

I) Lines 361-364: which Alpine lakes do the authors mean? The new one by the authors? Are the authors referring to summer temperatures and more intense summer precipitation (the proxies are about that, see Samartin et al. (2017), please refine.

J) It is true that July temperature during the period 1961-2018 increased by about +4.3 °C in the data of the authors (lines 366-378, 11 yr running average). However, this is at an intermediate level between weather and climate. At climatic temporal scales > 30 years for July means, the increase might be more similar to +2° C (14 vs 16°C). Why is this important? The proxies involved have in the best case multidecadal resolutions, in the case of Samartin et al. (2017) ca. 80 years for the past 1000 years and ca. 250 years for the YD/Holocene transition. Caution is therefore needed. No way to reconstruct rates of climate change at decadal scales with such data, they were instead designed to study multi-centennial trends such as the HTM. Where resolution of multiproxy studies was high enough (reaching decadal scales) the early Holocene warming in proximity of the Northern Apennines was estimated to ca. 3-4°C within 48 years (Schwander et al. 2000; von Grafenstein et al. 2000). Please refine to avoid mismatches of temporal scales.

K) Discussion of pollen, what kind of conifers were involved? I assume Abies alba mainly during the HTM. This implies that Abies alba reached its peak during the warmest period of the Holocene, in sample P7 (which chronologically coincides with the HTM peak of Samartin et al. 2017). Thus the sentence at lines 423-424 should be "In the P7 sample we observe a further increase in conifers (75%, mainly Abies alba) and a consequent reduction in deciduous trees (10%) during warmest HTM conditions." This finding might be explained by the fact that Abies alba is a warm-temperate tree (not a boreal conifer) that prefers rather moist conditions (see discussion in Tinner et al. 2013), which is in full agreement with the interpretation of the authors.

L) Lines 436-439. The creation of openland and grassland was a consequence of land use, not climate. To reduce the forest cover to 30 %, semi-desert conditions would be needed, even in hot coastal Sicily arboreal pollen was at ca. 80% during the Late Holocene, when land use was low. The authors should cite relevant literature from the Northern Apennines (e.g. Vescovi et al. 2010a and Vescovi et al. 2010b) that permits attributing such changes to human impact through high taxonomic resolution (e.g. increase of Rumex acetosa and R. acetosella at Greppo as proxies of pastoral farming, of Plantago lanceolata and Cerealia at Pavullo as proxies of arable farming).

M) Lines 462-465 see remark J and please refine to consider mismatches of temporal scales of time-series involved.

N) Figure 7: A pity the new Moo record is not on a time scale. How will future research be able to compare this with new results on a depth scale? Figure 10: x-axis label for pollen is missing. Is this 100%?

All in all, despite all this detailed points to refine, an exciting and excellent contribution, I am looking forward to see it published in CP.

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