



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-1212-RC1>, 2022
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Comment on egusphere-2022-1212

Robert F. Spielhagen (Referee)

Referee comment on "A 600 kyr reconstruction of deep Arctic seawater $\delta^{18}\text{O}$ from benthic foraminiferal $\delta^{18}\text{O}$ and ostracode Mg/Ca paleothermometry" by Jesse R. Farmer et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-1212-RC1>, 2022

Farmer et al. present a collection of benthic oxygen isotope data from Arctic deep-sea sediment cores, derived from benthic foraminifers. The data are partly new, but several data sets have been published in the last decades. The authors use these data to calculate past seawater $d_{18}\text{O}$, taking into account past changes in global ice volume and bottom water temperature. For the latter they use Mg/Ca data, which are a reliable paleothermometer. The major result is that glacial periods of the last 600 kyr often saw significantly lower values of $d_{18}\text{O}$ at the sea floor than interglacials. The authors then discuss possible explanations for this observation and rule out that the Arctic Ocean was filled with freshwater during recent glacials, as proposed by Geibert et al. (2021, Nature). Instead they suggest intensive brine formation during glacials as a process which could have led to a downslope transport of dense, low- $d_{18}\text{O}$ waters.

The manuscript is written in excellent English; it is well structured and the figures are illustrative. I suggest to add a table with details on core numbers, geographical coordinates and water depths. This table may be added as a supplement. The reader should not be forced to look up all the core details in various papers. There are some vertical differences in temperature even in the deeper Arctic Ocean and since these difference may affect the $d_{18}\text{O}$ in carbonates, water depths of individual cores (e.g., those used for Fig. 2) are critical information (even though there is a temperature correction from Mg/Ca data in the $d_{18}\text{O}$ data sets).

Overall, I find this manuscript already in a very mature condition. The Abstract is informative, as is the Introduction which gives various aspects of background information. One or two sentences on the research question(s) tackled in this manuscript would be helpful for the reader to better understand what the authors are aiming at. The work performed is nicely described in the last paragraph of the Introduction, but I guess the authors started with a research question before they performed the data acquisition and collection.

The second chapter gives further background information on the various factors controlling oxygen isotopes in seawater and on the modern oceanographic situation in the

Arctic Ocean. All necessary details are presented.

The chapter on Materials and Methods supplies details on the measurements performed, on the core chronology, and on the calculation of $\delta^{18}\text{O}$ of paleo-seawater. The subchapters give all necessary details in a concise manner. In particular I like the subchapter on chronology which clearly states some of the current problems with assigning definite ages to interglacial sediments older than 200 ka. Although the U/Th-based age model put forward by Hillaire-Marcel et al. (2017) is at odds with the "conventional" age model of, e.g., Jakobsson et al., 2000, Spielhagen et al., 2004; O'Regan et al., 2008, 2020, it should still be mentioned.

In the Discussion the authors argue that uncertainties with the chronology (including possible hiatuses like in the LGM) make it problematic to discuss individual periods with $\delta^{18}\text{O}$ minima beyond MIS 6. In the following subchapters they almost entirely concentrate on possible explanations for low glacial $\delta^{18}\text{O}$ of seawater and how these explanations can or cannot be reconciled with the Geibert et al. (2021) hypothesis. While I find the arguments sound and the debate highly interesting, I think the authors miss a chance to comment also on paleoenvironments in previous glaciations. I fully acknowledge that the authors want to be cautious with age assignments beyond MIS 6, but still they should discuss to some greater extent than at present the time back to MIS 15 for which they have collected a nice data set shown in Fig. 4. If they do not do this, one may ask why data from MIS 7-15 are shown at all and why $\delta^{18}\text{O}$ differences between individual stages and substages are laid out in detail in subchapter 4.2.

The debate on a possible "fresh glacial Arctic" explanation for the low glacial $\delta^{18}\text{O}$ of seawater makes up most of the Discussion chapter. I find the arguments given highly plausible, but I have to admit that I am somewhat biased against the Geibert et al. (2021) hypothesis, as demonstrated in our comment on that paper (Spielhagen et al., 2022, Nature). Nevertheless, I think that Farmer et al. have done a good job in collecting various other data speaking against a "fresh glacial Arctic" and discussing these in depth so that their own explanation for low glacial $\delta^{18}\text{O}$ data is left as the most plausible hypothesis.

This explanation is described in the last subchapter of the Discussion (5.2). The authors propose a weakened glacial stratification in the glacial oceans as the most likely cause for low $\delta^{18}\text{O}$ of intermediate to deep waters. They explicitly mention "enhanced vertical mixing" and "the transport of low- $\delta^{18}\text{O}_{\text{sw}}$ brines to intermediate depths". While the latter seems sufficiently clear, considering the previous discussion of ϵ_{Nd} values in subchapter 5.1, there is no statement on what may have caused "enhanced vertical mixing" and how and where this may have happened in an ice-covered ocean. This needs to be made clear - otherwise it will remain a "black box" for the readers. I also suggest to include a figure (cartoon) showing the proposed scenario for brine formation.

The Conclusion chapter nicely summarizes the major findings discussed in the previous chapter. It ends with some comments on the suitability of benthic $\delta^{18}\text{O}$ data and ostracode Mg/Ca paleothermometry for future paleoclimate research in the Arctic. In my

(not necessarily correct) opinion, the latter point should have been tackled with some pros and cons already in chapter 5 and not just as the last sentences of the manuscript.

Specific comments by line numbers

93: Actually, the isotopic change during the transition from sea water to sea ice is only very minor (fractionation factor ~ 1.003 ; <https://doi.org/10.3189/S0022143000042751>) and can be neglected when isotopic changes on glacial-interglacial scales are discussed. However, the d_{18O} /salinity relation in ocean waters can be strongly affected. This can lead to density changes and the sinking of low- d_{18O} near-surface waters to greater depths. Considering these details, the statement in line 93 is too much simplified.

109: What is "AL"?

124: color bar

144: analysis. Analyses

185-186: Since the default R is 550 yr in Marine20 (and was 402 yr in Marine13), it might be worth mentioning the R used in this study.

186: Blaauw (to be corrected also in the list of references!)

192: *Bulimina aculeata*

200: Lomonosov Ridge

282: A temperature of -0.3°C may be correct for intermediate waters (AIW), but deeper waters are colder (-0.9°C ; see line 106).

284: Temperatures are numbers and cannot be "warmer", only higher. Check for other usage of "cooler" and "warmer" BWTs throughout the manuscript!

289-291: I suggest to discuss the differences going from 50 ka to present and not vice versa.

346 (and 437): I would regard 7b and 13b as interstadials within MIS 7 and 13, comparable to MIS 5d and 5b within MIS 5. Please note that 5b (which was globally just a colder interval within MIS 5) saw one of the largest glaciations over northern Eurasia in the last 200 ky (Svendsen et al., 2004, QSR).

375: Since freshwater is buoyant, wouldn't it make more sense to say that the Arctic Ocean may have been filled with freshwater DOWN to 2500 m water depth? It certainly depends on the point of view, but I would understand "up to 2500 m" as the description of a bottom water mass.

451: Spielhagen

455: timescales???

470: Moreover, there were no large ice sheets on northern Eurasia during MIS 5c (Svendsen et al., 2004, QSR) to produce large amounts of brines.

519-521: This is correct. However, there is a recent paper (Rogge et al., 2022, <https://doi.org/10.1038/s41561-022-01069-z>) showing the formation of plumes which are sinking down to 1200 m even under modern conditions. During glaciations with (partly) ice-covered shelves and strong erosion, conditions allowing the formation of sediment-laden plumes may have occurred even more frequently than during interglacials. Since the Arctic deep-sea basins (>2500 m) are filled with fine-grained sediments (plumites, turbidites; see Goldstein, 1983, DOI: 10.1007/978-1-4613-3793-5_9; Svindland and Vorren, 2002, [https://doi.org/10.1016/S0025-3227\(02\)00197-4](https://doi.org/10.1016/S0025-3227(02)00197-4)), such plume formation may have been a major process for the vertical (and then horizontal) transport of both fines and low-d18O waters during glacials. Maybe the authors want to consider this possibility...

Figures: When figures consist of several "subfigures" (e.g., data panels), they are labeled A, B, C... In the text, they are referenced as a, b, c...