

Interactive comment on “Particulate barium tracing significant mesopelagic carbon remineralisation in the North Atlantic” by Nolwenn Lemaitre et al.

Anonymous Referee #2

Received and published: 26 December 2017

In this manuscript, Lemaitre and coauthors presented Baxs-derived mesopelagic oxygen consumption and carbon remineralization fluxes along the GEOVIDE section in the North Atlantic. Mesopelagic carbon remineralization fluxes were further compared with those from drifting sediment traps (both shallow and deep) and shipboard incubation from previous studies. A synthesis discussion on the difference in the primary production, upper-ocean POC export and mesopelagic remineralization was probably my favorite part of this manuscript. Overall, the data presented are very interesting and contribute to further our knowledge on the biological carbon pump efficiency. However, the current manuscript can be significantly improved to avoid the redundancy throughout the text, to avoid heavy citation of data that are currently not available, and to make

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clear justification of some of their conclusions.

Major comments: (1) Introduction This section is unsatisfyingly short in my opinion. It really only includes 12 lines of background (L41 – 53). It needs to be expanded to include our current knowledge of the biological carbon pump (BCP) to date: why is it important to study BCP? Why North Atlantic? Why use the Baxs proxy? What is the driving hypothesis and significance of your research?

(2) Sampling and analyses It is unclear why two completely different sampling and digesting methods were carried out for Baxs. While the authors gave great details on how different the methods were, there is little discussion on the comparison except in L207-210. The authors suggested good agreement between both datasets (their Fig S1); however, a closer look at the data only show good agreement for Baxs < 400 pmol/L. At Baxs > 400 pmol/L, data in Fig S1 (Go-Flo Baxs vs. Niskin Baxs) are more scattered. These are actually the samples that are from depths of interests (100 -1000 m), and likely suggest a discrepancy between the Go-Flo samples and Niskin ones. My other curiosity is, despite two different sampling techniques, why the authors applied two completely different filter digestions. Wouldn't it be better to use the same chemical protocols for a better data comparison?

(3) Section 3.1 This section involves large amount of discussion and I suggest moving most of this section to Section 4. I have a couple thoughts about this section. Firstly, Fig. 3 does not show any biogenic fragments, and thus I cannot judge whether barite crystals were actually observed adjacent to biogenic fragments. Secondly, barite crystals are believed to form in the microenvironment of decaying organic matter, so it is probably expected that no barite crystal is seen in surface samples.

(4) Section 3.3 This section is difficult to read, as there is a lot of jumping back and forth between provinces. I would suggest the authors to describe their data in a consistent way. For the selection of background level depth, the authors need to justified whether the absolute background values are more important for data comparison, or whether it

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is better to compare data consistently at the same depth.

(5) Section 4.2 The relationship of Baxs and carbon remineralization and their derived correlation in the North Atlantic are shown here, and the comparison between these and those in the Southern Ocean is very interesting. However, I would suggest the authors calculate the errors for both the slope and intercept for the North Atlantic data, then compare them with those from the Southern Ocean. Without showing the relevant errors, this comparison is meaningless. Also, what is the correlation coefficient of the North Atlantic data? Is the correlation actually significant (data are very scattered)?

(6) Some of the discussion (e.g., Sections 4.1.1, 4.4) and conclusions in this manuscript depend heavily on unpublished data (e.g., Lemaitre et al., in prep; Roukaerts et al., unpublished data). These data are not accessible to readers and reviewers, and thus the discussion and conclusions reached cannot be justified. The authors need to either add these unpublished data to the manuscript, or remove relevant discussion.

Minor comments: L54-58: out of place. Move down to L66. L57-58: Inappropriate citations of Cao et al., 2016 and Horner et al., 2015. Both of these studies did not measure Baxs, but water column Ba isotopes. L87-91: It would be helpful to draw the subarctic front and formation site of the Labrador Sea Water in either Fig 2. L93-96: Are these results from this study or others'? L101-102: This sentence is confusing. Table S1 does not show PP or POC fluxes. L127-128: Were filters rinsed with MQ and dried at sea as well? Or were they kept frozen until home analysis? L129: 'for 4 h', not 'during' L142: 'at similar depths' L142-143: the sentence reads as "the comparison . . . was excellent". Please rewrite this sentence. L147-149: It would help to explain why only these few discrete samples were scanned. L178-186: This belongs to Methods. L187-188: The FE-SEM result is two fold of that measured by ICP-MS! L200-201: Some of these stations are not listed on Table S1 or shown on map. L204: These maxima appear to be at 200 – 600 m in Fig 4, not 100 – 300 m. It also doesn't seem that such maxima necessarily spread over a larger depth range. L210: Reference for

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the Th-234 data. L210-211: Not clear why this needs to be mentioned here. L214: It needs to be justified why such value (180 pmol/L) is chosen as the background value. L218: The Baxs value is different from that in L200 L221-222: There is no double peak at St. 32: the Baxs values between 200 and 450 m are the same within errors. L225: To be scientifically correct: [Baxs] reach ~ 750 pmol/L at 200-400 m. L226-227: This sentence is confusing. L228-232: Do vertical profiles between GEOSECS and GEOVIDE stations agree with each other, or do only the ranges agree? Since the ranges in [Baxs] are quite large, comparison of these ranges is meaningless unless you can show the comparison in a plot. L 241-244: Since there is no difference between the 100-500 m and 100-1000 m depth intervals, it is unclear to me why the 100-1000 m interval represent "the best the complete mesopelagic layer". L 245-146: delete "between 100 and 1000 m", as it is already specified that this is the mesopelagic depth interval used. L250-251: move to L244 L297-301: It is unclear how the advected signal was calculated. L297-298: Repetition of L221-222. L302-304: Please also speculate what causes the second Baxs peak at St. 38. L341-343: Repetition of L155-157. L355-359: These fit better in Section 4.3. L364-366: 100 – 1000 m, to be consistent. L369: ' . . . is in the same order of magnitude as to . . .' L374-377: This sentence is difficult to read. L378: what does it mean ' . . . with the region around Cape Verde . . .' L381: similar to, not similar than

Fig 4 is referenced after Fig 5 in the text. Fig 5: corresponding to Section 4.1.2, this figure would benefit if depth ranges of major water masses are superimposed. Fig 6: Make the color coding consistent with Fig 1. Fig S1 is never referenced in the main text.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-400>, 2017.

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