

Interactive comment on “The composition and distribution of labile dissolved organic matter across the south west Pacific” by Christos Panagiotopoulos et al.

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The authors measured [DOC], [DCNS], and BP across a gradient of oligotrophic conditions in the South Pacific subtropical gyre. They calculate a variety of parameters relating to the role of bacteria in DOC cycling and carbon export in the region, including BCD and DOC residence times. The results and discussion of this manuscript need to better reflect the data presented in the figures – there are some notable disconnects, such as BCD, where the results in the figures are neither presented in the Results section nor is the significance of those results expounded on in the discussion. I've noted the instances that most stand out to me here:

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1) The Results section needs to include BP, BCD, and PP (the latter is perhaps from another study, but it could still be summarized). My first thought on looking at Figure 4, in context of the authors' discussion of accumulating DOCs, was that it seemed like BCD and PP might be reversed. While I quickly realized that's not the case, as the manuscript stands, there is no way to double-check the figure against numerical values.

We agree with this comment and in the revised MS we expanded our discussion on BCD and GPP (page 8, lines 192-197). The figure has changed accordingly (now Fig. 2), however the discussion on BCD and GPP are not the main points of this paper and the reviewer is invited to check on Van Wambeke et al. (2018).

2) The discussion section needs to be expanded, as the context and significance of the results are largely glossed over. In particular, the authors neglect to discuss the incongruity of BCDs that so largely exceed PP, yet they note a residence time of DOCs of up to several months and a glucose-heavy DCNS pool that implies highly workedover DOM. So then what could possibly be supporting that high BCD in the gyre? Of course all of these measurements/estimations have uncertainties and assumptions, but those caveats need to be presented and/or the authors need to explain what they hypothesize is driving such extreme heterotrophy in the system.

We fully agree with this comment and in revised MS all of these points were carefully addressed:

- We explained how the DOC excess was calculated and this we assumed that corresponds to DOC semi-labile (page 11 lines 259-267). - We compare the MA and WGY areas using statistical tools (Man-Whitney tests) and the results are given in Table 1 - We expanded our discussion on residence time calculation indicating the uncertainties and assumption of our approach (lines 290-300 in the revised MS).

I recommend the authors conduct one more proofread for grammar – by and large the manuscript is well written, but there are many instances of missing words such as

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prepositions.

Title: Would be more accurate to refer to DOC/DCNS rather than to labile DOM, as in fact the authors largely discuss presumed semi-labile DOM and make no direct measurements of DOM lability.

We agree with this comment and we indicated semi-labile DOM in the title.

Abstract: I would find it useful to have a brief summary of methods in the abstract (i.e., that residence times were estimated by comparing stocks rather than by experimental approaches).

DONE, see lines 32-36, page 2 in the revised MS.

Line 59: The abbreviation of gyre alone is very unnecessary. It saves 2 letters but makes the text significantly less reader-friendly.

We agree with this comment and we deleted this abbreviation from the whole text.

Line 67: I don't believe Goldberg et al. measured the percent of labile DOC constituted by carbohydrates, and further, the composition of most truly labile DOC isn't well characterized (e.g., reviewed by Carlson and Hansell 2015, cited in the manuscript).

In this sentence we simply indicated that the semi-labile of DOC is mainly represented by carbohydrates. There are several references that support this statement (Benner et al., 1992; Aluwihare et al., 1997; Skoog and Benner, 1997; Benner 2002; Aluwihare et al., 2005; Repeta and Aluwihare, 2006) including the references that we provided in text. Indeed Golberg et al. (2011) did not measure the % of labile DOC but their study showed a systematic removal of DCNS within DOM across the ocean basins pointing to semi-labile nature of carbohydrates. Note also that semi-labile DOC contains also carbohydrates that do not belong to the category of DCNS such as those measured in this study and these carbohydrates exist as unhydrolyzed polymers and methylated carbohydrates as shown by NMR (Panagiotopoulos et al., 2007).

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Panagiotopoulos et al. 2007. Identification of methyl and deoxy sugars in marine high molecular weight dissolved organic matter (HMWDOM). Org. Geochem. 38, 884-896.

Line 89: This sounds like a hypothesis, but there is no discussion of N2 fixation in the results. Even if characterizing the N2 fixation gradient was the overall cruise goal, it should be removed from this manuscript unless the authors were to actually compare their results with N2 fixation via correlations or similar.

We agree with this comment and we deleted the word nitrogen fixation.

Results: A table with results in numerical format needs to be included. I was interested in seeing real numbers to judge for myself if the 4% difference in DOC concentrations between regions was consistent enough to be significant. It's much more difficult for others to use the work as a reference in the future if they are limited to estimating values from averages in figures. Finally, this is bad practice for data availability purposes. Would be fine to put it in supplementary material as long as this is clearly referenced in the text.

DONE, a Table with average volumetric values and ranges of DOC, DCNS-C, DCNS-C/DOC and DCGlc-c/DCNS-C was added in the revised MS. Statistics were performed as well to compare distributions in MA and WGY areas.

Line 252: I would like to see more discussion of the caveats of this estimation of residence time. E.g., the semi-labile pool is heterogeneous and composed of compounds that will be more or less biologically available, while BCD is derived from a measurement of BP that lasted 1-2 hours and therefore is likely based on the use of the most labile compounds available at that time. This may still be a useful metric for comparing between the two regions but it needs to be presented less as a clear-cut value.

We agree with the referee comment, as BP tracks labile to ultra-labile DOC substrates whereas DOC SL includes a pool turning over on time scales of weeks to months. In fact the DOCSL corresponds to the excess DOC (DOCEX) the latter calculated as the

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difference between an average deep DOC value ($39.6 \pm 1.4 \mu\text{MC}$, $n = 36$) from the bulk surface DOC pool. This info is now given in the revised MS (lines 246-249). Additional information is given in lines 263-297.

Regarding the estimation of the residence time we explained the assumptions that we made and we also included data of a biodegradation experiment (3 experiments 10 days each) performed also during the OUTPACE cruise. The results of this experiment (Van Wambeke et al., 2018; Table 5) showed that labile DOC represented only 2.5 to 5% of the DOC pool. This info is now included in the MS (see lines 290-300).

Lines 282-284: This sentence is confusing, please rephrase.

DONE, see page 14, lines 331-334 in the revised MS.

Line 303: How is this calculated? Is it DCNS divided by BCD, as for DOCsl above? That seems to make a lot of assumptions if so.

We agree with this comment. In the initial submitted MS we estimated DCNS residence time by dividing DCNS stock (integrated 0-200m) with BCD. We realized that this calculation was not corrected because (a) BCD was calculated after integration 0-euphotic zone and (2) only a portion of BCD is used for DCNS hydrolysis and subsequent bacterial uptake. In the revised MS (although some assumptions were made) we calculated DOC and DOCEX and DCNS-C stocks after integration 0-euphotic and we applied a percentage of 11% that corresponds to the part of BCD employed for glucose bacterial uptake after polysaccharide hydrolysis according to (Piontek et al., 2011). This info is now included in the revised MS (see page 15 lines 352-357). The results showed that a higher residence time of DCNS in the WGY ($Tr = 91 \pm 41$ days, $n = 3$) than the MA area ($Tr = 31 \pm 10$ days, $n=8$).

Line 306: The hypothesized role of carbohydrates in export to depth is not coherent with the statement three lines above that these compounds have residence times of 3 or 8 days, especially in a stratified system such as the gyre. Please expand on

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this statement to explain this speculation better. (It's obviously quite possible this is happening, as DCNS are present at depth – perhaps this indicates an issue with the DCNS residence time calculation, as above?)

See previous comment on DCNS residence time calculation. The text is corrected accordingly (see page 15, 16, lines 361-364).

Line 324: I don't follow this sentence; please rephrase.

Done. See page 16, lines 384-389.

Line 541: Specify who/what is CLS.

The Figure was modified according to reviewer suggestions and its present legend does not contain this term.

Line 557: Carbon stock should not be d-1 here.

We agree with this comment and we corrected it in the revised MS.

Figures 1-3: The longitude should be marked consistently between Fig. 1 and Figs. 2-3.

DONE

Figure 1: A locator map would be appreciated for those reading this as a stand-alone paper and not as part of the broader cruise special issue.

DONE. Fig. legend was changed accordingly.

For Figs. 2 and 3, have you tried plotting panels B and C on the same axis? The ability to visually assess DCGlc as a proportion of DCNS might be worth the loss in resolution in panel C. (This is only a suggestion, please take it as such.)

In the revised MS we added this info in Table 1 (average values) and the reviewer can see the differences among MA and WGY areas.

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Figure 5: It would help the reader to label the depths on each panel.

DONE.

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