

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

Julie Meilland et al.

jmeilland@marum.de

Received and published: 8 January 2020

Authors: We thank the Referee#2 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:

Referee#2: The paper by Meiland et al. presents a really interesting study of planktonic foraminiferal occurrence in the Barents Sea. Notably, the study includes both plankton tow and core-top samples, including Rose Bengal staining of recently deposited foraminifera. They have also included an analysis of protein biomass in their methodology, which could be an interesting complement to their observations. Overall, the study is quite interesting, however, I have some suggestions for potentially improving

[Printer-friendly version](#)

[Discussion paper](#)



analysis and presentation, which I hope the authors will consider.

Overarching comments:

1) It appears to me that one of the critical limitations of the study at presented is a lack of time constraints. The authors compare planktonic foraminifera standing stock (instantaneous), to Rose Bengal stained (integrated over weeks to... years?), and unstained core tops (integrated over decades?). The comparison between abundances across these different timescales is potentially a huge strength of the work, but it is difficult to interpret without further time constraints and/or clear discussion of these issues.

Authors: We agree with Reviewer's comment and provided additional information about the different time constraints. Based on literature and sedimentation rate in the study area (Fossile et al., 2019), we can safely say the unstained core tops represent less than a decade.

Could the authors, for example:

a. Include an estimate or discussion of what Rose Bengal stained sediment top foraminifera represent? A month? A season? A year?

Authors: We can reasonably think that the stained organisms represent the Spring/summer population that recently felt and the discussion is based on this assumption. We amended the Material and Methods accordingly to make it clear fo the reader. The sediments in the studied area are oxidised and therefore it is safe to say Rose Bengal wouldn't stain foraminifera over a long time period (i.e. a year).

b. Timing between and dates of plankton tows? It looks as if these tows were taken over the course of 6 weeks. If so, this should be made explicit, with dates included, and discussed. Especially as the authors discuss both seasonal and lunar production in some species, this is a potentially important point. Could assemblages have changes of the course of late summer to fall? Are different periods in the lunar cycle being

BGD

Interactive
comment

Printer-friendly version

Discussion paper



sampled?

Authors: Sampling dates are available in Table 1. The latitudinal transect (South to North) was sampled across 4 days only in August. We can therefore not expect a change of assemblages in the water column due to the summer/fall transition or to the lunar cycle.

c. Include a more thorough discussion of the evidence for a temperature-driven change in assemblage over the past decades? While I agree this is hypothetically plausible, given the uncertainties in timescales outlined above and the well-described seasonality and patchiness of planktonic foraminifera in tows, this is not currently a convincing line of argument based on the data.

Authors: We understand reviewer's concern and we toned down this hypothesis over the manuscript.

2) The inclusion of protein biomass measurements is a particularly interesting aspect of this work, but the results are not clearly synthesized in the discussion. For example, I'm struggling to understand how conclusion e) that planktonic foraminiferal dynamics and metabolism are decoupled, relates to the data. I'd urge the authors to be more explicit first about how protein biomass is a proxy for metabolism, and then be very specific in discussing what aspects of "dynamics" and metabolism are decoupled. My confusion may stem from lack of expertise in this area, but clarifying the importance of these findings and linking them to the conclusions can only increase the impact for a less specialized audience.

Authors: Proteins are one of the main constituents of organic carbon in living organisms and come from the food organisms consume and transform (metabolise). They support organisms' growth and give us information on how they feed: are organisms starving? Adapting to/degrading food? We can therefore suspect that for individuals of a same size a reduction in protein concentration could be the signal of a metabolism slow down. This can be explain by 1) a lack of resources, 2) unsuitable resources,

BGD

Interactive
comment

Printer-friendly version

Discussion paper



3) a global unsuitable environment, 4) a change of “behaviour” (i.e. dormance) etc. . . If foraminifera have a lower metabolism in the North of the studied area, one could expect to observe fewer individuals but our observations do not show a link between abundance of foraminifera and their protein concentration. That’s the decoupling we are talking about. We added more details about the relevance of protein quantification over the manuscript and we clarified our message on the decoupling between foraminifera dynamics (abundances) and metabolism (protein concentrations).

This comment is obviously stylistic, but I would discourage the overuse of acronyms to improve overall readability. For example there is no need to abbreviate “planktonic foraminifera” to PF. Additionally, if acronyms must be used, please avoid starting sentences with them, ie., the second sentence of the abstract.

Authors: We understand the Reviewer’s remark and amended the abstract accordingly.

17: Subfossil -> just say core top if you mean core top Authors: we made the change.

18: four same -> same four Authors: we made the change.

29: is -> are Authors: we made the change.

42: exhibit -> exhibiting Authors: we made the change.

47: is -> are Authors: we made the change.

71: no “highly” Authors: we removed it.

73: no “as” Authors: we removed it.

125: “a few” Authors: we made the correction

126: CTD -> CTD Authors: we made the correction

128: how were foraminifera cleaned? Authors: Foraminifera were cleaned with a brush and filtered sea water. We amended the text accordingly.

143: I don’t think this is correct. Rose Bengal staining should indicate the presence of

organic material, but gives no information about the presence of coloured cytoplasm. Authors: We amended the sentence accordingly “This organic stain reacting with cytoplasm was used here to distinguish PF still bearing fresh cytoplasm and thus very recently deposited from empty tests of fossil PF”

155-156: This sentence requires some clarification Authors: We agree and finally decided to remove the sentence.

Section 4.2. Can you be consistent with the significant digits on the foraminiferal relative abundances? Authors: We checked the consistency of digits and corrected them when needed.

241: where in the “South”? Authors: South has been replaced by “69.8°N”

247: to -> with Authors: We made the correction.

248-255: I am wary of the over-interpretation of these results given that they are based on single tows and the repeated observations of planktonic foraminiferal patchiness (including as discussed in this paper and in Meiland et al., 2019). Authors: We agree with the reviewer and mentioned patchiness as a plausible explanation “The low abundances at the two ends of the studied transect could reflect planktonic foraminifera patchiness pattern of distribution (Meiland et al., 2019) or highlight the fact that waters under continental influences. . .”

259: no “as” Authors: we removed it.

267: remove “best probably” Authors: we removed it.

299-301: please clarify that “size” refers to shell size, not cell size. Is it possible that part of what you are observing could be a decoupling of shell and cell sizes at high latitudes? Authors: Following the reviewer’s comment we added “test” before size. A strong enough decoupling of shell and cell sizes at high latitudes to explain our observations seems unlikely since specimens selected for protein measurements were selected on the basis of cytoplasm presence in all visible chambers. Also, none of the

[Printer-friendly version](#)[Discussion paper](#)

specimens were encrusted and the “available” space in the shell should have therefore been comparable.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-429>, 2019.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

