

Interactive comment on “Particulate biogenic barium tracer of mesopelagic carbon remineralization in the Mediterranean Sea (PEACETIME project)” by Stéphanie H. M. Jacquet et al.

Anonymous Referee #2

Received and published: 11 November 2020

This manuscript presents a large data set of excess particulate Ba concentrations (Baxs) in the Mediterranean Sea showing spatial variations between basins. POC remineralization rates (MR) were estimated by Baxs inventories in mesopelagic waters and compared to data of prokaryotic heterotrophic production (PHP). This contribution is a good addition to the study of oceanic Ba cycle, in particular in marginal systems. However, I found that the data interpretation needs significant improvement and justification. Some explanations and statements are vague without solid evidence.

Major Issues:

Issue #1: Using Dehairs's transfer function. This is my biggest concern. I don't think this function can be directly used in the Mediterranean Sea without restriction. I also read the manuscript of Jacquet et al. (in review), which is also under review now at Biogeosciences. In Figure 2b of that manuscript, only a single data point from the Mediterranean Sea is located on the transfer line deduced from the Southern Ocean, while the Atlantic data point is clearly off the line. In addition, as shown in Figure 4 of this manuscript, the PEACETIME data set overall does not follow the trend of the Southern Ocean. In fact, Lemaitre et al. (2018) obtained a new transfer function for the Atlantic. Consequently, it is premature to make a statement of the universal validity of the Dehairs's transfer function. To fix this issue, I suggest the authors trying to develop a new transfer function specifically for the Mediterranean Sea using the large data set of this work, following what Lemaitre et al. (2018) did for the Atlantic scenario. A secondary option is keeping using Dehairs's transfer function, but the estimated POC MR needs very careful verification to prove such application is reasonable. This is exactly my second major concern.

Issue #2: Justification of the estimated POC MR. Whether the POC MR derived from the Baxs proxy is in order lacks justification. I suggest the authors comparing MR (Figure 5 and Table 2) with export production and/or primary production in the upper water column of the Mediterranean Sea. If these data are not available in the PEACETIME project, the authors can include literature data obtained from the Mediterranean Sea or from other similar systems for discussion.

Issue #3: Hypothesis of particle injection pump. To me this hypothesis, as the major implication of this study, was proposed without context in both the abstract (Lines 26-29) and the text (Lines 249-252). I didn't follow how Baxs variations between basins reflect the functioning of particle injection pump. I suggest the authors clarifying this point with more detailed discussion.

Minor Comments:

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

Lines 66-68: van Beek et al. (2009) also reported Baxs in the Mediterranean Sea.

Lines 152&158: in the “Results” section, expand description of the vertical distribution of particulate Al/Ca/Sr.

Lines 179-182: the description here is not consistent with data shown in Table 1, please double check.

Lines 196-198 & 227-229: what’s the pattern of particulate Al and lithogenic Ba? Please be specific. “slight” means important or not important?

Lines 229-242: This part of discussion is unclear and needs reorganization. To me, the authors tended to explain two contrasting scenarios (increase and decrease in MR at two sites, respectively) using a same reason (i.e., dust input).

Line 256: what does “globally” mean?

Figures 2-3: I suggest the authors removing the data point of 2000 m to better show the Baxs maximum in the mesopelagic waters.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-271>, 2020.

Printer-friendly version

Discussion paper

