

## Response to Antje Voelker

*We would like to thank Antje Voelker for her helpful comments on our manuscript. Below we provide a detailed response to her 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)*

Greco and co-authors compiled new and published vertical abundance data from multi-net tows to evaluate – using statistical approaches – the habitat depth of polar foraminifera *N. pachyderma* and its relationship to environmental parameters. The study provides new and important insights into a species widely used in paleoceanographic reconstructions, but still with limited information on its living conditions. The authors compare their evidence also to the outcome of the PLAFOM2.0 model (with limited success). With the environmental changes currently occurring in the subpolar North Atlantic and Arctic Ocean this study is for sure timely and relevant for any future studies. The manuscript is well written, the data well presented and deserves to be published in Biogeosciences after minor revision. The following are more general comments that might help improve the manuscript, but are not essential for accepting the manuscript:

1) There exists a very nice study (PhD thesis) [in German] on "The planktonic foraminifera *Neogloboquadrina pachyderma* (Ehrenberg) in the Weddell Sea, Antarctica" by Doris Berberich published as *Berichte zur Polarforschung* 195, in 1996. Although this is a different genotype than in the northern hemisphere, it seems that some aspects of the Greco et al. and Berberich observations are similar. So I urge the authors to have a look at this work. I do not know, if the authors could verify with their data if the deeper depth habitat in their data is also related to more adult/ terminal stage specimens and thus potentially to the reproduction cycle. Berberich is also discussing influence of phytoplankton abundance (i.e., food supply) on the foraminifera abundance and sees similar changes in depth as discussed on p. 9 lines 17 to 30. She is referring to Arikawa (1983) when discussing the relationship between *N. pachyderma* abundance and the deep chlorophyll *a* maximum. So the Arikawa study is another one the current authors should look into as support for their observation that the depth habitat of their genotype of *N. pachyderma* appears to be below the chlorophyll maximum. Arikawa, R. (1983), Distribution and taxonomy of *Globigerina pachyderma* (Ehrenberg) off the Sanriku Coast, Northeast Honshu, Japan. *Tohoku Univ. Sci. Repts., Ser. 2 (Geol.)*, 53, p. 103-157

*Thank you for these suggestions. Our dataset does not allow for an extensive investigation of differences in DH among size classes of *N. pachyderma*. For the limited number of stations where we have size data, we found that specimens of smaller size show a deeper depth habitat than the bigger ones. Our subset thus shows the opposite pattern reported by Berberich's. Whether this pattern is real or not requires more detailed size distribution data that we currently do not have.*

*We also thank the reviewer to point us at the interesting work of Arikawa. In his paper, he describes ecological and taxonomical features of the pacific genotype of *N. pachyderma* (Type VII) collected with horizontal tows from two stations off the Sanriku Coast (Japan). At page 113, lines 1 to 9, Arikawa discusses the general distribution pattern of the planktonic foraminifera concentrations:*

*'At each station, the population density of each species in water columns broadly corresponds to the value of chlorophyll *a*, which indicates a standing crop of phytoplankton, producer. At both stations, the shallower maximum exists at a depth of 50m and 75m (around or just below the maximum of chlorophyll *a* concentration).'*

*Our data on type I shows a different pattern and indicates no link between DH and the DCM (Figs 5a and 9). These contrasting observations suggest distinct ecological patterns among the different*

genotypes of *N. pachyderma*. However, our study aims to explore the DH of genotype I and we think that a detailed comparison with all other genotypes would require more data and we hence refrain from a rather ad-hoc comparison. We consider this a potential avenue for future research.

2) p. 4 line 29: did the authors inquire at the AWI oceanography group if the CTD data collected during the ARK campaigns might have been stored there? Since I participated in ARK-X/2, I verified the cruise report and it clearly says on page 95 that at most stations with plankton sampling hydrographic information was obtained with a CTD probe.

*We queried PANGAEA data repository to retrieve the CTD data from the ARK X-2 station and did not find the data for this station. However, we used the counts from that station to investigate the effects of lunar day and DVM on the DH of N. pachyderma.*

More detailed comments to the manuscript itself:

1) throughout the manuscript you are referring to the North Atlantic, even though your samples are actually limited to the subpolar and polar regions of the North Atlantic. If you do not want to use the term Nordic Seas (for the area between Iceland, Greenland, Norway and Svalbard), you could use "northern North Atlantic" to better describe the geographical range of your samples.

*We follow the suggestion and will refer to our sampling area using more precise geographical ranges.*

2) p. 3 line 26: why is food source/supply not mentioned here -although one could argue that this could be a consequence of the change in the environmental conditions?

*We mention the food source at page 3 line 23 where we introduce the environmental drivers of the depth habitat of N. pachyderma.*

3) Material: please provide a table with the stations, date/ year of collection, data source for published data. From your figures one can deduce the season etc., but not how the samples are distributed over the years. Please also provide the name of the station excluded from the Jensen (1998) data set.

*Reviewer 1 also suggested to implement the table in the manuscript. However, due to the size of such a table we prefer to make it available as supplementary material at zenodo.org, where long-term storage is guaranteed. We would like to point out that the link to the table with the complete metadata and environmental data was already provided in the "Data availability" section in the original manuscript. .*

4) p. 4 line 18: please provide the depth until which pigment concentrations were measured. Were the profiles also done down to 300 m?

*We will provide the range of the measurement of the pigment concentrations. The submersible fluorospectrometer recorded data from the surface until 300 m.*

5) p. 5 line 11: small English correction; it should say "related to"

*Done.*

6) p. 7 line 15: it would be good if you could provide the reader with the information how and in which geographical resolution sea ice and chlorophyll are presented in the earth system model, from which PLAFOM2.0 derives its environmental conditions. I wonder if the poor relationship between observations and model might be a resolution problem or sea ice itself not being presented in the model.

*The disagreement is not due to sea ice not being modelled. The here analysed model simulation was an ocean-ice-only simulation of CESM1.2 with active ocean biogeochemistry, whereby the ocean model was coupled to the sea ice model. However, the reviewer rightly points out that the coarse resolution of climate models is often a challenge in model-data comparison. Here, both the ocean*

*component and the sea ice component of CESM1.2 have a zonal resolution of 1° and an increased meridional resolution of ~0.3° near the Equator. However, here we avoid this complication of the relatively coarse resolution of the model by simply looking at modelled versus observed relationships between DH and environmental variables. We will add the following explanation to the text:*

*'By comparing modelled with observed ecological patterns, rather than individual observations, we ensure a more consistent evaluation of the model performance.'*

*We will also make it more explicit that Fig.8 compares modelled DH with modelled sea ice and chlorophyll a concentration. The comparison is thus entirely in model space and not a strict data-model comparison.*

7) p. 9 line 14: if the authors would like to include a study more concentrated on isotopic evidence from the Arctic Ocean they could add the following reference: could also look into Hillaire-Marcel, C., 2011. Foraminifera isotopic records: : with special attention to high northern latitudes and the impact of sea-ice distillation processes. IOP Conference Series: Earth and Environmental Science 14, doi:10.1088/1755-1315/14/1/012009

*Done.*

8) p. 9 line 30: although the authors write on p 10 line 18 that the species is likely not grazing on fresh phytoplankton, I wonder if type of food source might not be a driver with a preference for "fresh food" during period with a shallower DH and more refracted organic matter during periods when the species prefers the depths below the chlorophyll maximum.

*This is an interesting point. In our data, we observe that the depth habitat is in virtually all cases located below the chlorophyll maximum (Fig. 9), thus *N. pachyderma* consistently feeds below the DCM, where fresh phytoplankton is rare. Hence, we do not observe indications for food source as a driver of DH. Assuming that *N. pachyderma* has specific food preferences, such proposed change in its diet also seems unlikely. However, we agree with the reviewer that more investigations on the diet of this species are needed for a better understanding of its ecology.*