

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 #1

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Jacquet et al. present mesopelagic particulate organic carbon (POC) remineralisation fluxes in three different basins of the Mediterranean Sea using excess Barium (Baxs) as a proxy. This tracer, through the transfer function relating Baxs to oxygen consumption, has been successfully used in different regions of the World Ocean (Southern Ocean, North Atlantic, North Pacific) and has also been shown to be relevant in the Mediterranean Sea (Jacquet et al., in reviews). The study reveals interesting basinal variations in the magnitude of mesopelagic remineralisation (MR). Based on Baxs concentrations, greater 100-1000m MR fluxes are determined in the western basin

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(Algerian basin) compared to the Tyrrhenian and Ionian basins. This greater deep remineralisation could be due to different processes such as 1) a strong convection in the western basin leading to a strong particle injection pump, or 2) dust deposition event in the eastern basins leading to a more efficient export of particles, thereby escaping mesopelagic remineralisation. Overall the data is interesting and necessary for better understanding the biological carbon pump. The two possible explanations of greater MR fluxes (lack of dust depositions, water mass convection) are also interesting, but are unfortunately not detailed enough and, even not mentioned in the conclusion. Moreover, the authors do not provide enough arguments and evidences to support their interpretations. For example, there is nothing demonstrating the good quality of the presented data, no data on dust deposition, and no explanations/data evidencing a winter deep convection in the western basin. My review consists in a relatively long list of questions, highlighting the lack of details in the manuscript. After revision, the new manuscript should provide all the details answering these questions. This will make the manuscript more convincing as for now the reader must believe your interpretation only with words and not with facts. Finally, the comparison of Baxs data from the same station but at different visits deserves more attention as there are no many studies (or none?) investigating the Baxs evolution over time. The significant difference of DWA Baxs and MR flux between visits however brings another question about the Baxs proxy: Is the seasonal time integration proposed in earlier studies correct?

Please, see my comments in the attached file.

Please also note the supplement to this comment:
<https://bg.copernicus.org/preprints/bg-2020-271/bg-2020-271-RC1-supplement.pdf>

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

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