

Interactive  
Comment

## ***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.***

### **Anonymous Referee #1**

Received and published: 23 July 2014

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. 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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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.

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*']. *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.

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.

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  $^{13}\text{C}$  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.

To conclude, the manuscript of Pados et al. leaves me quite unsatisfied. I would guess that the authors could do much better, and produce a focused and well argued paper

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

with the same data used here. 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.

---

Interactive comment on Biogeosciences Discuss., 11, 8635, 2014.

**BGD**

11, C3676–C3678, 2014

---

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C3678

