Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-158-RC2, 2018

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

Anonymous Referee #2

Received and published: 18 July 2018

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:

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 DOCsl, 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.

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 DOCsl of up to several months and a glucose-heavy DCNS pool that implies highly worked-over 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.

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 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.

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).

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

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).

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.

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.

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.

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

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.

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 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?)

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

Line 541: Specify who/what is CLS.

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

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

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.

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.)

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

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-158, 2018.