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Climate of the Past Discussions

Interactive comment on "Large spatial variations in coastal ^{14}C reservoir age – a case study from the Baltic Sea" *by* B. C. Lougheed et al.

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We thank the reviewers (Philippa Ascough and William Austin) for providing timely, thorough and knowledgeable reviews of the discussion manuscript. These reviews have highlighted some points within the manuscript that must be better and more cautiously communicated to the reader and will serve to improve the manuscript.

We first address points raised by Philippa Ascough:

Regarding those samples which we have considered as affected by hard-water (i.e. 14C age > 500 14C yrs), it is correct that we were unfortunately not able to quantify the hard-water effect through isotopic analysis. To our knowledge, this has not been achieved in previous studies either. Philippa Ascough makes a good point that

δ13Caragonite could probably not be expected to be a hard-water indicator, seeing as carbonate DIC and marine DIC have similar δ13C values. We will therefore change the manuscript to remove any suggestions of δ13C being an expected possible hard-water indicator of some kind. We have, however, consulted geological bedrock maps from the nations surrounding the Baltic Sea and found that five of the six samples with a 14C age > 500 14C yr were in close proximity to coastlines bearing carbonate bedrock and/or carbonate aquifers. Terrestrial runoff in these locations can therefore be expected to contribute DIC with depleted 14C content for these five samples. The sixth sample with high 14C age is from a deep area of the Baltic where laminated sediments (i.e. sapropels) are common, whereby a lack of bioturbation allows the build-up of layers of old (organic) carbon, which can contribute older DIC during oxic periods when molluscs thrive.

Philippa Ascough is furthermore correct in saying that the geographical distribution of hard-water may affect the quality of our R(t) vs salinity relationship, even in the non-coastal areas. We discussed this possibility in the manuscript when we pointed out that the R(t) vs salinity relationship in the northernmost basins (where there is almost no carbonate bedrock) is much more robust than in the southern basins, where carbonate bedrock is more common. This increased hard-water variability is reflected in the lower correlation coefficient and larger 1σ confidence limits in the case of the southern basins, which we feel realistically takes the increased hard-water variability into account. We do not claim that hard-water variability can be totally ruled out for the non-coastal samples and we will make this clearer.

Philippa Ascough points out that Macoma is an infaunal feeder. It uses ambient seawater DIC to biomineralise shells. Clearly, like any other biomineralising organism, the Macoma mollusc will also use 14C depleted DIC if it is present, hence they are not immune to being affected by a hard-water effect, and we do not argue otherwise in the manuscript. Other types of feeders can also take old 14C, seeing as 14C depleted carbon enters the base of the marine food chain through algae and phytoplankton which

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also consume 14C depleted seawater DIC. We consider that the Macoma molluscs offer a superior spatial reconstruction of R(t), as they are more likely to reflect local R(t) than higher order organisms. Furthermore, Macoma is a very common mollusc in the Baltic Sea, and therefore a useful 14C dating tool for Baltic Sea researchers.

We address the following point made by both Philippa Ascough and William Austin:

Both reviewers point out that δ18Oaragonite has not been corrected for temperature fractionation effects to produce δ18Owater. δ18Owater would of course be expected to have a much better correlation with salinity and would therefore, in turn, serve as a better proxy for R(t). We did attempt to correct for fractionation following the equilibrium equations for aragonite producing molluscs (Grossman and Ku, 1986), but this exercise did not lead to an increased correlation coefficient with salinity. Due to the presence of a seasonal thermocline in the Baltic Sea, there can be large intra-annual water temperature variations and it is difficult to determine in which season and, therefore, at what temperature, the molluscs originally biomineralised aragonite. Also, the 14C dating method requires multiple growth rings for analysis. We tried to correct for temperature fractionation effects using winter, summer and annual temperature, but to no avail. Clearly, the fact that we attempted this correction should be mentioned in the manuscript for the benefit of the reader, and the temperature data from the supplementary information should be included in Table 1, as William Austin suggests. We would point out, however, that δ18Oaragonite shows a statistically significant correlation with salinity even when it is not corrected for temperature fractionation effects, and this fact in itself shows that salinity is dominating the δ18Oaragonite signal (Figure 3C). So we still propose that δ18Oaragonite could be used as a palaeo-R(t) estimator, especially when one considers that there is currently an absence of alternative methods for the Baltic Sea. William Austin is correct that caution should be urged at this point and that researchers should use sufficiently large uncertainty estimates for palaeo-R(t) values derived in this way. However, our paleo-R(t) estimation is still a vast improvement on the current situation, whereby Baltic Sea researchers use "best-guess" R(t) values with no inclusion of error uncertainties and often little consideration for spatial and/or temporal R(t) changes.

We once again thank both reviewers for taking to the time to read through the manuscript and will use their comments to make significant improvements to the manuscript.

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Interactive Comment