

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

Anonymous Referee #2

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The paper by Meilland 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 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 com-

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parison 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. 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? 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 sampled? 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.

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.

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, i.e., the second sentence of the abstract.

17: Subfossil -> just say core top if you mean core top

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18: four same -> same four

29: is -> are

42: exhibit -> exhibiting

47: is -> are

71: no “highly”

73: no “as”

125: “a few”

126: CTD -> CTD

128: how were foraminifera cleaned?

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.

155-156: This sentence requires some clarification

Section 4.2. Can you be consistent with the significant digits on the foraminiferal relative abundances?

241: where in the “South”?

247: to -> with

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).

259: no “as”

267: remove “best probably”

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?

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

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