

Interactive comment on “Holocene sub centennial evolution of Atlantic water inflow and sea ice distribution in the western Barents Sea” by S. M. P. Berben et al.

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Received and published: 7 October 2013

We would like to thank Dr. Juliane Müller for her thorough and constructive feedback. Hence, we will take all corrections and suggestions into consideration in order to improve our manuscript. Therefore, we would like to respond to some of the comments.

“However, a comparison of the results with the foraminifer, stable isotope and ice rafted detritus data published by Werner et al. (2013; Paleoceanography) would be convenient.”

A more elaborate comparison of the results in this study with regards to Werner et al. (2013) will be executed and incorporated within the different discussion parts. Major

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points are as follows. Throughout the early to late mid Holocene, a strong influence of Atlantic water was attributed by Werner et al. (2013) (as in this study) to an observed high relative abundance of *T. quinqueloba*. Further, for this time period, Werner et al. (2013) made a link between high planktic foraminiferal fluxes and ice-free conditions or a seasonal fluctuating sea-ice margin. Here, a similar conclusion of reduced sea ice conditions is also drawn, albeit based on the sea ice biomarker analysis. Hence, both studies seem to confirm similar observations of oceanographic changes throughout the region by different proxy records. Throughout the late Holocene, both studies indicate reduced salinity or a freshening of the uppermost surface layer with concomitant experiences of increased sea ice conditions. Despite the observed cooler and weaker subsurface Atlantic water inflow during the late Holocene by Werner et al. (2013), they also conclude that there was a slight re-strengthening of Atlantic water inflow after 3 ka as seen in their sSST record. Although a similar increase in sSST is not record in this study, we do point out that an increased influence of Atlantic water was also suggested by different studies from the same study area. Nonetheless, Werner et al. (2013) found a substantial different abundance of *T. quinqueloba* throughout the mid Holocene compared with the record presented in this study. This leads to conflicting interpretations - more specifically - a strongly weakened Atlantic water inflow or drop in Atlantic water temperatures (Werner et al., 2013) versus a stable influence of Atlantic water inflow (Berben et al., 2013). Although we present an argument to explain this within the manuscript that is based on different preservation conditions, we also suggest that the different location between both studies may simply reflect differences in distance between each study area and the Arctic Front.

“Regarding the biomarker part of the manuscript, the meaning and thus the use of 24-methylenecholesterol should be explained in more detail. Previously, Knies et al. (2005; *Geochimica et Cosmochimica Acta*) and Cabedo-Sanz et al. (2012; *Quaternary Science Reviews*) used this biomarker as an indicator of sea ice cover (during the Younger Dryas) in the southern Barents Sea - this should be briefly compared to own data.”

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We agree that the data were presented but not discussed. The aim was to test the hypothesis that this sterol had a (mainly) sea ice origin. In fact, the data in Cabedo-Sanz et al., (2012) and those shown here suggest that while there may be some enhancement of the 24-MC signal when sea ice is present, it is not as selective a biomarker for sea ice as IP25 (the abundance and flux profiles are intermediate between those of IP25 and brassicasterol)

“With regard to the use of IP25, the paper by Müller et al. (2012; Quaternary Science Reviews) about Holocene sea ice variations in eastern Fram Strait could be addressed as well. Plus, the explicit description of fluctuating sea ice conditions (p.4915, line 13-17) and the similar observation reported by Müller et al. (2012).”

The paper by Müller et al. (2012) does not record the very early Holocene so cannot be compared here. However, the increase in IP25 (and inferred sea ice) from early Holocene to recent in Müller et al. (2012) is not seen in the current study and such a comparison can be made. This is useful and likely reflects the relative latitudes of the two study locations – in fact, the absence of IP25 in the study by Cabedo-Sanz et al. (2013) for northern Norway emphasizes this point further. That said, we report variable sea ice conditions here since ca. 1100 cal yr BP and a similar observation was reported by Müller et al. (2012) for the West Spitsbergen Shelf, but for the last 3000 yr.

“The IP25 and phytoplankton biomarker data by Berben et al. also enable the calculation of the semiquantitative PIP25 index. And though Müller et al. (2011, 2012) point out that the palaeoenvironmental interpretation of IP25 and phytoplankton biomarker data should be mainly based on the individual biomarker records, the calculation of the PIP25 index for this study could contribute to the evaluation of the applicability of this novel approach.”

We are in a position to calculate the PIP25 index data and have done so. In the submitted paper, we chose not to show these data for two reasons. First, Navarro-Rodriguez et al. (QSR, 2013) showed that this approach to semi-quantitative sea ice reconstruc-

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tion does not work well for the Barents Sea (including the study location here), for recent sea ice conditions at least. Second, the data presented here correspond to the Holocene only and we have additional data for the YD and before (not shown). Depending on which set (or subset) of the data are used, substantially different outcomes are found due to the impact on the so-called balance factor (c) used in the calculation of PIP25. Previously, Belt and Müller (QSR, 2013) (Belt an author here; Müller, the reviewer) described how this represented a potential limitation of the PIP25 index in general terms and the current study illustrates this limitation with a tangible example. Since the reviewer has raised the inclusion/discussion of the PIP25, we will briefly describe these two points in the revised paper.

“Page 4896, lines 7-10: which kind of reconstructions? Depth habitat of what?”

This concerns different reconstructions of paleo temperatures based on alkenones (e.g. Calvo et al., 2002), diatoms (e.g. Andersen et al., 2004; Birks and Koc, 2002; Koc and Janssen, 1994) and planktic foraminifera (e.g. Andersson et al., 2003; Risebrobakken et al., 2003; Sarnthein et al., 2003). The differences among them might be partly attributed to the different depth habitat of the different proxies.

“Page 4909, lines 10-12: explain possible meaning of high sterol and TOC contents”

The high sterol and TOC contents are most likely indicative of an enhanced primary production, most likely related to marginal ice zone conditions.

“Page 4912, lines 18-19: could the drop in $\delta^{13}\text{C}$ at about 8.5 ka relate to the 8 ka event?”

Based on the fact that this drop in $\delta^{13}\text{C}$ is only reflected by one data point and further at this timing no changes are observed by the other presented proxies such as sSST and $\delta^{18}\text{O}$, it is rather hard to attribute this single observation to the 8 ka event.

However, all the remaining minor issues, such as language corrections and references, will be addressed accordingly, unclear concepts within the introduction as identified by

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the referee (p.4895-4896) will be clarified and we are happy to make the data available for other scientists.

Interactive comment on Clim. Past Discuss., 9, 4893, 2013.

CPD

9, C2268–C2272, 2013

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