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Interactive comment on "Reconstructing Holocene temperature and salinity variations in the western Baltic Sea region: A multi-proxy comparison from the Little Belt (IODP Expedition 347, Site M0059)" *by* Ulrich Kotthoff et al.

Anonymous Referee #3

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The manuscript by Kotthoff et al. based on a multi-proxy approach is very interesting. However, while combining that much different proxies is not an easy task, some more thorough discussion is needed concerning (1) the climatic forcing possibly explaining the different salinity and temperature trends observed over the Holocene, and (2) the high discrepancies between proxies for the same parameter (temperature). A graphical comparison with previously published records from the study area is also missing. The text suffers from some imprecision in the Results and the Discussion, some parts of the text should be reorganized, some figures should be modified and some new figures should be provided (as supplements). Finally, a calibration issue related to some

organic proxies should be addressed. Therefore, I recommend the publication of the present study, but only after major revisions.

Major comments:

The introduction should be reworked partly. After the second paragraph, it sounds like an enumeration and description of the proxies that will be applied in the study. It is not necessary and belongs rather to the Discussion part. Instead, previously published Holocene records from the Baltic Sea and the Skagerrak region should be mentioned and the main results should be described as in lines 97 to 104, but in more details. In my opinion, at least the following studies on Holocene temperature and salinity changes should be mentioned: Emeis et al., 2003, The Holocene (salinity and temperature); Warden et al., 2016, Organic Geochemistry (salinity); Krossa et al., 2015, Boreas, and Krossa et al., 2017, The Holocene (salinity and temperature); Ning et al., 2015, Boreas (salinity); Butruille et al., 2016, The Holocene (temperature); Zillen et al., 2008, Earth-Science Reviews (climate and hypoxia); Widerlund and Andersson, 2011, Geology (salinity). Based on these previous results, the necessity of a long and continuous record from the Belt Sea as intermediate location linking the Baltic Sea and the Skagerrak region can be introduced (in lines 97-104).

Some other parts of the text should be reorganized. As some results are discussed/presented in Section 2.2.1, I would suggest merging it into Section 3.1. The problem related to Mg/Ca contamination is mentioned at least three times in the manuscript (Methods, Results, Discussion). Because of repeating this issue again and again, one could consider removing completely this record from the study as this proxy is not really reliable. It would be a pity however. Therefore, I recommend shortening and grouping the different parts about this contamination issue somewhere in part 4.2, and discussing this issue in more details in the supplements (if necessary). Concerning Section 4.2, while it is possible to discuss the records based on the different environmental zones for the salinity and productivity proxies, it is confusing for the temperature proxies as these latter present completely different long-term and short-term trends. I suggest reworking/reorganizing this part. First, the differences in the temperature proxy records (trends, absolute values, amplitudes, etc) should be discussed, then the temperature trends should be summarized as a function of the environmental zones, and finally the potential forcing behind the temperature records should be discussed (see below).

I have a few comments concerning the TEXL86 temperature proxy. First of all, I was wondering if a standard was used for GDGTs quantification. If yes, I suggest using the absolute concentration of the branched GDGTs rather than the BIT index as the BIT index is often mostly function of variability in crenarchaeol (usually the dominant GDGT). Generally speaking, the use of TEXL86 should be avoided because the crenarchaeol regioisomer plays a role in the temperature predictability (relatively more of the regioisomer is observed at higher temperatures). Furthermore, the TEXL86 calibration from Kabel et al. (2012) is only based on the highest correlation with summer SST, but has no "biological grounds". When looking at the supplementary information in Kabel et al. (2012), it appears that the correlation is high ($r2 > 0.7$) for all months from May to November (i.e. not only for the summer months) and not only for TEXL86, but also relatively high ($r2 > 6$ from June to October) for TEXH86. Moreover, the IODP M0059 site location is out of the area covered by Kabel et al. (2012) calibration, what may play a role considering a possible influence of strong salinity gradient on Thaumarchaeota distribution in the western Baltic Sea. Another factor potentially complicating the TEXL86 record is the presence of a redoxcline and hypoxic to anoxic conditions. It is known that in the modern, Thaumarchaeota are most abundant at depth near the redoxcline in the Baltic Sea (e.g. Labrenz et al., 2010, ISME Journal; Berg et al., 2014, ISME Journal). Therefore, on the one hand the recorded temperature may rather be from the subsurface, or even the near bottom if anoxic conditions are present near the bottom as in the Bornholm Basin. On the other hand, culture experiments have shown that increased O2 limitation may result in increased TEX86 SST estimates (Qin et al., 2015, PNAS). Indeed, van Helmond et al. (2017) have shown that seasonal hypoxia occurred over the last 8,000 years at Site M0059 and intensified during the

HTM and, more especially, during the MCA, i.e. when the TEXL86 temperatures are highest. This aspect should be shortly discussed. It would be very interesting to plot also a TEXH86-based temperature record using a global calibration, e.g. Schouten et al. (2013, Organic Geochemistry) or Kim et al. (2012, EPSL) subsurface calibrations on Fig. 4 (or as supplement) as well, and to discuss potential differences. Moreover, as methanogenic and, more especially, methanotrophic archaea produce GDGTs involved in TEX86 in substantial amounts, it would be interesting to test their potential influence by plotting e.g. the Methane Index (Zhang et al., 2011, EPSL) as supplementary information.

Some imprecision are apparent in the text. Compared to e.g. the LDI record (same samples as for GDGT analysis), the oldest three samples of the TEXL86 record are "missing" (not plotted), without explanation, what makes a true comparison difficult (see e.g. lines 667-668). Furthermore, to be as correct as possible, a global TEX86 calibration for lakes (e.g. Powers et al., 2010, Organic Geochemistry) should be applied for the samples from EZ1, as this latter is characterized by freshwater conditions (lines 510-511). Obviously the TEXL86 record is NOT "... to some degree similar to the clumped isotope record" as stated by the authors (lines 674-676). For examples, the temperatures are equally high in the HTM and the modern in the clumped isotope record, but not in the TEXL86 record. The absolute values are different as well. Moreover, if plotting the TEXL86 temperature data of Kabel et al. together with Site M0059 on Fig. 4 (what I suggest to do), I suspect that these two records are not that similar (lines 685-688) concerning both the temperature amplitudes and the trends.

As for the TEXL86, a calibration based on lake sediments (Rampen et al., 2014) should be used for the samples from EZ1. The strong temperature increase (10 °C) at the transition between EZ1 and EZ2 may be an artefact due to the different calibrations as discussed in lines 659-661.

The Diol Index is not convincing as surface salinity proxy. It suggests similar conditions/salinity during the freshwater lake, as well as in the mid-Littorina Stage (ca.

4,500-4,000 cal. yr BP) and maybe the late Littorina Stage (ca. 1,000-500 cal. yr BP), although the Littorina Stage was marine-to-brackish. While salinity may affect this index together with temperature, I suggest removing this proxy from the study, or discussing it in more details. Based on the proxy results, it is in my opinion difficult to separate between surface and deep salinity changes, especially at 37 meter water depth. The salinity history reconstructed here concerns probably rather the complete water column. While a precipitation increase (pollen-based record) may explain a salinity decrease between 8,000 and 4,000 cal. yr BP, why is the salinity increasing/high between 4,000 and 1,000 cal. yr BP, while the precipitations are highest? Please, discuss the potential mechanisms for this salinity increase, as well as for the salinity decrease over the last 1,000 years.

Considering the high heterogeneity in the different temperature proxies from Site M0059, some previously published, marine-based and pollen-based temperature records as mentioned in lines 626-636 should be plotted in Fig. 4 for comparison.

A discussion concerning the forcing and mechanisms behind the temperature records and the difference in the temperature trends of the proxies is missing, or not thorough enough. Why are the LDI and TEXL86 records that much different although both should reflect summer temperature? Why are the pollen-based and TEXL86-based summer temperature records that much different? Why are the trends in MTCO and MTWA opposite? What are the expected/modeled evolutions of winter and summer temperature in northern Europe during the Holocene? How does seasonality change over the Holocene? What about insolation? Etc . . .

Minor comments (some redundancy with the major comments is possible):

Lines 124-125: Are those surface or deep currents?

Lines 145-147: What are the time intervals for “a transitional low salinity phase” and “the Littorina Stage”? Please, add.

Line 330: Is now published in Marine Geology.

Lines 340-343: For consistency, these lines should be moved to Section 3.2. What is meant with “... and between Holes (Fig. 2).”?

Chapter 3.3.2 (font size too small): A reference to Fig. 3 is missing.

Line 491: Why not mentioning that this is the Ancylus Lake Stage as in van Helmond et al. (2017)?

Line 502: Why “lowermost part”? The complete EZ1 suggests freshwater conditions.

Lines 504-505: But this is in disagreement with van Helmond et al. (2017) suggesting a eutrophic freshwater environment with high productivity...

Lines 510-511: To be as correct as possible, a TEX86 global calibration for lakes (e.g. Powers et al., 2010, Organic Geochemistry) should be applied for the samples from EZ1, as this latter is characterized by freshwater conditions.

Lines 537-538 and 541: Could you develop/discuss these sentences about foraminiferal δ18O? It could also be a temperature effect ...

Lines 546-547: On which proxy (proxies) are these salinity values based?

Line 549: The values of the Diol Index are as low as in freshwater water conditions. This is not so realistic.

Line 552: I’m not convinced about this decreasing surface salinity as (1) the results based on the Diol Index are not realistic and (2) there is no trend in the diatom assemblages.

Lines 562-565 and 576: The peak (one sample ...) in marine diatoms is not synchronous with the high values of the Diol Index that occurred after the transition. These are two distinct events. And the Diol Index values are not “particularly high” compared to the rest of the record.

Lines 569-570: Why is "∼1,700 cal. yr BP" written here in brackets? While the high productivity together with the high precipitation during EZ3 is apparent, there is nothing particular at ca. 1,700 cal. yr BP.

Lines 571-572: This sentence is not clear. Please, explain.

Line 590: Replace with "between ∼2,000 and ∼300 cal. yr BP".

Line 593: For consistency with van Helmond et al. (2017), please use Medieval Climate Anomaly (MCA).

Line 599: This sentence is long. Suggestion: "... ostracods. As the assemblage ...".

Line 611: Why not mentioning the pollen-based transfer function as organic temperature proxy?

Line 618: "... are feasible ..." sounds strange, wouldn't e.g. "... were obtained ..." be better?

Line 622: Change "comprising" to e.g. "representing".

Line 623: Replace "... fits well with ..." with "... are close to ...".

Lines 624-626: If possible, a calibration based on lake sediments (Rampen et al., 2014) should be used here. The strong temperature increase (10 °C) at the transition between EZ1 and EZ2 may be an artefact due to the different calibrations as discussed in lines 659-661.

Lines 626-636: Some of these records (marine-based and pollen-based) should be shown here for comparison, especially considering the high heterogeneity in the different temperature proxies from Site M0059. A reference to Krossa et al. (2017) alkenone-based records form the Skagerrak is missing.

Lines 632-633: Because of the extremely low and inconstant sample resolution of the clumped isotope record, no trend can be seen. Remove this part of the sentence. For

the same reason, I would further suggest to remove the lines between the dots in Fig. 4. Such an extrapolation is not realistic.

Line 641: Change MWP into MCA.

Lines 655-657: This sentence is not necessary. It could be removed.

Lines 661-663: This sentence is not necessary. It could be removed.

Lines 667-668: However, the oldest three samples of the TEXL86 record are missing, what makes a true comparison difficult. I suggest removing "...absolute temperatures based on the TEXL86 lipid paleothermometer and ...".

Lines 667-668: If not done, add a line break here. The text is much too dense.

Lines 672-674: Remove this sentence. If a summer calibration (Kabel et al., 2012) is used, than the reconstructed SST should be close to summer SST.

Lines 674-676: No, the TEXL86 record is NOT "... to some degree similar to the clumped isotope record". The temperatures are equally high in the HTM and the modern in the clumped isotope record, but not in the TEXL86 record. The absolute values are different as well. Remove this part of the sentence. And change the end of the sentence in " ... as well as the temperature records based on pollen and Mg/Ca ratios of benthic foraminifera."

Lines 685-688: Please, plot the TEXL86 temperature data of Kabel et al. together with Site M0059 on Fig. 4. I suspect that these records are that similar concerning both the temperature amplitudes and the trends. Remove "the" before "TEXL86".

Lines 713-714: But this concerns only a very little aspect/part of the records ... This is not really convincing. Same comment for the Abstract.

Line 718: NO ! This temperature increase is very probably an artefact.

Line 726: But no quantitative record is shown in this study...

Lines 727-730: These results are based on a figure from the supplements...

Figures:

Fig. 2: Please change MWP to MCA and HCO to HTM (for consistency with the text) and explain the acronyms (LIA as well).

Fig. 3: For consistency with the text, please rename “Diatom abs. Abundance” into “Abs. Diatom Abundance (ADA)”. And add “(CRS)” after “Chaetoceros resting spores”.

Fig. 4: Why no GDGT-based data exist for the three deepest/oldest samples although LDI-based data are present? Please change MWP to MCA and HCO to HTM (for consistency with the text) and explain the acronyms (LIA as well). Plot the TEXL86 temperature data of Kabel et al. together with Site M0059. The scale of the BIT index is not readable. The BIT index should be removed and should be plotted correctly (e.g. with a break in the Y axis) with the TEXL86 temperature record as supplementary figure. Add 2 previously published temperature records from the region (1 pollen-based and 1 marine-based record). The text in the topmost part should be turned over.

Supplementary information:

Where are the captions for Tables S1 to S4?

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