

Interactive comment on “Evolution of dissolved and particulate chromophoric materials during the VAHINE mesocosm experiment in the New Caledonian coral lagoon (South West Pacific)” by M. Tedetti et al.

Anonymous Referee #2

Received and published: 6 April 2016

Overview

In this manuscript, Tedetti et al. investigated the temporal changes in the optical properties of CDOM and particulate matter during a mesocosm study located outside a lagoon in New Caledonia. P-fertilization of the mesocosm was carried out during the study in order to stimulate diazotrophs and N₂ fixation, and the evolution of the optical properties was studied in this context.

The study led to the following conclusions: 1) A strong connection was observed be-

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tween the abundance of synechococcus and the absorption of particulate matter and CDOM during the mesocosm, suggesting synechococcus was a strong contributor to the particulate absorption and was strongly involved in the production of CDOM. 2) The data also support the idea that N₂ fixation by diazotrophs enhanced the synechococcus bloom and indirectly contributed to the production of the chromophoric material in the mesocosm, suggesting the existence of an indirect link between N₂ fixation and the production of chromophoric material. 3) There was a surprising decoupling between FDOM and CDOM during the mesocosm study, which the authors attributed to the two components being regulated by different processes.

Overall, the study provided convincing evidence of a strong link between the dynamics of synechococcus bloom and that of the chromophoric material, and provides good reasoning supporting the idea (although it does not provide hard evidence of it) that there is a link between N₂ fixation and the production of chromophoric via the stimulation of the synechococcus bloom.

Overall the manuscript is well written and referenced. The figures and tables are generally of high quality and clear. The methods are adequate and clearly explained, and the conclusions drawn are generally well supported by the data presented. The results and conclusions advance our understanding of the processes regulating CDOM and chromophoric particulate matter in the ocean. The topic and scientific contribution are appropriate for “Biogeosciences” and for this special issue.

I recommend the manuscript for publication after the following comments are addressed (minor revisions).

————— Major Comments —————

1. The S_{g+p} data: I do not see the value in presenting the a_{g+p} spectra or the corresponding spectral slope coefficients (S_{g+p}). The results are shown but the implications are never adequately discussed in the manuscript. The point of presenting these data remains unclear, and the data are more distracting than anything. Furthermore, I don't

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find it suitable to calculate a S-value from ag+p spectra that are not really exponential. I would recommend removing the ag+p and Sg+p data, unless they are used in a meaningful way in the discussion and they enhance the conclusions of the manuscript.

2. Decoupling between CDOM and FDOM: The authors attribute the lack of correspondence observed between CDOM and FDOM to the fact the dynamics of these two components are probably driven by different processes. While this conclusion is not erroneous, another possible explanation is that the components that are fluorescing are not major components of the CDOM (meaning they absorb but not strongly enough to affect the CDOM variability in a significant way). I think this could be included in the discussion of this result.

3. Link between N₂ fixation and chromophoric material: The following paper might provide some useful insights about the role of N in the formation of CDOM/FDOM: Biers et al. (2007) The role of nitrogen in chromophoric and fluorescent dissolved organic matter formation. Marine Chemistry. doi:10.1016/j.marchem.2006.06.003

4. Figure 5: I think Figure 5 could be improved. The presentation of all the spectra in the left panels makes it difficult to discern any spectra. For each variable, I would recommend the authors show only 3-4 spectra from distinct times during the mesocosm study (e.g., Initial ; P1 ; P2). In order to show the full range, the average of all spectra could be shown with the range shown as a gray area (instead of showing the standard deviations). Again, I don't think adding the ag+p spectra adds to the paper, and I would suggest removing these data unless the authors can use the data in a meaningful way.

5. New figure: I leave this to the discretion of the authors, but I think adding a figure showing the plots of the relationship between ag vs synechococcus and ap vs synechococcus would help emphasize to the readers (who often don't read the entire manuscript and just look at figures) that there is strong connection between ag/ap and synechococcus. Showing this in a figure would help getting the point across (this is probably one of the most important finding in the paper).

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6. Abstract: I think it would be worthwhile to expand and clarify the last sentence of the abstract. This is an important point of the paper, but the last sentence will be a little unclear to someone who hasn't (and might never have time) to read the entire paper. I suggest replacing last sentence with something like that: "Finally, the results of this work support the idea there is indirect coupling between the dynamics of N2 fixation and that of chromophoric material via the stimulation of synechococcus bloom."

————— Minor comments —————

Abstract, Line 45: I would suggest using a more specific term than "activities"

Abstract, Line 48: Replace "proving that these were" by "suggesting they were". Also, see Major comment 2) shown above.

Line 171: Please explain what EVA is here.

Line 203: I suggest adding "(see section 2.2)" after "onboard"

Line 278: Replace "With regard to our" by "Considering the"

Line 306: Please add citation for fluorometry method

Line 314-316: Can you provide a citation and expand briefly about the clustering approach used.

Line 324: Weird sentence structure. Please change to "BP was calculated. . .leucine, and is shown here in ng C L-1 h-1."

Line 336-339: Please provide name of instruments used for DIN and TN analysis

Line 380-382: I know no replicates were measured, but is there any information about the typically uncertainty in this measurement that could be added here.

Line 382: I suggest using "outside the mesocosm" here and throughout the manuscript instead of "OUT"

Line 397: Change to "No significant difference"

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Line 402: Please provide value correspond to “much higher”... (~ 10 times)

Line 425-427: Again, consider removing the ag+p data as they are simply a combination of ap and ag features and they are not really insightful

Line 436-439: This kind of presentation makes it hard to match values to compare them. Please consider writing this sentence so each variable is directly shown with to its corresponding range.

Line 483-484: The high correlation between either ag or ap and ag+p should be somewhat expected considering ag+p is a combination of ag and ap.

Line 546-548: The data in Figure 8 support this statement, but I did not see a statement in the results that explained that the combined fluorescence values of the Tryptophan-like and Tyrosine-like component were substantially higher than that of the humic-like component. I think this needs to be mentioned in the results to substantiate the claim made here.

Line 553: It would be good to cite one of Benner’s paper considering its contribution to the topic. (e.g., David and Benner (2007) Limnol. Oceanogr.)

Line 565: I don’t think you can claim it is no longer photodegradable because it no longer absorbs in natural solar radiation range. There could be secondary photochemical reactions that could still photodegrade it (e.g, via reaction with radicals produced from other photochemical reactions).

Line 605-607: Rephrase with something like “ Several observations suggest the observed change in particulate matter absorption (ap) during the experiment was mainly driven by Synechococcus”.

Line 634-635: Rephrase as follows: “. . .Table 2), thereby suggesting CDOM was produced by heterotrophic bacteria. . .”

Line 660-663: How was the decrease that attributed to N limitation? Is this from another

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work in this special issue?

Line 669: replace “putting forward a rapid. . .” by “suggesting there is a rapid. . .”

Line 687-690: Replace “submitted to” by “affected by”. Also, please remove “which led to modifications in the CDOM molecular weight”. There is no evidence of that in the data. A link between S and MW has been mainly shown for land-derived CDOM and for S275-295.

Line 693: Replace “CDOM in total” by “CDOM to the total”. Also, did the contribution of pure water included in the total absorption here, or is this the contribution of ag to ag+p? Please clarify.

Line 745-746: Wording is a little strong and definitive for this part of the study. Please rephrase with something like “Finally, this study strongly supports the idea of an indirect link between the dynamics. . .Pacific.” You might want to mention that more work is needed to directly demonstrate the role of N₂ fixation in the production of chromophoric material.

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