Climate of the Past Discussions

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Comment on cp-2020-163

Anonymous Referee #2

Referee comment on "Co-evolution of the terrestrial and aquatic ecosystem in the Holocene Baltic Sea" by Gabriella M. Weiss et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2020-163-RC2, 2021

The article by Weiss et al. is well written as well as well illustrated and the record is based on a core from a particularly interesting region of the Baltic Sea for which new biogeochemical data is to be appreciated. I am not an expert on this kind of data myself, but think that this publication very well and precisely documents the used approaches, and I think that the results and the discussion are worth being published to broaden the view on the Holocene in the southern Baltic Sea, even though the relatively low temporal resolution of the record makes detailed comparison with other records difficult (see below). I also think that the age model could be explained in more detail since one has not only to visit the supplementary material of Weiss et al. 2020 but ideally also Warden et al. 2016 to get a precise idea how it was generated. Another aspect I see a little sceptical are the results concerning the terrestrial ecosystem structure since I think that with the used proxies, it can only be well estimated concerning a few certain plant types (such as *Sphagnum* indicated by the C_{23} n-alkane). In case of several statements concerning the vegetation/climate development, I think the literature cited for comparison is only partly suited to support the results based on the biogeochemical proxies – probably better references for direct comparison can be found (see below).

Detailed remarks

Title and Abstract: I personally think that “structure” in the title implies more than the paper can provide in the end. Consider that in the abstract only a water source shift and a suggested vegetation diversification are mentioned concerning terrestrial ecosystem structure. General, the abstract could give less methodology and more own environment-related results.

Sections 1 to 3.2

I have almost no remarks to sections 1 to 3.2, because they are generally very well written and are of appropriate length and focus. One remark only: In line 33 the expression "the SIS melted, exposed the land …" sounds a little odd to me. I cannot say very much concerning the method sections since this is out of my expertise, but as far as I can tell this is also well done.

Section 3.3

I am not sure if there is an inconsistency with the discussion here – maybe it should be mentioned here already that the C28 1,14-diol was present in high amounts during the MB phase as mentioned in section 4.4 - the expression "only present" does not imply high amounts in my opinion.

Line 199: Checking Kotthoff et al. 2017, it appears to me that the Yoldia Sea (YS) is not reflected in the record described there, a comparison should not start before ca. 8000 yr BP.

Section 4

Section 4.2 line 234 and following: it is implied here that the pollen records mentioned in line 236 are nearby, but I think the records used un Seppä and Birks 2001 are quite far away (some pollen records from northern Germany, Southern Sweden, Denmark or Poland would be nearer).

Section 4.2.2

First paragraph: Again, I am not sure if Seppä and Birks 2001 is well fitting here.

In line 269 in the same paragraph it is stated that "the maximum extension of *Pinus* and *Juniperus* was recorded at 9.2 ka in these regional lakes"… Checking the cited literature, I can only partly agree: There are only minor increases of *Juniperus* pollen in the related time interval in Seppä et al. 2005/Digerfeldt 1977 and the *Pinus* peak is earlier. In Antonsson and Seppä 2007, there is a peak in *Pinus* percentages at 9.2 ka, but *Juniperus* percentages are significantly higher during the late Holocene and one could not speak of a maximum extension of this taxon around 9.2 ka. The sentence in line 270 and the following lines is correct concerning *Alnus*, but the pollen diagram of Lake Trehörningen depicted in Antonsson and Seppä 2007 does much more imply a decrease in *Pinus* (this one is very clear) than in *Juniperus* percentages (which does not seem to be consistently present between 11 and 5 ka anyway). I would think in this context that the attribution of the C isotope shifts to *Juniperus* shrub extension is not supported by the cited pollen records.

It would be really nice to have closer pollen records for comparison in which coeval *Juniperus* increases were present to support the interpretation concerning the C isotope shifts (e.g. the record shown by Yu et al. 2005 is quite near, but does not reveal such a signal), or if possible to see pollen data from core 64PE410-S7 itself.

Line 294: A citation for the 8.2 ka event would be good, particularly if one could be found for the research area.

Conclusions:

Here, the 8.2 ka event is mentioned again (and marked in the figures, too) while it was said in 4.2.2 that no such event could be found in the own record and no citation was given in the discussions for such an event. If mentioned in the conclusions, this should be discussed in more detail and with a citation that there was indeed a cold event around that time in the research area. Generally, it would be good if conclusions based on own results would be better indicated.