

Interactive comment on “Population dynamics of modern planktonic foraminifera in the western Barents Sea” by Julie Meilland et al.

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Authors: We thank the Referee#1 for taking time to review our manuscript and appreciate the valuable comments and suggestions. We have addressed the comments in the following sections and in the revised manuscript:

This paper compare plankton town and sediment cores to gain insight in the PF population dynamics in the Western Barent Sea. Protein analysis is also performed on foraminifera test as proxy of metabolism. The authors can cut few sentences in the introduction which will be benefit in fluency.

Authors: We followed reviewer’s comment and modified the introduction accordingly.

Some methodology information are within the results session and need to be re-

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moved/merged within the methods session. Also it is not clear wherever or not Chla and phytoplankton data are new data or already published in another paper. This clarification will imply some change within over the text.

Authors: We followed reviewer’s suggestions and re-organized the text accordingly.

Most important, the discussion presents several strong statements which need to be better supported to reduce the amount of speculation. The conclusion have to be re-write in order to be a critical synthesis of the paper and not just a summary of the paper. Overall, the combination of data is interesting and the paper merit a publication on this journal but after medium revision. Please refers to specific comments for more details.

Authors: some of the statements in the discussion have been toned down and more details were added when needed. We re-arranged and shortened the conclusions.

INTRODUCTION Lines 40-44 This part in not fluent and need to be reorganise/shorted. The authors first talk about phytoplankton compositions, then they list calcifying organisms (including zooplankton). There is also no need to highlight the non-calcifying organisms since it not help the reader to focus on the main question the paper want to address.

Authors: We agree with the reviewer and we shortened and re-wrote this part.

Line 49-51 As for the previous comment, there is no need to add more information about fish community. This sentence reduce the fluency of the paper. I suggest to delete it.

Authors: The sentence has been delete.

Line 56: before to use the abbreviation PF, the author need to identify what this means. Planktonic foraminifera (PF). Please be consistent over the all text.

Authors: Corrections were made.

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Line 71-74: Move the sentence “planktonic foraminifera..indicator..changing environments” before the sentence “ more studies on living. . .ecological preferences”

Authors: We changed sentences order.

Line 75-76: This information appears in the text 3 time: introduction, methods and acknowledgment. Please remove from the introduction.

Authors: Done

Line 79: What living fauna is referring to? I assume PF but wrote in this way looks like the full zooplankton assemblage.

Authors: We amended the sentence according to the reviewer’s comment.

Line 80-83 This sentence need to be re-write because it is a bit confused as it is presented. LPF individual protein are investigated from net samples. However here seems like protein have been analysed also in core samples. The author need also to take more effort in describe why it is relevant to do this study in the Barent Sea and why it is relevant to do protein analysis. In other word the author need to work a bit more on how to “set the scene”

Authors: The sentence has been re-written and more details given about the relevance of protein quantification. Protein represents a large part of zooplankton organic carbon composition and could provide crucial informations on individuals’ food availability, uptake and ecological strategy. Doing these measurements in our study area is extremely relevant as 1) the studied latitudinal transect allows to observe a wide range of S and T°C and thus potential adaptation of foraminifera and 2) as no data are available in the region.

Methodology 3.1 My understanding is that phytoplankton analysis (pigment and composition) are coming from a previous study. If it is this the case, the authors should not include this information in the method and neither in the results.

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Authors: All information relative to environmental parameters were shifted to the “oceanographic setting” section of the paper.

Line 110 I am aware the fraction smaller than 63micron can be relatively low, however the author should acknowledge somehow the decision to use 100micron instead 63micron.

Authors: The sampling occurred in summer/fall 2014 when phytoplankton blooms are known to occur. To avoid clogging in the nets and because it is a very standard mesh-size globally (in mid- low- latitudes), we choose to use a mesh size of 100 μm . We acknowledged this decision in the manuscript.

3.3 Not quite understand the reason to have a different head line. 3.3 is presenting analysis of protein from forams collected in the net. It is much more fluent to have only 3 headlines in the methodology i-hydrological environmental collection, ii-town, iii-core. So in this case will be sufficient just to merge 3.2 with 3.3.

Authors: We agree with the reviewer and merged sections together.

Results 4.1 As for my previous comments this session have to be removed since it is not a result of Meilland et al. I understand that the author will use this data to compare with forams data. This is fine but need to be part of the discussion only.

Authors: This section has been shifted under “Oceanographic settings”.

177-180: This information need to be moved/merged in the methods and the reason of the selection of the 2 transect have to be clarify better.

Authors: We understand the reviewer’s comment however this short paragraph is used here to help the reader and we wish to keep it there. The choice of the two studied transect is now justified in the Material and Methods section.

Line 178: Does the author means total and relative abundance? Please be consistent along the text with the terminology

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Authors: We make the distinction between absolute abundance (ind. m⁻³) and relative abundance (%). We don't mention total abundances in the manuscript or when we do it is to refer to all species together.

Line 208-214: Most of this information need also to be moved/merged in the methods

Authors: The fact that protein measurements were successful on 272 specimens is already a result and we think it belongs to this part. The complementary information of this paragraph are here to help reader going through this section.

Line 225-228: as for previous comment please move/merge with methods

Authors: We understand the reviewer's comment however this short paragraph is used here for the context and help the reader. We would prefer to keep it there.

Discussion Line 262-263 The author investigate the possibility of the high abundance of GU as potential consequence of the climate change. This is a big statement supported only by data collected in one single shot (not time series) in the ocean. I suggest either remove or at least to acknowledge the limitation of this statement.

Authors: As suggested by the reviewer, we toned down our statement and acknowledged its limitations in the manuscript.

Also this statement is in disagreement with what the author said before (lines 246-247) about the low influence of T and S on PF density. Please clarify better.

Authors: Lines 246-247 concerns the potential influence of environmental parameters on the density of planktonic foraminifera as a population while the lines the reviewer refers to concerns the density/ecology of one species in particular: *G. uvula*. This is why we separated the two paragraphs.

What about the potential influence of net mesh size? Can the small missing fraction bias the relative abundance between species?

Authors: The influence of the used mesh size would bias results the other way around

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with more specimens of small size (*G. uvula* and *T. quinqueloba*). Therefore the fact that we used a 100 μ m mesh size only support the fact that the observation of *G. uvula* and *T. quinqueloba* in such densities is interesting.

What about timing in collection (day/night)? Are the author assuming the foraminifera do not perform diel vertical migration? If this is the case need to be supported by literature.

Authors: We indeed assume planktonic foraminifera do not perform diel vertical migration, as published by Meilland et al., 2019, cited in the manuscript.

Line 284-294 Do the author found a specific correlation between *Phaeocystis* and GU or TQ in all the stations? Also the author have to explain better why *Phaeocystis* is considered high quality food, why they should prefer it? What is the strategy diet difference between GU, TQ and NP? The author need to expand this part to better defend the statement.

Authors: In this paragraph we only speculate on the fact that *G. uvula* and *T. quinqueloba* could reflect food composition more than food availability. We do not comment on the nutritional quality of *Phaeocystis*. To answer the second part of the reviewer's comment, only little is known about in situ diet preferences for these species, especially in the studied region. It is therefore difficult to go further in our hypothesis.

Line 294 please provide literature of study which use Chl-a satellite data as indicator of foraminifer's extension production as example.

Authors: Here we say Chl-a satellite data are used for the observation of phytoplankton bloom, not necessarily as indicator of PF extension. However, several studies compare Chl-a satellite data to PF distribution it is for example the case in one of our previous publication (Meilland et al., 2016).

Line 298-316 the discussion linked to the protein results is a bit disconnected with the rest. Can protein results help the authors to drown a better picture concerning

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the relation/discrepancy between net versus cores? Despite the variability of protein concentration with the latitude is an intriguing result it does add to much to the value of the paper in the way it is included in the discussion.

Authors: Based on both reviewer's comments we provided more information about the relevance of protein analyses and re-wrote parts of this section.

Line 340 Similar to previous comment. Can the authors really speculate that a collection of a sample in a specific time can be indicative of a shift in population when compared a decade average from the sediment core? Speculation is allowed in a certain perimeter but it is very important that the authors acknowledge and clarify the limitation of their statement.

Authors: We fully agree with the referee's comment and we toned down our statement.

Conclusion The conclusion need to be reorganize and shorted. In general the conclusion should be not just a summary. To me this looks more like a summary at the end of thesis chapters than a conclusion. The authors have to provide a synthesis of the results in order to highlight the relevance of them within a big pictures. This is a relative short paper so there is no need to re call point by point (from a to f!) all the results achieved. What the author need to provide here is a critical thinking and elaboration of the most relevant MS findings and what are the new insight they bring in the marine research community.

Authors: the conclusion has been reorganized and shortened following the reviewer's suggestions.

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