

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.

Nathalie Combouieu Nebout

nneboutcombourieu@mnhn.fr

Received and published: 3 March 2020

This paper aims to reconstruct the story of precipitation in Northern Apennines. This work intends to produce new data from an area that may be affected by floods during the Holocene. It combines sedimentological, pollen data and pedology data in comparison with well-known speleothem and chironomid July temperature records.

When reading this paper lot of questions and remarks arise and I have listed the main ones below.

- My first remarks focus on the figures. I do not understand exactly how they have been

C1

constructed.

In Figs 7 and 10, is the S1 core built in age or in depth? I understand it is in depth. The ages on the left, are in cal BP but they do not correspond to those indicated in the table TS2. We have an age range and if we go in the table of the supplementary data we have an age that does not correspond to the age of the correlation date that has been placed on the figures. It will be very informative if in the table TS2, authors add other columns especially with the raw data on ages (C14 BP ages) and the ranges of cal BP ages in front of their cal BP ages indicated in the table.

In both figures, if the S1 record is built in depth, how the authors can compare their record with the speleothem record in cal yr even if we have the ages on the left of the record pointed in depth with their age ranges. We see that we have in Fig 7 a dotted line with number on it, but what is the significance of the number? I understood it corresponds to one of the limits of age ranges marked on the left. But, on which basis did the authors chose that number for the correlation? The representation in depth did not take into account the sedimentation rate. Are the authors really sure of the position of the samples face to the well-constructed chronology of speleothem record? As a consequence, what is the validity of the comparison of the isolated pollen samples with the very detailed and high resolution speleothem record and with the high resolution July temperature record from chironomid?

- A second group of remarks is on the pollen samples and their interpretations. My feeling with the use of pollen data here is driven by the numerous studies that have been published before, especially in Climate of the Past, and which show high resolution data reconstructing very detailed records. Here we have only 11 samples for a record of about 6-7000 years in two phases. This statement does not in any way affect the reliability of the analyses that are without doubt very good. Nevertheless, it affects the interpretations that may be less reliable based on such a very small number of samples. You explain that you choose the samples “because they fall in two stratigraphic units of your interest”. Does it mean that you only have a look on samples in

C2

specific parts, omitting to see what happened elsewhere? It is a strange way to do the palynology in a Holocene core in which lot of vegetation changes can be described and have been described in lot of other cores. It has been better to analyze a continuum of samples along the core such in other pollen studies and see the changes that occurred in the parts of author's interests in the complete pollen diagram. For example, regarding the 4.2 kyr event, see the publications in CP special volume published last year. A decrease showed through one sample is not reliable in my opinion, further analyses around this sample will be crucial to confirm that the observed pollen assemblage is not linked to a taphonomic bias. In fact, the sample linked to 4.2 kyr event is placed in correspondence with a lithological change. Did the sample be collected in the sand where the taphonomy is generally of high level or in the peat layer? I understood that the sand layer is linked to the 4.2 kyr. So if the pollen data are from the peat layer, authors link what they found according to the sand with what they have in another layer that may have a different age. Perhaps I have not understood the interpretation in line 432-434. Explain more.

A question: in your record, what is the time resolution in the pollen record? About 300 yrs for the closest ones if I use the chronology on the left of the figure and if I try to evaluate according to the position of the samples. It is not sufficient for concluding about the links with short climate event occurring during the Holocene. Thus, the unique sample P10 (whose age we don't know exactly) with a decrease in forest is used for correlation with the 4.2 kyrs event and the samples P05 and P06 for the correlations with the 8.2 kyrs event while we have two dates very far and two samples (whose ages we don't know again). It is not realistic in my opinion.

No information is done on the composition of the plant groups proposed in fig. 10. Here we are in mountains and it is bad to gather Pine with other conifers as their behavior is different from those of Abies or Picea. What is the composition of the Hydro+hydro+helio group and of the Pasture-Meadow group? Does this last correspond to herbaceous open vegetation? Do you think that you may have indication of pasture

C3

in your record with 11 samples? Probably it will be better to return to a wider naming (perhaps only meadows?) and remove pasture.

Minor remarks: What is HCO in fig.7 it has not been defined

To conclude I think that this paper is very interesting. Nevertheless, the pollen data have to be at least considered with more caution and probably removed from this paper in the proposed format as, in my opinion, they are not able to endorse reliable conclusions due to their low resolution and highly unprecise ages.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-135>, 2019.

C4