

Response to Robert F. Spielhagen

We would like to take this opportunity to thank Robert F. Spielhagen for his helpful comments on our manuscript. Below we provide a detailed response to his comments (in italics), indicating the changes that have been made. Line numbers refer to those of the revised manuscript that includes all tracked changes.

With kind regards,

Mattia Greco (on the behalf of all co-authors)

General comments

Planktic foraminifera of the species *Neogloboquadrina pachyderma* are a major carrier of paleoenvironmental information in Arctic and sub-Arctic marine sediments and widely used to reconstruct properties of the upper water column, namely sea-ice coverage, salinity, and temperature. The variability of their geochemical composition (e.g., stable isotopes, Mg/Ca) can be very large within an investigated sediment core. In particular to interpret this variability it is important to know which factors may determine the habitat depth of the species. A number of studies have determined the depth distribution of *N. pachyderma* under a large variety of boundary conditions (oceanic parameters). A synoptic study involving all the available depth distribution data plus oceanic data from the same sites was much needed but still lacking. The manuscript by Greco et al. fills this gap with a statistical evaluation of mostly published data from the Arctic Ocean and its neighboring seas, amended by some new data from Baffin Bay and results from a numerical model. The combination of biological, physical oceanographic, biochemistry and modelling data results in a novel approach to determine the habitat depth of *N. pachyderma* on a larger scale and is thematically well suited for the journal *Biogeosciences*. It is well-written in very good English. The structure of the manuscript follows standard principles of scientific publications. The abstract gives a good overview of the topic, the methodological approach, the major results and the main conclusions. The Introduction chapter gives a good overview of the present knowledge and thereby manifests the problem of defining the factor(s) determining the depth habitat of *N. pachyderma*. It also describes the general approach applied here and which environmental factors are considered as potentially responsible for the variability in habitat depth. The Material and Methods chapter describes in detail the origin of the data sets used for the following evaluation, the methods to obtain the new data from Baffin Bay, and the statistical methods applied to evaluate and weight the environmental factors determining the habitat depth. I am not an expert on such statistical methods and can therefore not evaluate whether proper attention has been paid to significance levels. The Results chapter lists briefly but in sufficient detail the major outcome of the statistical evaluations, in particular the correlation of habitat depth to individual and combined environmental parameters and how the results from statistical evaluations compare to the model results. The Discussion chapter puts the results of statistical evaluations in context and elaborates which environmental factors are determining the habitat depth. The outcome is discussed with respect to previous hypotheses on which parameters have forced *N. pachyderma* to live shallower or deeper in the water column. Interestingly, some of these published hypotheses (which in most cases were based on regional studies) are not supported by the conclusions of Greco et al.. The very large data base of the present study (I cannot see that relevant published data sets were left out) is the advantage of the present study and adds significantly to the credibility of the conclusions presented here. To me, the discussion appears to the point and overall sound, and I cannot see that systematic errors may bias the conclusions. These are compiled in the Conclusion chapter which lists the major findings but also open questions which may trigger further research in this field. The figures and tables are mostly clear and easy to understand (see comments below for minor exceptions). Overall, I think this paper is already in a mature state and does not need significant changes. Publication in *Biogeosciences* is recommended after some minor revision in response to the points listed below.

Specific comments to the authors

Title and manuscript text: Regarding the use of "planktonic" (instead of "planktic") in this manuscript I suggest to read the advice of the Godfather of Paleooceanography, Cesare Emiliani, which can be found here: <https://www.cambridge.org/core/journals/journalof-paleontology/article/plankticplanktonic-nekticnektonicbenthicbenthonic/CDF06242F0F9130B7A5A082DFDDFC425>

We agree with the reviewer that the correct expression would be "planktic" as explained in the referred paper. However, the expression "planktonic foraminifera" is more common in the literature than "planktic foraminifera" (up to about 5 times more- as a quick search in Scopus revealed). For practical reasons we therefore prefer to keep using the term "planktonic".

You use both "habitat depth" and "depth habitat" in the manuscript. The latter is defined on page 5 (line 17ff), the first not. Do these terms have different meanings? If yes, you should explain this. I note that even at the end of the manuscript, in the Conclusion chapter, you still use both terms (page 10, lines 17/18). That is confusing!

Reviewer 1 also pointed out that we used these two terms interchangeably in the manuscript. We will correct and homogenize the terminology adopting only the term "depth habitat".

It will be helpful for the reader to receive a bit more information on the PLAFOM2.0 model. As it stands, we just learn that it can predict the seasonal and vertical habitat of *N. pachyderma*. For those readers who have not studied the Kretschmer et al. (2018) paper in detail, you should use 2-3 lines to explain what the model is based on and which boundary conditions are used.

We added the following information about PLAFOM2.0 in the text:

*'This model is driven by temperature, food concentration, and light availability (which matters only for species with symbionts). The species-specific food concentrations are simulated by the Community Earth System Model, version 1.2.2 (CESM1.2, Hurrell et al., 2013) at every time step and are subsequently used by PLAFOM2.0 to calculate the monthly carbon concentration of *N. pachyderma* and four other species of planktonic foraminifera.'*

Many readers may not be acquainted with all the statistical parameters applied to determine correlations, anticorrelations, significance limits etc.. Those terms used widely throughout the manuscript (e.g., r, p, R, F-test, t-test) should be explained in the manuscript, including a comment on what higher or lower values mean.

We will add some explanatory notes at page 6 lines 14-16. However, these are all standard statistical concepts that we feel should be familiar to readership of Biogeosciences and we have hence not provided a detailed explanation.

I suggest not to mix British and American spelling. Either use "paleo" and "...ize" or "palaeo" and "...ise". Please check the entire manuscript for other language cases (e.g., "metres" vs. "meters").

We will check the manuscript and correct the language inconsistencies.

Comments by page and line numbers (page/line)

2/1: Better write "dominant plankt(on)ic foraminifer species"

Done

2/8: Arctic and North Atlantic oceans

Done.

2/10: Here and in other places you mention "chlorophyll a", later you also just write "chlorophyll". Be precise in what you mean.

We re-checked the usage of the two expressions and specified the data we refer to in the method section.

2/23-24: "When the organism dies, its calcite shells sink to the seafloor and when preserved in the sediments, they serve ..." Do not mix singular and plural.

Corrected.

3/10-14: You are discussing the issue of diel vertical migration again on page 7, lines 18ff, largely repeating what is said here. I suggest to shorten this part in the introduction and put the discussion where it belongs.

*We think that this part is important to highlight why there is still a need for more analyses on the influence of DVM on the DH of *N. pachyderma* even in the presence of previous investigations.*

3/22: drivers

Done.

3/22-26: Very long sentence, hard to read. Split it into two.

Done.

4/17: with a conductivity

Done.

4/18: obtain vertical profiles

Done.

4/19: for all stations

Done.

4/20: chlorophyll a concentration profiles

Done.

4/21: from the PANGAEA

Done.

5/1: all stations

Done.

5/2: time of collection

Done.

5/11: related to SST

Done.

6/23: neither in the complete data

Done.

8/21: relationship between DH and environmental parameters

Done.

8/21-23: Three sentences starting with "This...". Maybe rephrase?

Done.

8/25ff: Better write "In the model, this overestimation of the MLD affects..."

Done.

8/34: matter

Done.

9/1: depth of

Done.

9/2 sea surface

Done.

9/6: tolerance limit

Done.

9/5-7: Split long sentence into two.

Done.

10/2-3: ...evidence ... indicates

Done.

10/6-7: compromise between ... and ...

Done.

10/22-25: Split long sentence into two.

Done.

10/27: mismatch in the

Done.

11/6: gratefully acknowledged

Done.

12/3-4: Delete blanks!

Done.

12/10: ocean

Done.

12/13: Carstens, J.

Done.

12/13: Sarnthein

Done.

13/4: Delete "(Ehrenberg 1861)"

Done.

16: A word is missing in the table caption!

Done.

Fig. 2c/d: Several symbols are hidden. Possibly use open symbols with no filling?

We have tried this solution, but even though some symbols overlap we think that the original figure with transparent symbols is clearest and prefer to keep it. It is important to note that the purpose of the figure is not primarily to show each individual point, but the overall absence of a linear trend between sea ice concentration and sea surface temperature, which allows us to investigate their effect on DH independently.

Fig. 9: Why is "Productivity" related to a filled symbol in the legend while the triangle is open in the figure?

Corrected.