

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

**Anonymous Referee #1**

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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. Some methodology information are within the results session and need to be removed/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. 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 publica-

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tion on this journal but after medium revision. Please refers to specific comments for more details: INTRODUCTION Lines 40-44 This part in not fluent and need to be re-organise/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. 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. 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. Line 71-74: Move the sentence “planktonic foraminifera..indicator..changing environments” before the sentence “ more studies on living. . .ecological preferences” Line 75-76: This information appears in the text 3 time: introduction, methods and acknowledgment. Please remove from the introduction. Line 79: What living fauna is referring to? I assume PF but wrote in this way looks like the full zooplankton assemblage. 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” 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. 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. 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. Results 4.1 As for my previous comments this session have to be removed since it is not a result

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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. 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. Line 178: Does the author means total and relative abundance? Please be consistent along the text with the terminology Line 208-214: Most of this information need also to be moved/merged in the methods Line 225-228: as for previous comment please move/merge with methods 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. 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. What about the potential influence of net mesh size? Can the small missing fraction bias the relative abundance between species? 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. 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. Line 294 please provide literature of study which use Chl-a satellite data as indicator of foraminifer's extension production as example. 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 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. 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

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is allowed in a certain perimeter but it is very important that the authors acknowledge and clarify the limitation of their statement. Conclusion The conclusion need to be re-organize 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.

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