

**Associate Editor Decision: Publish subject to minor revisions (review by editor)** (13 Nov 2018) by

Sophie Bonnet

Comments to the Author:

Dear authors,

Your manuscript has been sent to the original reviewers, who find that the manuscript has greatly improved. However one of them still recommends minor revisions (see below) before your manuscript will be accepted.

Best regards,

Sophie Bonnet

This revision is much improved – the more detailed descriptions of calculations and associated caveats are appreciated. I have a few comments on minor points that could use further clarification.

You might add a supplemental table with the actual statistical results, PDF document would be fine, just to make that info available for those who are interested. Likewise, will the actual data be archived somewhere? If so, you should point the readers to that repository; if not, you might consider putting the results in a supplemental file (i.e., concentrations at each depth at each station, in much more detail than Table 1). A major advantage of this work is that it contributes to our knowledge of an under-sampled region of the ocean, so those results will certainly be of use to the broader community.

Full DOC and DCNS data are available at the following link [http://www.obs-  
vlfr.fr/proof/ftpfree/outpace/db/](http://www.obs-vlfr.fr/proof/ftpfree/outpace/db/) and are in open access. This info is given in the revised MS in Table 1 legend.

Line 26: Please say 18degS instead of -18degN.

**DONE**

Line 29: Are you saying there's no difference in DOC concentrations at specific depths? Because at first glance this statement reads as inconsistent with the differences in integrated stocks reported just below. You do need to be careful throughout the manuscript to be clear when you're discussing concentrations (no differences between regions) vs. integrated stocks (significant differences between regions). E.g., Table 1 vs. Figure 6 – the distinction is clear with attention to detail, but I think it'd be worthwhile to make sure your readers have minimal opportunities to misinterpret. The body of the text is generally fine, it's more in the figures, abstract, conclusions, etc.

We agree with this comment and we re-phrased the sentence (lines 31-35 in the revised ms). In fact, in lines 29-31 we gave mean values of DCNS and DOC which were about similar in both areas whereas euphotic zone integrated values of same parameters changed considerably among the MA and WGY zone.

Line 125: This link doesn't work for me (it may be temporary, not sure). Might be less problematic to replace with a citation to something from Hansell's group talking about the ref waters.

Done. We corrected the link

Line 300: This is confusing and needs to be rephrased – you say you’re calculating the residence time of DOC<sub>sl</sub> as DOC<sub>ex</sub>, but then you talk about how the calculation overestimates the residence time of ultra-labile DOC. Are you trying to say that the residence time is over-estimated \*because\* it’s calculated using uptake of ultra-labile DOC?

We agree with comment. Indeed the DOC<sub>EX</sub> is representative of DOC<sub>SL</sub> but at the opposite BCD is not accurately estimated for DOC<sub>SL</sub> because the leucine technique (incubation of 2 h) used for its determination tracks only the utilization of ultralabile to labile components. As such the residence time time of semi-labile DOC calculated as the ratio of DOCEX to BCD is overestimated. This point was better explained in the revised MS (see lines 292-306).

Line 353: biogeochemical rather than biochemical?

Changed according to the reviewer suggestion (see line 357 in the revised MS).

Line 365: This raises the question in my mind of whether the MA area experiences higher turnover or seasonal mixing than the gyre, to the extent that it would compensate for the shorter residence time of DCNSs in that region, such that the export might not be that different between the two regions, just controlled by different mechanisms? I am definitely not insisting that you go down that path, but it could be interesting.

Yes it would be an interesting perspective to link results of residence time to organic matter export or sequestration but with the current data in hand it is quite hard to explore further the reviewer’s suggestion. In fact additional data including carbohydrate values in sediments traps, seasonal variability among the two areas will definitively help to go through this path.

Line 372-373: Can you cite something for the statement that exopolysaccharides would be hydrolyzed (suggesting the production of nitrogen-requiring exoenzymes) but then not taken up? Or if speculative, please be more explicit that that is the case. Or are you saying that glucose is a by-product of other exohydrolytic activity that isn’t used and so accumulates? If the latter, this sentence needs rewording – took me a while to get to that possible meaning.

Yes this is speculative. We agree that if carbohydrate hydrolysis exists, some N should be available for the synthesis of ectoenzymes hydrolyzing it. (see revised MS lines 376-377).

Line 385: missing an adjective (higher/greater)

Indicated higher in the revised MS.

Lines 395-396: I would remove this sentence – it’s not wrong, but it’s not a meaningful conclusion from this particular study.

DONE

Figure 4: The relatively high deep-water DCNS concentrations at ~175W are intriguing – is there a water mass difference that could be contributing? Quite a lot higher than the values you cite in Line 218.

Yes, in the SD11 station DCNS values were around 2  $\mu\text{MC}$ , we do not know why but all over the OUTPACE cruise mesopelagic DCNS ranged 0.3- to 2.4  $\mu\text{MC}$  (line 216 in the MS).

Figure 6: Perhaps take a look at an axis break between the DOCex and DCNS-C values, to make DCNS-C easier to see, if differences or lack thereof between regions is the point? Unless the point of the figure is to emphasize that DCNS-C is a very small percentage of the overall DOC stock, in which case it works well for that purpose.

We agree with this comment and Fig. 6 was changed accordingly.

Throughout: Mann-Whitney is written as Man-Whitney in several places.

DONE

(see lines 627, 646, 649, 654)