

Title: Particulate biogenic barium tracer of mesopelagic carbon remineralization in the Mediterranean Sea (PEACETIME project)  
Author(s): Stéphanie H. M. Jacquet et al. MS No.: bg-2020-271 MS type: Research article  
Special Issue: Atmospheric deposition in the low-nutrient-low-chlorophyll (LNLC) ocean: effects on marine life today and in the future (BG/ACP inter-journal SI)

#### Response to Referee #2

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.

Reply: thanks, but do you mean “marginal” in term of coastal/marge/plateau zones, impacted by convection and lithogenic /continental inputs? Or “marginal” in term of secondary type/minor system? Because the Mediterranean Sea is not marginal system. Due to limited exchanges with adjacent basins and the existence of an intense overturning circulation qualitatively resembling the global one but characterized by shorter time-scales, the Mediterranean Sea is to date considered as a laboratory to observe and understand the impact of transient climate variability on ecosystems and biogeochemical cycles. In a context of climate changes, better balancing the regional carbon budget and C storage capacity is of crucial importance in the MedSea.

However, I found that the data interpretation needs significant improvement and justification. Some explanations and statements are vague without solid evidence.

Reply: ok we'll strengthen our explanations.

#### 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.

Reply: the ms. Jacquet et al. #bg-2020-241 is now published in Biogeosciences. We show that the Dehairs function can indeed be used in the MedSea. Furthermore, in Lemaitre et al. (2008) authors clearly mention that the transfer function they obtained (from apparent oxygen utilisation divided by the water mass age) in the north Atlantic is not significantly different to that reported by Dehairs from the Southern Ocean. There is no need to develop a new function here because by testing the Ba vs JO<sub>2</sub> relationship at ANTARES we obtained very close trend. Also, time integration of AOU is less precise than direct measurements. Lemaitre was furthermore one of the reviewers of the #bg-2020-241 paper and approved our conclusion that the function is relevant in the MedSea.

We agree that efforts should be put in the ms on confronting our MR fluxes with more data and literature.

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.

Reply: indeed production, export or other surface C fluxes were not measured during the peacetime cruise. It was not in the core of the project. We'll give some range of values found in literature.

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.

Reply: ok, we'll better explain how particle injection could impact Baxs variations between basins. Briefly, this process controls the depth where remineralization of POC occurs (and subsequent barite formation). The injection process (strong convection) has been reported in literature to be particularly salient in the western basin. In a previous paper we also reported its potential impact on the dissolved Ba distribution. As the Baxs vertical distribution clearly reflects a deeper export of material in the western basin during peacetime, we formulated that the origin of this material could be the particle injection pump (and subsequent remineralization, barite formation, etc...). We'll better discuss it in the ms.

Minor Comments:

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

Reply: added. Note that it is in part the same data set as published in Sternberg et al., 2008- but discussed in the light of Ra/Ba ratios.

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

Reply: added in "results" and "discussion 4.1" sections.

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

Reply: ok modified

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

Reply: added. A low (to very low) lithogenic contribution does not exceed 20%.

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).

Reply: the decreased (lower intensity) and upper-mesopelagic restricted layer MR is potentially due to the dust input at station #Tyr. The impact is not supported by Al data at Fast station. We modified and clarified the discussion.

Line 256: what does "globally" mean?

Reply: nothing ☺ – we removed it

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

Reply: it is important to keep it for data presentation and to see how concentrations decrease to the background. The mesopelagic maximum is enough salient in Fig2&3.