

## ***Interactive comment on “High variability of export fluxes along the North Atlantic GEOTRACES section GA01: Particulate organic carbon export deduced from the $^{234}\text{Th}$ method” by Nolwenn Lemaitre et al.***

### **Anonymous Referee #2**

Received and published: 16 July 2018

This manuscript presents a valuable dataset that deserves publication. However the manuscript is missing clear motivations and objectives (see comment 1). This shortcoming has an impact throughout the manuscript, which is tedious to read and not as informative as it could be (see comment #2). The manuscript is long but new results advertised in the abstract are not clearly highlighted and discussed in the main text (e.g. control from phytoplankton size structure and the stage of the bloom). The result section is a very descriptive listing of all measured parameters and the discussion resembles a result section (comments #3 and #4). I recommend major revisions to

C1

improve the readability and strengthen the main points.

#### Major Comments

1) The manuscript lacks a clear objective. In Line 61, “According to the impact of these biogeochemical factors [ . . . ], the efficiency of the NATl to transfer POC .. can be questioned. In this context, we investigated the . . . export using Thorium.” How is the state-of-the-art presented between L32 to 60 questioning the transfert efficiency established in previous studies? After presenting the state-of-the-art, I strongly encourage the authors to present what open question or inconsistency they are trying to address with their dataset. Possible avenues are: What is missing in previous studies? How is this dataset complementary or inconsistent with previous data? I suggest following the traditional structure: 1\_ Previous studies showed that X . . . . 2\_ However, Y is still unknown (or this is inconsistent with Z); 3\_ Here, we examine/show/leverage. . . . This objective should also guide the reader in the result and discussion section (see comment 2).

2) the result section appears as a long list of parameters (e.g. 3.3- Particulate Th and POC distribution, 3.4- POC:Th ratios, 3.5-POC export; 3.6- PP), and include too many methodological details. For example: L 287 to 292 “LSF particles are collected on silver GF/F filters . . .”; L329-332; L377 “Using the 8-day average data, PP was estimated for the preceding month and the whole productive period. . . ”. The result section should be re-worked to emphasize the important connections between the different measured parameters (export, PP, planktonic composition etc.). One option would be to present the results per biogeochemical province and make these links. Another option would be to organise the result section based on processes and/or novel findings (for example: Control by phytoplankton size structure, Modulation by stage of the bloom . . . or Flux attenuation in mesopelagic zone, which is now in the discussion but would fit better in as a result section- see comment #4). Please move methodological details to method section or remove when it is duplicated.

C2

3) the discussion of uncertainty opens the discussion (sections 4.1 and 4.2 NSS and physical transport). While I value this discussion, it is not novel and has been discussed in previous studies. I suggest to move it to the end of the manuscript. Please start with what is new and motivating before discussing the limitations of the method.

4) large part of the discussion pertains to the result section (sections 4.3, 4.4 and 4.5) and could help organise the results (see comment #2). There are also many methodological details in the discussion section that should be (re)moved (e.g. L446, L462, L483), in particular when these details are stated several times. For example, L483 explains what thorium deficit is, even though this explanation is already included in the introduction, the method and the result sections. The discussion should emphasize what this study brings to existing studies and discuss the limitations.

5) please streamline the text. Many methodological concepts are presented and introduced in several sections (e.d. thorium deficit, PP measurements etc.).

---

Minor comments: L43-46: two sentences are repeated in the introduction. Remove one version.

L61-63 needs to be rephrased (see comments above)

Method section could be sharpened by limiting the use of “moderate” (e.g. L91, L106, L116). Please limit the use of “moderate” (e.g. L91, L106, L116), which is rather vague. The word “briefly” (L127, L137, L222) should also be avoided. Either you have described the method and you can remove “briefly”, or you haven’t described enough and should include additional references or details.

L102: associated with (not to)

L235: other estimates of PP mentioned here could be added for comparison in Figure 4. This would give confidence in the author’s choice and inform the reader on the uncertainty associated with these estimates.

C3

L259-261: move definition of PPZ above, when it is first mentioned.

L110: Tonnard et al, in prep and L521 Lemaitre et al, in prep. Does the journal authorized unpublished papers?

---

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-190>, 2018.

C4