

Interactive comment on “Oxygen and carbon isotope composition of modern planktic foraminifera and near-surface waters in the Fram Strait (Arctic Ocean) – a case-study” by T. Pados et al.

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Anonymous Referee #1

The manuscript of Pados and co-authors on ‘Oxygen and carbon isotope composition of modern planktic foraminifera and near-surface waters in the Fram Strait (Arctic Ocean) – a case-study’ is well written and presents new data on a region rather intensely studied since the 1980s. Most of the conclusions hence read quite familiar, although the absolute numbers may slightly differ from those published before.

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1. Comment: In addition some of the results presented in the manuscript repeat those just published by the same authors (Pados and Spielhagen, 2014, Polar Research). For example, figure 2 of the present manuscript resembles figure 2 in Pados and Spielhagen (2014) [idem Table 1]. The data (standing stocks) presented in figures 5 and 6 of Pados and Spielhagen (2014) are exactly the same as in the present manuscript. The authors may want to present the data (T, S, and standing stocks) in a more synthetic way, though, and add a new perspective to the same story. However, despite all redundancies and duplications, the manuscript may still merit publication, to make sure that the precious new data are not getting lost.

Authors’ general response: As mentioned in line 12-14 on page 8638, we analyzed in the above-mentioned two manuscripts (Pados and Spielhagen, 2014 and present manuscript) the same samples taken during cruise ARKXXVI/1. However, the aims of the two studies are completely different and they were meant to give a detailed description of planktic foraminiferal fauna in the sampled area from two different perspectives. Pados and Spielhagen, 2014 focuses on the ecology (faunal composition and depth distribution) of planktic foraminifera, while the present manuscript reports on the stable isotope composition of the tests and aims at discussing the effects of environmental factors on the oxygen and carbon isotope composition of the shells.

Our specific response to: [...] some of the results presented in the manuscript repeat those just published by the same authors (Pados and Spielhagen, 2014, Polar Research). For example, figure 2 of the present manuscript resembles figure 2 in Pados and Spielhagen (2014).

As the two papers are associated, the data presented in Pados and Spielhagen, 2014 holds important background information for present study as well. However, not every reader of present paper may want/will be able to download and read the other manuscript only to get an overview over the research area. Therefore, in chapter 4.2 we shortly summarized the results of Pados and Spielhagen, 2014. Moreover, we consider it absolutely necessary to give an overview of oceanic conditions (T, S) during

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sampling (Figure 2 in both manuscripts) since these data are crucial for understanding the isotope data presented and discussed.

Our specific response to: [...]The data (standing stocks) presented in figures 5 and 6 of Pados and Spielhagen (2014) are exactly the same as in the present manuscript.

Since figures 5 and 6 in the Polar Research manuscript are not shown in the BGD manuscript (and nothing similar either) we assume the reviewer means that figures 3 and 4 in the Polar Research manuscript resemble figures 5 and 6 in the BGD manuscript. However, while the first two show the species composition and the relative abundances in the samples, the latter two show standing stocks for the two species (calculation is explained in chapter 3 Materials and Methods). These figures 5 and 6 in the present manuscript aim to show the calculated calcification depths of the two species. This calcification depth is calculated from the standing stock, which therefore is important information in the present paper. Neither standing stocks nor calcification depths were shown or discussed in the Polar Research paper and thus they are new data when shown in the BDG manuscript.

2. Comment: On the scientific level, I have some concerns about the use of name and species concept of *Neogloboquadrina pachyderma*. The species concept is discussed in detail by Darling et al. (2006, *Paleoceanography*), and coiling direction may not be used to distinguish *N. pachyderma* (more left than right coiling) from *N. incompta* (more right than left coiling)[the authors may just skip '(sin.)' after '*N. pachyderma*'].

Authors' response: The reviewer is right, from the genetical point of view it is not correct to use "*Neogloboquadrina pachyderma* (sin.)" for describing a species. We are aware of the problem, however we did not have the possibility to genetically analyze our samples, we could only visually differentiate the species. Therefore we follow the suggestion of the reviewer. In the revised manuscript we have added a sentence explaining that we use the term "*Neogloboquadrina pachyderma*" (with no addition of "sin."). However, we also state that for isotopical analyses we have used left-coiling specimens

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only, which may genetically be (to a very minor amount) *N. incompta*. Considering the fact that each isotopic measurement was performed on a sample of several left-coiling specimens, we think it is still justified to use "*Neogloboquadrina pachyderma*" when discussing the results.

Changes in the manuscript: We have deleted "sin." after "*N. pachyderma*" and in the "Material and Methods" we have added a sentence explaining that we follow the species concept of Darling et al. (2006). Moreover we clarified that we used only left coiling specimens for the stable isotope analysis.

3. Comment: *T. quinqueloba* may or may not contain symbionts (page 8645, line 26 to page 8646, line 1), and which would affect the interpretation of the stable isotope signal (Hemleben et al. 1989, Ortiz et al. 1995). I would suspect that *T. quinqueloba* do not harbor symbionts at the high latitudes sample here.

Authors' response: The reviewer is right, Ortiz et al., 1995 describes *T. quinqueloba* collected in the California Current as asymbiotic. However, all studies conducted in high latitudes that we are aware of describe *T. quinqueloba* as symbiotic species (Schiebel and Hemleben, 2005, *Palaeont. Zeitschr.*; Simstich, 2003; Jensen, 1988, *Ber. SFB 313*; Hemleben et al., 1989; Be, 1977). We therefore assume that the observations of Ortiz et al. (1995) represent more the exception than the rule.

Changes in the manuscript: None.

4. Comment: I don't see how increasing river discharges at quite remote places would affect the stable isotope of the foraminifers sampled in the Fram Strait (page 8648, lines 17-23). Please explain.

Authors' response: Increasing river discharges (e.g., Peterson et al., 2002) in the last 8 decades have significantly changed the freshwater budget of the Arctic Ocean (Morrison et al., 2012, *Nature*) and resulted in a net storage of freshwater here. Accordingly, model results (e.g., Holland et al., 2006, *J. Clim.*) indicate an increased freshwater

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export through Fram Strait in the 20th and 21st century. Since this export occurs in near-surface water masses inhabited by the planktic foraminifers, the isotopic composition of waters and contained foraminifers should have changed accordingly, resulting in a reduction of $\delta^{18}\text{O}$ values.

Changes in the manuscript: We have added one sentence that explains this chain of arguments in more detail.

5. Comment: Chapter 5.3 on Carbon isotope values of DIC and foraminifera finally gives no answer on the question asked here. The idea of discussing the affect of the carbonate ion effect on the ^{13}C signal of the foraminifer test analyzed here is good, but the data are possibly not suited for an in-depth discussion. The chapter hence ends with the unsatisfactory remark that offsets might be caused by differences in age (which might be true, but does not answer the question initially asked). I would suggest reorganize and shorten the chapter.

Authors' response: The reviewer is right, indeed we cannot discuss the carbonate ion effect (CIE) very detailed because as we mention in the manuscript we did not measure the carbonate ion concentration. However, since other factors can be excluded (at least to a certain degree), we consider it appropriate to propose a possible effect of the carbonate ion concentration on the carbon isotope composition of the shells. This may help to encourage interested colleagues to investigate this effect in the working area in more detail.

Changes in the manuscript: We have added a short remark to underline the importance of carbonate ion concentration data sets. Regarding the $\delta^{13}\text{C}$ differences between fossil and modern foraminifera, we have shortened the discussion here and concentrate on a hint to the Suess effect.

6. Comment: Why not discussing differences with older data from the same region, to show an increasing (or not) effect of carbonate ion concentration on calcification at high latitudes over the past 20 years or so.

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Authors' response: Criticism accepted.

Changes in the manuscript: Assuming the validity of the hypothesis on the effect of increased carbonate ion concentration through high primary production, we included a comparison of vital effects in $\delta^{13}\text{C}$ incorporation with the results of Volkman and Mensch (2001) and Stangeew (2001).

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