

Clim. Past Discuss., author comment AC1
<https://doi.org/10.5194/cp-2020-163-AC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Reply on RC1

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.,
<https://doi.org/10.5194/cp-2020-163-AC1>, 2021

This is a well-written manuscript that presents analyses of biomarkers and stable isotopes of specific compounds. The analyses are state of the art and have not previously been used in the south-western Baltic. It is important to use new methods to improve our understanding of past environmental changes. However, the interpretations of the data are not straightforward and after reading the manuscript I did not feel that I learned much new about the history of the Baltic Sea. I am sorry but I don't feel that the study "provided new insights into changes ... in the vegetation in the western Baltic region throughout the Holocene" (line 210).

We thank the reviewer for their praise and constructive comments on our manuscript. We will restructure the manuscript to focus more on the hydrological shifts noted in the isotope data and how our results confirm diversification of vegetation in the region after ~10.5 ka.

The interpretations are hampered by the fact that the geography of the Arkona Basin changed over time. During the Yoldia Sea stage it was a bay of the Baltic Basin, which was connected to the Kattegat via straits in south-central Sweden. During the early part of the Ancylus Lake stage the outflow shifted from south-central Sweden to what became the Danish straits. During the Littorina Sea stage it was part of the Baltic Sea, with outflowing and inflowing water masses. Water level varied a lot over time due to glacioisostatic rebound and global eustatic sea level rise.

We agree that the geography of the basin changed a lot during the Holocene. Our main interest lies in the terrestrial catchment and linking plant biomarker isotopes to changes in vegetation and regional hydrology. The main impact of the geographical changes occurring in the basin will be on the distance from the continent to the core site (i.e., decreasing with decreased sea level). In addition, increased sea level can cause enhanced coastal erosion, mobilizing soil and plant matter. We will add more discussion on the impact of sea-level changes in the revised manuscript.

Another issue concerns erosion, reworking and redeposition. Erosion is particularly important at the transition from the Ancylus Lake to the Littorina Sea. Could it be that

reworking explains the low $\delta^{13}\text{C}$ values of bulk sediment samples from the Transgression phase?

During the transgression, there is a shift from low $\delta^{13}\text{C}_{\text{bulk}}$ (characteristic of C_3 plants and freshwater algae, Fig. 2) to higher values typical for marine environments. It is possible that, at the beginning of the transgression, enhanced erosion of coastal soils explains the sharp peak in TOC (Fig. S1) although it is not reflected in the $\delta^{13}\text{C}_{\text{bulk}}$.

It is mentioned that some of the molecules analyzed in the study can be wind transported (line 69). I wonder if the molecules in the sediment formed in the Arkona Basin and the surrounding land area? Or could it be that the molecules come from the Kattegat and were transported by inflowing water to the Arkona Basin? Or did they come from northern parts of the Baltic Sea or from the whole drainage area of the Baltic Sea?

Long-chain alkanes were likely produced by vegetation in the whole drainage area of the Baltic Sea. The short-chain alkanes could have been produced in the Arkona basin or transported into the basin from surrounding lakes and rivers.

The authors discuss the results "by Baltic Sea phase"(s) (line 212). The phases are Yoldia Sea, Ancylus Lake and Littorina Sea. However, the discussion is divided into: Yoldia Sea, Ancylus Lake, Ancylus Regression, Ancylus Lake vegetation and hydrological change, Marine transgression and finally Littorina Sea and Modern Baltic phases. I suggest that the authors use only three headings. What they term Marine Transgression is part of the Littorina Sea stage.

We will change the division of the discussion to follow this suggestion.

Modern palaeostudies relies on well-dated records and high temporal resolution. However, I get the feeling that the chronology of the studied record is poorly constrained. The age-depth model needs to be described in detail in the paper. It is mentioned that the age-depth model is described in detail in Weiss et al. (2020) and that it was created by combining ^{14}C -ages of mollusk shells and ... (line 99). According to Weiss et al. (2020) three radiocarbon ages were obtained. I could find no information on the ages or which species was used for dating in the 2020 paper or in the supplementary material. Also, I saw no information about which calibration curve was used. It should also be explained in the paper that only calibrated ages are discussed. In particular, I wonder how the older non-marine part of the core was dated.

The core was dated by correlating XRF data to two nearby cores described in Warden et al. (2016). The ^{14}C ages were corrected using local marine reservoir values from Loughheed et al. (2013; doi:10.5194/cp-9-1015-2013). We will add a more detailed discussion of this information to the revised manuscript.

With respect to resolution, I note that four samples were analysed for the time period from 6 ka to the present. The resolution is higher in the older part of the record.

The focus of our study is on the Ancylus Lake, hence the difference in sampling resolution. We will make this more explicit in the revised version of the manuscript.

The paper must also provide information about the sediments in the core, based on a visual core description.

We will add a supplementary figure with the core description.

One of the main scientific questions in the development of the Arkona Basin and the Baltic Basin is the dating of the transition from the Ancylus Lake to the Littorina Sea. Andrén et al. (2000) dated it to ca. 10.1 cal. ka BP and Berglund et al. (2005) dated it to ca. 9.8 cal. ka BP, whereas other studies have dated it to 7-8 cal. ka BP. I am surprised to see that this question is not mentioned in the manuscript. Did the study provide any information on this issue?

Our results suggest a later transition from the Ancylus Lake into the Littorina Sea. We will add more detail about this in the revised manuscript.

I think one of the most interesting outcomes of the study is that the Ancylus Lake stage was highly dynamic with large isotopic shifts. But the interpretation is difficult. It was partly caused by decreasing influence of melt water. But I suggest that the shift in drainage may also play a role. I am not sure about the importance of the short-lived cold events during this time.

We agree with the reviewer that the Ancylus Lake phase was dynamic. We will add discussion about the shift in drainage as another possible contributing factor to the complexity of the phase in the revised version. However, we note that such depleted hydrogen isotope values of lipids followed by a large, positive shift is difficult to explain by a change in drainage alone.

Other comments

Line 15. According to the first sentence in the abstract: "The Baltic Sea experienced a number of marine transgressions and regressions throughout the Holocene". However, only one marine transgression is discussed in the manuscript. The northern part of the Baltic Sea experienced regression throughout the Holocene, whereas the southern part experienced one marine transgression during the Holocene.

We will revise this statement to: "The Baltic Sea experienced changes in marine input throughout the Holocene."

Line 16. According to the next sentence: "These fluctuations in sea level coupled with substantial regional ice melt led to isostatic adjustment and periodic isolation from the North Sea". Isostatic adjustment was caused by ice retreat, not by the "fluctuations in sea level".

Thanks for catching this. We will revise it to: "The substantial regional ice melt led to isostatic adjustment and periodic isolation from the North Sea."

Line 28-29. "that promote ... diverse phytoplankton communities". A reference is needed. Is the diversity larger than in other areas?

We will add the following references to the end of this sentence: e.g., Wasmund and Uhlig (2003) doi: 10.1016/S1054-3139(02)00280-1; Golubkov et al. (2020), doi: 10.1016/j.oceano.2020.11.002.

Line 31, 33. Retreat was caused by melting and calving.

We will add "calving" to the reason for the retreat of the SIS.

Line 33. "the Scandinavian Ice Sheet covered large swaths of Europe". Change to: the Scandinavian Ice Sheet covered large parts of northern Europe.

We will make this change in the revised manuscript.

Line 35. The Yoldia Sea stage began in the earliest part of the Holocene. Therefore it was not caused by global and regional temperatures that continued to rise during the Holocene.

We will remove “during the Holocene” in the revised version.

Line 38. Sea level fluctuations. It should be water level fluctuations, because lake stages are also involved.

We will correct accordingly in the revised manuscript.

Line 38. The Ancylus Lake was a freshwater lake - I find it a bit strange to call it a low salinity phase.

We respectfully disagree. Some brackish diatom species have been found in Ancylus Lake sediments (Alhonen, 1972) and Winterhalter (1992) noted that saline water flowed into the basin during the Ancylus Lake phase. It seems most likely that the surface water layer was fresh, but there was brackish / saline water at depth.

Line 41. The freshening was probably caused by land uplift, increased precipitation and decreased evaporation, not by lack of large marine transgressions.

We will omit “lack of large marine transgression” in the revised manuscript.

Line 42. until now the authors have discussed salinity changes, but now they state that “The complex climate dynamics caused substantial shifts in the salinity of the Baltic Sea during the Holocene, indicated by changes in the phytoplankton population”. However, the main salinity changes were not caused by climatic changes.

We respectfully disagree and think the main salinity changes are linked with climatic changes. The melting of the SIS followed by large freshwater input and isostatic changes are all linked to climate. These factors contributed to the changes in the regional environment (i.e., salinity of the basin) which in turn led to shifts in the phytoplankton (and higher plant) populations.

Line 57. recalcitrant – is that the same as resistant?

Yes.

Line 60, 61. Are there any relevant C₄ and CAM plants in the region?

To the best of our knowledge, there are no C₄ or CAM plants in the region.

Line 71. It is mentioned that wind-transported n-alkanes are generally deposited within weeks. Does this mean that part of the n-alkanes could have their origin in North America?

Alkanes can be transported long distances. For example, alkanes thought to be from the Sahara desert were found in Atlantic sediments, but in low abundance (Schreuder et al., 2018, 10.1016/j.orggeochem.2017.10.010). It seems unlikely that n-alkanes in our record were transported all the way from North America.

Line 74. preserve information – change to can provide information.

We will make this change in the revised manuscript.

Line 77. convolutes – is this the correct word?

We will change this to “complicates” to avoid confusion.

Line 96. what was the diameter of the piston core?

The piston core has a diameter of 10 cm.

Line 97. what was the water depth at the core site?

The water depth at the core site was around 45 m.

Line 98. Arkona Basin not Arkona basin.

We will change into “Basin” in the revised manuscript.

Line 110. I don't think the authors extracted sediment samples, it should be lipids.

Organics were extracted from the sediments. We will change the sentence in the revised manuscript.

Line 161. The authors note large difference between the piston core described in the manuscript and nearby cores. I wonder if these differences could be explained if the other cores are gravity cores?

It seems our wording was not clear here. The nearby cores also recorded large changes in sedimentation rate. We will revise the sentence to make this more explicit.

Line 199. The temperature reconstructions for the early part of the record is not similar to those of Kotthoff et al. (2017). The record by Kotthoff only went back to ca. 7.4 ka. I don't think that 18.5°C for the Yoldia Sea phase, 11.5°C for the Ancylus Lake phase and 24° fit with other temperature reconstructions from the region.

It is true that the record of Kothoff et al. (2017) only goes back to 7.4 ka. As the only other record of temperatures reconstructed using LDI in the area, we felt it should be included. Our record has similar reconstructed temperatures as presented in Kothoff et al. (2017). Because our main focus was not on temperature, we originally chose to omit more detailed information about this to avoid adding to the complexity of the discussion. We will add more discussion about the utility of this proxy in the Baltic and the similarity with other Baltic temperature reconstructions in the revised version.

Line 212. change Baltic Sea phase to Baltic Sea phases.

We will change accordingly in the revised manuscript.

Line 215. The salinity of the Yoldia Sea phase was first discussed from the presence of *Yoldia arctica* (now *Portlandia arctica*, a marine bivalve). However, marine species are only recorded from the Baltic proper (including the Gotland Deep discussed by Sohlenius et al.). Not sure if any marine species have been recorded from the southern part of the Baltic Basin.

Alkenone distributions characteristic of marine alkenone producing species were observed in these samples (described in Weiss et al. (2020)).

Line 218. Yoldia Sea sediments in the Arkona Basin are usually considered to be barren of fossils (except for reworked pollen and spores). It is interesting to see that the authors found diols that are produced by freshwater eustigmatophytes. However, I wonder if these algae lived in the Arkona Basin, or in rivers and pools in the catchment?

Eustigmatophytes have soft tissues only, but the organic compounds they produce (i.e., diols) do preserve well in sediments and are colloquially known as "chemical fossils". Eustigmatophytes producing the C32 1,15-diol were likely living in the rivers and ponds of the catchment as suggested by previous studies (Lattaud et al., 2017).

Line 234. The pollen records referred to are not nearby.

We agree with the reviewer that the records are not just next to the core site, however we consider southern/central Sweden to be within the Baltic Sea region.

Line 235. Strictly speaking, temperatures can be low or high, not cold.

We will change this in the revised manuscript.

Line 241. Moros et al. (2002) did not suggest that the Baltic Sea experienced "a large regression" at 10.2 ka – they only suggested a regression. The evidence for this regression was weak. It is currently debated if the Ancylus Lake stage ended with a large regression, a small regression or no regression. If there was a indeed a regression in the Arkona Basin, it was definitely not caused by "a continental uplift".

We will revise the sentence to state simply that Moros et al. (2002) suggest a regression occurred at 10.2 ka, and then discuss how our data confirms the idea of a regression at this time.

Line 247. The authors suggest that a meltwater pulse occurred at 10.2 ka. However, some studies indicate that the Scandinavian Ice Sheet expanded at ca. 10.2 ka (the Erdalen event).

With our results, and those of Weiss et al. (2020), a meltwater pulse is a possible explanation for the negative alkane and alkenone hydrogen isotope values noted before 10.2 ka. The Erdalen event mainly concerned the western Norwegian glacier, which might explain why the Arkona Basin sediments did not record such an event. Previous studies suggest a decrease of the Scandinavian Ice Sheet at that time (Muschitiello et al., 2015; Cuzzone et al., 2016). The decreased amount of ice melt as source water for plants is reflected in the shift to higher hydrogen isotope values. We will make sure it is clear in the revised manuscript that the meltwater pulse was before 10.2 ka.

Lines 258-259. did you observe a thin layer rich in remains of terrestrial plants at the same level? Such a layer is seen in many cores from the Arkona Basin.

We will check the core for this thin layer.

Line 266 Pinaceae is a family name, it should not be in italics

We will correct accordingly in the revised manuscript.

Line 267 "can be tentatively attributed to *Juniperus* shrub extension" (should be expansion?). To my knowledge, no pollen records from the region show a *Juniperus* peak

at ca. 9.2 ka. However, the 9.2 event was short-lived and you need extremely high-resolution pollen analyses and high sedimentation rates or varves) to be able to detect possible influence on the vegetation.

We will revise the sentence to say “expansion” rather than “extension”.

Line 268. I don't understand why the authors chose to compare their record with pollen records from far away (northernmost Finland and Bohuslän in south-central Sweden). Why not compare with nearby pollen records? Anyway, to my knowledge no maximum occurrence of *Pinus* and *Juniperus* at 9.2 ka have been reported in pollen diagrams from the Arkona Basin region. See for example the detailed and well dated pollen diagram from Krageholmssjön in Scania in southernmost Sweden (Berglund et al. 2008, Veget Hist Archaeobot).

We will add the two Berglund et al. (2008) findings into our discussion of regional vegetation. These two pollen records show an increase in *Pinus* between 9.2 and 9.5 ka (Berglund et al. 2008a – doi: 10.1016/j.quaint.2007.09.018, and Berglund et al., 2008b – doi: 10.1007/s00334-007-0094-x) and revise the discussion to emphasize the dominance of woody species between ~11 and 3 ka.

Line 269. what is a regional lake?

By “regional lakes” we mean lakes from the Scandinavian region. We will make this clear in the revised manuscript.

Line 289. Not sure what you mean by this: “The global transition from a glacial to an interglacial climate state across the Holocene, was punctuated by a few abrupt cold events”. The cold events mentioned in the following happened long after the glacial-interglacial transition.

We agree with the reviewer, the actual transition between the glacial/interglacial period happened before the Holocene. The cold events mentioned here occurred during the Holocene. We will correct this sentence in the revised manuscript.

Line 293. It is unclear to me whether the authors see evidence of the 9.2 ka event in their data.

At 9.2 ka we see a peak in $\delta^2\text{H}_{\text{alkanes}}$ which may have been caused by the 9.2 ka event.

Line 297. Moros et al. did not give an age of 7.7 ka for the re-establishment of the connection between the Baltic Sea and the North Sea. From where did you get this age?

Moros et al. gave an age of 6.475 ± 50 ^{14}C yr BP for foraminifera from the transgression. Warden et al. 2016 gave the age of 7.7 ka for the start of transgression which reached a maximum at around 7.2 ka, this reference will be corrected accordingly.

Line 298. Do you mean that the onset of the transgression lasted from 7.7 to 7.2 ka. Or do you mean that the transgression lasted from 7.7 to 7.2 ka? I believe that the marine transgression of the Arkona Basin began somewhere between 7 and 8 ka and continued for the rest of the Holocene, although the transgression rate slowed down after ca. 6 ka.

Based on the discussion in Warden et al. (2016), the start of the transgression

occurred around 7.7 ka and reached a maximum at around 7.2 ka. We will clarify this in the revised manuscript.

Line 300. regional warming began already in the earliest part of the Holocene, although interrupted by some short-lasting cold events.

We will change this in the revised manuscript.

Line 304. change *n*-alkanes were to *n*-alkane values were.

We are referring to the group of mid-chain *n*-alkanes so the “s” is needed.

Line 341. Again, to my knowledge, no “large fluctuations in the extent of gymnosperm cover” have been reported by pollen studies from the region.

We will change this in the revised manuscript.

Line 345. “lack of diversification of terrestrial vegetation noted for this period”. Which period? The Ancyclus Lake stage? The pollen records from the region show that many species arrived during this period.

Between ~11 and 6 ka, woody species dominate pollen assemblages in the region and there is a gradual increase in species diversification. We will revise this statement accordingly in the revised manuscript.

Line 347. “regional warming which continued into the Late Holocene”. Warming certainly did not continue into the Late Holocene.

We will remove “which continued into the Late Holocene.”

Line 351. I don’t think that we can ever “fully understand the complexity of paleoenvironments” as stated by the authors – not even if we use multiple proxies.

Perhaps this is true, but multiple proxies can certainly provide a more holistic understanding.

Figures and tables

The authors have Age (ka), Age (Ka) and Age (kyr). I am not sure if the journal has a style to follow, but it should be consistent.

We agree that Ka and ka should be consistent. Kyr and ka mean different things: ka means years ago (e.g., 10.2 ka event) and kyr refers to kilo years (e.g., the event lasted for 5 kyr).

Table 2 and 3 can go to supplementary.

We will add these tables to the SI.