

Clim. Past Discuss., author comment AC3
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Reply on RC3

Gabriella M. Weiss et al.

Author comment on "Co-evolution of the terrestrial and aquatic ecosystem in the Holocene Baltic Sea" by Gabriella M. Weiss et al., Clim. Past Discuss.,
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This is a review for the manuscript "Co-evolution of terrestrial and aquatic ecosystem structure with hydrological change in the Holocene Baltic Sea" by Weiss et al. The authors use a large suite of organic biomarker proxies to analyze environmental change in the Arkona Basin for the Holocene. As expected, large changes in the different proxies indicate large changes in hydrology and possibly climate in the region fitting with the well-known different phases of marine conditions in the Baltic. However, I do have some problems with the current structure and missing discussion. The resolution of the records is very low, in several cases there is only two or three datapoints for a phase. Along with the absence of any information on the age model, this makes a discussion on trends in phases and at their transition and the presence or absence of events like 8.2 or 9.2 kind of useless. The discussion basically reads as a long list, i.e. "this proxy changed in this direction meaning that" without hardly including anything on the many studies in the area itself; I think the ms is missing a big opportunity to make this a much better story (see also the comments below).

I am not an organic biomarker specialist so I cannot judge on the suitability of the methods, although the description of the analyses and their background in the introduction is very elaborately done.

We thank the reviewer for their constructive comments. The two other reviewers also asked for more information about the age model. As mentioned in those responses, "the core was dated by correlating XRF data to two nearby cores described in Warden et al. (2016). The ¹⁴C ages were corrected using local marine reservoir values from Lougheed et al. (2013; doi:10.5194/cp-9-1015-2013). We will add a more detailed discussion of this information to the revised manuscript."

I'm missing a discussion that is involving the enormous amount of studies that have been performed in the Baltic already. Most references are only related to biomarker records, some of them from non-Baltic locations. Because the resolution and age control are low and you are focusing on different phases, my suggestion would be to restructure the ms by starting to identify your different phases and what they are based on (i.e. existing

literature) and then pool the samples you have for those phases into a specific signal for that phase so that you are basically creating snapshots of those phases. In a next step these can then be compared with studies that are especially concentrated around the southern Baltic like the Arkona/Bornholm area (IODP expedition 347 – Site M0065; Heinrich et al., 2018; Anjar et al., 2012; Jensen et al., 2016); Belt seas and Kattegat (e.g. Kotthoff et al., 2017, Ni et al., 2020; Hyttinen et al., 2020), or lake/terrestrial records from northern Germany and southern Sweden (e.g. Dräger et al., 2017; Hannon et al., 2018). If such studies can be linked with the biomarker results it would make the study much more valuable in identifying the processes behind the signals.

Thank you for this suggestion. We will add a more extensive discussion of the existing literature on the Baltic Sea throughout the Holocene.

Line 31: The Baltic Basin existed long before the deglaciation

We will note this in the revised manuscript.

Intro: A very detailed background on organic proxies, but nothing about other salinity proxies in the Baltic. Many studies have attempted to reconstruct salinity changes in the Baltic, e.g. Gustafsson and Westman, 2002; Emeis et al., 2003; Mertens et al., 2012; Ning et al., 2017, Ni et al., 2020 and others.

We will add a discussion of salinity reconstructions and these references to the first part of the introduction.

First paragraph of the intro could use more referencing, it's very well studied!

We will add more references to the introduction.

Terrestrial vs marine....how does that work in the Baltic? How do you define the brackish environments with this regard?

The discussion of terrestrial versus marine should be changed to terrestrial versus aquatic (which would then be lacustrine versus marine). Brackish is the mixing of lacustrine and marine.

Age model: Just a short reference to previous papers explaining the age model is not enough. Sedimentation rates and variations can be extreme in the Baltic especially when changing between the different settings. It is essential that this is explained and shown in the ms. The Arkona Basin is located at an interesting point just after where the saline inflows enter the present Baltic. Timing in this area does not necessarily have to be the same as in the Baltic Proper or the Straits/Kattegat. Simply assuming that this is the case is unlikely to be true. Same goes for comparison with the lake records in Finland.

Other reviewers have suggested that we add more discussion of the age model as well, so we plan to add that into the revised manuscript.

I would not call the first 9.30 meters the top of the core when the whole core is 12 m. A 100 cm resolution is very low; is there any particular reason why this is so slow when downcore records were going to be reconstructed?

Our focus was primarily on the Ancyclus Lake phase, hence the higher resolution sampling in that part of the core.

Sediments in the Baltic are notorious for transport of material. How does this affect the different proxies? Radiocarbon studies in the central Baltic have shown that organic matter

is continuously re-deposited and can result in large temporal differences. Could this the reason some of your changes are not aligning with commonly accepted events?

Since we do not have radiocarbon ages on organic matter, we cannot rule out redeposition as an explanation for the smoothing out of some major events. However, the proxies used here should be affected by deposition in a similar manner to micropaleontological and other proxies used to establish such events in the Baltic. Those proxies record commonly accepted events, thus we have reason to believe our proxies should as well.

Line 241: With continental uplift I assume you mean isostatic rebound?

Yes, we do mean isostatic rebound.

Lines 246-248: "The SIS was retreating at this time (Muschitiello et al., 2015; Cuzzone et al., 2016), thus it is plausible that a meltwater pulse transported a higher concentration of *n*-alkanes from the north into the basin just after 10.2 ka." This reads like kind of a loose statement. This would require things like age control, and are there signs of a meltwater pulse (e.g. sedimentological)? Has this been shown before, then cite it, and if not you need to bring more explanation.

The sign of the meltwater pulse comes from the hydrogen isotope values and the statement will be revised to note that the meltwater pulse was before 10.2 ka. The increase in *n*-alkane hydrogen isotope values suggests less contribution from a ²H-depleted (lower hydrogen isotope composition) meltwater contribution to the system.

Section 4.2.1: First you conclude that the low concentration of alkanes indicates less continental runoff, but then following the other proxies you conclude that more continental runoff occurred. What does the literature say about this? Which pathways, e.g. rivers, were in the area, maybe climate was actually changing becoming drier or wetter.

In the early part of the Ancylus Lake, the *n*-alkane concentrations are low (likely due to ice cover) but then they increase by a large amount. We will make this more explicit in the revised manuscript. An increase in precipitation has been noted for other records, which we discuss in section 4.2.2.

Lines 293-295: "While our record is of insufficient resolution to capture this rapid event, the increase of $\delta^2\text{H}$ alkane values noted at 9.2 ka is presumably also influenced by the environmental conditions present at that time. Another rapid cold event occurred at 8.2 ka, which is not observed in our record, but may be elucidated with higher resolution sampling at this time interval." Indeed, as the age control is lacking and the resolution low your 9.2 event may well be the 8.2 one.

Yes, that is possible. However, both are cold events and would influence the proxies in the same way.

Several of the curves in the figures have no error bars on them. It would be good to add them. Add other relevant study sites to the map and include them into the discussion, e.g. Bornholm, northern Germany, southern Swedish lakes, Little Belt.

The error bars seem to be missing because they are small in some cases. The errors are listed in the tables (which will be moved to the SI in the revised version). We will add more locations to Figure 1.

The Pangaea link is still missing.

It will be added in the revised version.

To conclude, I think this dataset definitely has the potential to make an interesting manuscript after re-structuring and expanding the discussion. So currently I recommend major revisions.

Thank you for the constructive comments. We will add the suggested information into the revised version of the manuscript.