

Interactive
Comment

Interactive comment on “Early season mesopelagic carbon remineralization and transfer efficiency in the naturally iron-fertilized Kerguelen area” by S. H. M. Jacquet et al.

C. Jeandel (Referee)

catherine.jeandel@legos.obs-mip.fr

Received and published: 30 July 2014

Dr Jacquet and co-workers report an interesting study variability of the mesopelagic barite in excess along a broad N-S transect in the Kerguelen area, from which they deduce the zonal variability of the POC export. They also performed this study within a meander east of the Kerguelen plateau, transect allowing them to follow the temporal evolution of these oceanic parameters. They eventually compare some of their results (when possible) to earlier data obtained at the same location but at fall, which provides an insight on the seasonal variability of the Baxs distribution, POC export (EP) and their remineralization rate estimate, that they compare to primary production (PP and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EP being published in companion papers of the same issue). The set of data is of good quality, the complexity of the area made the interpretation challenging, difficulty which is honorably overcome by the authors. This work deserves to be published in Biogeosciences, but not without some improvements proposed below.

General Comments

The whole paper is based on the use of Baxs as proxy of the POC remineralization, proxy that was well described by many preceding works of the same authors and others. Indeed, the Baxs mesopelagic maxima is ubiquitous; Bishop, Dehairs and others demonstrated that micro-crystals of barite precipitates in biological microenvironments (fecal pellets, aggregates. . .) , and that the release of these micro-crystals yielding the observed maximum corresponds to the disintegration of this biological material, concomitant to the maximum consumption of oxygen, the latter being related to POC oxidation. However, this reasoning and the empirical relationship allowing relating Baxs to POC remineralization is based on one dimensional approach. The Kerguelen plateau is particularly dynamic, and several preceding works discuss (and even propose a modelling, for the Pa/Th distribution, Venchiarutti et al, 2008) the importance of the advection on the fate of Trace Elements and isotopes on the plateau and around. I am concerned by the fact that the impact of advection and internal tides is never discussed in this work. For example, the maximum of Baxs at the reference station is considered only as a remnant signal from a preceding bloom, occurring in late winter. Other works are observing maxima of LSi, particulate Fe, Mn and Al (van de Merwe, Lasbleiz et al) that could be advected from the Leclaire Rise, located 75 km north west of R-2. . . knowing that re-suspended sediments are also enriched in Baxs (as shown in this work at stations A3 for ex) why is the hypothesis of such horizontal transport not discussed here? On the plateau, what is the importance of horizontal versus vertical transport? This should be more deeply considered in the present manuscript.

Detailed comments

BGD

11, C3944–C3947, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



In the abstract, it should be explicitly written that the “mesopelagic POC remineralisation” reported here is deduced from Baxs proxy: this would be more precise -In the introduction and section 3.2, no reference is made to E. Sternberg work, who also demonstrated (with F Morel) that Baxs in the surface water is “scavenged Ba” but not immediately crystallized as barite, and brought some clues on the seasonal “rhythm” of the barite formation by studying a “short time series” in the Mediterranean Sea. This could be added.

-At the end of the introduction, I found question 1) not clearly written, please explicit better what was this first motivation.

-Sampling and analyses: neither the blanks, nor the reproducibility are given, should be added.

-In the result section, the surface maxima observed at E1 and the pic at 100 m at E4-E are not described. Could be done.

-Paragraph 4.1: the first sentence is not clear as it is written: the link between “low productivity, low export and highest DWAV is not direct, which appears to be the case at the first reading of the sentence: rephrase.

- End of the same paragraph: about the hypothesis of “recurrent winter production” that might explain the R-2 maximum. . .was such phenomena visible and already observed with the satellites?

- Discussion, station in the meander (4.3): I appreciated the evolution of the different ratios considered here, that allows following an interesting temporal evolution of the biogeochemical dynamic in this “recirculation”. Fig 5b and the related discussion would be easier to follow if the authors could add an inset to the Fig3, inset showing the full depth profile of Baxs at the stations TNS-6 and E-1 where bathypelagic processes are suspected.

-Table 1: in the date of sampling, the precision “2011” is perhaps not useful

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



-Figures: None of the figure proposes a circulation scheme, that could be helpful (in connection with my general comment)

-In the caption of fig 4: the authors could add “POC, deduced from the Baxs maxima”

Interactive comment on Biogeosciences Discuss., 11, 9035, 2014.

BGD

11, C3944–C3947, 2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C3947

