

Dear Dr. Seidenkrantz,

Thank you for the opportunity to revise our manuscript, “Co-evolution of terrestrial and aquatic ecosystem structure with hydrological change in the Holocene Baltic Sea” for publication in *Climate of the Past*. We appreciate your feedback and understanding throughout the review process and the constructive comments from three reviewers which have significantly improved the manuscript. To address the most significant issue, we have now included a figure of the age model. We have also addressed the remaining comments from the reviewers. Our response to reviewer comments can be found below. As requested, we are submitting both a clean copy of the revised manuscript and a separate document with highlighted changes.

We believe this improved version better presents the biomarker-based results in context with what is known about climate in the Baltic Sea region throughout the Holocene. We hope you will now be able to accept this revision for publication in *Climate of the Past*.

On behalf of all co-authors,
Gabriella M. Weiss

Dear Dr. Weiss,

Thank you for your re-submitted version of your manuscript “Co-evolution of terrestrial and aquatic ecosystem structure with hydrological change in the Holocene Baltic Sea” to “Climate of the Past”. As you are aware, your manuscript has been evaluated by three reviewers; all were the same as for the first review. All reviewers feel that you have significantly improved your manuscript and have to a large extent followed their comments.

However, as also pointed out by some of the reviewers, one important issue still remains: the age model. You now better explain, how you constructed the age model; however, as you do not show any figures on this, the reader has no chance of evaluating your age model. Thus, it is imperative that you present the age model.

You explain that you constructed the age model via a combination of 14C dates on your own core and correlation of the XRF data with two existing cores. However, you neither provide the 14C dates, nor show the actual data used in the correlation. It is imperative that this information is added. I strongly urge you to 1) add a table with the dates in the standard format (lab no, core depth, material, dates, errors, calibration, $\delta^{13}C$ if available) and 2) add a figure where you show the actual correlation of the data. You could make a figure where you show a picture/lithological log as well as the Ca/Ti and Br data from your own core (including marking the levels of your 14C dates) to the left and the data as well as dates from the two existing cores to the right. The correlation (wiggle matching) of course needs to be shown through correlation lines.

You must also 3) show the actual age model created through the Bacon software based on the 14C dates and XRF tie points. It is possible, that the age model can be combined in the same figure as the correlation figure above.

If you end up with an overload of figures, you could easily combine Figs 4-6 into one figure with 4 sub-figures.

We have included a figure of the age model – XRF data with tie points, 14C dates, and the age model generated by Bacon.

In addition, Reviewers 1 and 2 indicate a few further, but minor corrections and comments that you should take into accounts.

As always, when you resubmit, please reply to all reviewers’ comments in detail and mark clearly any changes made to the manuscript in the text through highlights or track-changes.

I look forward to seeing your revised manuscript.

Kind regards,
Marit-Solveig Seidenkrantz
co-Editor in Chief, Climate of the Past

Non-public comments to the Author:

I have marked the decision as "minor corrections", as, if I am satisfied with your improvements, I may not send the manuscript out to review again. However, please note that I will not accept the manuscript for publication unless you present your age model, so in this respect, it is in fact a

major, imperative correction. I know that making new figures is time consuming, but it will also significantly improve your paper.

We appreciate your understanding and have now included a figure with the age model.

Reviewer #1

Comments to manuscript: Co-evolution of terrestrial and aquatic ecosystem structure with hydrological change in the Holocene Baltic Sea

I have commented on an earlier version of this manuscript and suggested major revision. I can see that the authors have followed most of my suggestions as well as the suggestions by the other referees.

Thank you for the constructive suggestions that helped to improve the manuscript.

One point I raised was the question about the age-depth model. The authors have added some notes on this issue, but they still do not present an age-depth model. It is still unclear to me how the older non-marine part of the core was dated. The authors note in the manuscript that the “age model was created by combining ^{14}C -ages of mollusk shells and correlation of Ca/Ti and Br records with two nearby cores... (Warden et al., 2016). However, Warden et al. did not date the non-marine part of their cores. Instead, they referred to Moros et al. (2002). Moros et al. referred to Björck (pers. comm.). The most honest would be to plot data against depth, but this makes it difficult to compare and discuss with other records. Anyway, I think that the need to mention that the chronology of the older part of their record is uncertain.

We have now included an age model figure (Figure 2) which shows the XRF tie points and ^{14}C ages.

Other comments

Line 14. Ice melt, should be ice retreat

We have changed “melt” to “retreat.”

Line 19. Western should south-western

We have changed this.

Line 19. “In the earliest part of the record (10-8.2 ka)”. According to line 16 the record spans the last 11 ka. Does the record span 10 or 11 ka?

The record does go back to 11 ka, however 10 – 8.2 ka is the period where the hydrogen isotope values show a big change. We have revised this statement to say, “In the earlier part of the record (specifically 10 – 8.2 ka) ...”

Line 34. The Baltic Ice Lake started to form around 13 ka (Björck 1995). Please note that this reference refers to C-14 years BP. Therefore, it should be c. 15.5 ka, not 13 ka.

Thank you for catching this. We have changed the date to 15.5 ka.

Line 34. The Scandinavian Ice Sheet retreated (by melting and calving). Retreated is more correct than melted.

The addition of “melting and calving” was suggested by another reviewer. We choose to keep the parenthetical note in.

Line 49. Continental uplift, it should be glacio-isostatic rebound.

We have changed this phrasing as suggested.

Line 57. Deciduous woody forests, suggest change to deciduous forests.

We have removed the word “woody.”

Line 90. There are no C4 or CAM plants in the region, therefore the discussion about such plants are not relevant.

We have removed this statement.

Reviewer #2

No comments, suggested to accept as is.

Thank you!

Reviewer #3

Weiss et al. have extensively responded to the comments from the three reviewers and significantly improved the manuscript. The division of the discussion into the different phases improves the clarity a lot. What I would still suggest though is to add the ages you use for these phases, either in a table (where you could also add the previously determined ages for these phases and/or per area, e.g. Baltic Proper, Arkona/Southern Baltic, Belt Seas/Kattegat, or simply between brackets with each header. The reason being that, as you also point out, these phases may have been slightly different in different areas of the Baltic. I would also consider to change the title further to include the Ancylus Lake already with ages in brackets, because that is the main message of the paper. The rest of the Holocene has a few data points too, but is not adding that much to the story.

Thank you for your comments that have helped improve the structure and flow of the manuscript. We have now added ages in brackets to the headers in the discussion section.

I think the introduction can still be improved. From my first review I gathered that background on salinity was missing as the main reconstruction using the organic proxies was to reconstruct

salinity. But now an extensive paragraph is added on the vegetation history. This is definitely a good overview of what is available in the area but it also makes me wonder a bit what the organic proxies are adding to this. You mention the organic proxies are to complement the existing pollen records. So is this then adding information because they are more sensitive to hydrological changes than the pollen records, as these are not that much variable in these areas? I think you can bring this in the introduction more clearly as a statement that using the organic proxies you can separate between, e.g., meltwater and precipitation phases.

We have added a sentence to lines 77-78 that states, “Organic compounds and their isotopic signatures can provide additional constraints on changes in hydrology not easily discernible from pollen assemblages (e.g., they allow for differentiation between ice melt and precipitation as a water source).”

Presentation of the age model is still a major issue. All three reviewers pointed this out and the authors responded in detail. But I can still not find where these data are presented. I suggest adding this correlation with the other sites in the supporting information. Working with these different phases in the Baltic makes the age model essential, and when the reader then has to go find another paper or even the supporting information to those other papers, is not very convenient. The Warden paper does not have bromine data in it, and in the Weiss 2020 paper I can only find an excel table as supporting information that has the XRF data in it, but nothing on the age model. It would also be good to provide an error estimate on the ages, i.e. using the 9.2 ka as boundary between the two sub-phases of the AL needs an uncertainty range on it.

We now include a figure with the age model (Figure 2) which contains XRF tie points, 14C dates, and the age model output by the Bacon software.

Lines 204-206: “The large variations in sedimentation rates for core 64PE410-S7, and the nearby cores to which it has been correlated, are likely related to the shallow water depth in the Arkona Basin”. Why is that?

We have revised this sentence to emphasize that the fluctuating connection with the North Sea caused the large changes in sedimentation rates in the shallow Arkona Basin. “The large variations in sedimentation rates for core 64PE410-S7, and the nearby cores to which it has been correlated, are likely related to changes in the connection between the North Sea and the otherwise shallow Arkona Basin (~45m).” (Lines 213-215)

Lines 406-410: Why would this indicate that these temperatures would be unreliable? I think this may simply point to a signal that is mainly produced during a specific season, e.g. spring.

We have added this information (now on line 419), “This indicated that reconstructing temperature using LDI in the MB may be unreliable, likely due to the influence of diatoms and freshwater input, or that LDI may be reflecting a specific season (e.g., spring).”

In summary, I think the manuscript has improved a lot but still needs some streamlining and the information on the age model definitely needs to be included. After that, the manuscript would be a valuable addition to Climate of the Past.

Thank you for your constructive comments that have helped improve our manuscript.