

Comments by referee 2 and our responses

Comments on the manuscript bg-2020-411: Influence of atmospheric deposition on biogeochemical cycles in an oligotrophic ocean system

This manuscript by Van Wambeke et al. aims at investigating the impact of atmospheric depositions on the biogeochemical properties and processes in the Mediterranean Sea surface mixed layer. For this purpose, the authors present a large amount of data, collected both in the water column and in the atmospheric depositions, along a transect covering both the Western and Eastern basins of the Mediterranean Sea, during the PEACETIME cruise. The authors also presented the results of an enrichment experiment.

The strength of this paper, as the authors stated in their conclusions, is that it provides simultaneous sampling on both atmospheric depositions and the surface ocean on a large portion of the Mediterranean Sea. Moreover, as the authors highlighted in their introduction, the “in situ” study of atmospheric depositions is very rare due to obvious limitations. For these reasons, I think these data should be published.

However, the manuscript still needs a bit of refining. I hope that the following comments will be helpful to the authors.

We acknowledge the general comments of the referee and will respond to his/her more specific comments below. Citations of some parts of the revised version are in italics.

Main considerations:

1) I understand the difficulty of writing a paper with so many results. However, an effort need to be made to shorten the whole manuscript. In the current form I find it quite hard to read, it is too long, one can get lost while reading it. I suggest the authors try to smooth it and make it shorter. There are long sentences that can be shortened, or written with fewer words.

We agree, this was also recommended by the other referee. We considerably reduced the number of time we report values and ranges in the text, when those were available in a table. A new Table (S2) was added in the supplementary section to present N and C fluxes and stocks at the long duration stations ION and FAST.

The old paragraphs 2.3 in M&M and 3.5 in results section as well as old Figure 6 dealing on the enrichment experiments have been moved to the supplementary information (see below). Section 3.3 has been reorganized in a more logical order; section 3.4 has been reduced, as well as the discussion. All in all, the ms is now reduced by a hundred lines (~2 pages).

2) I think that the setup of the enrichment experiment is somehow in contrast with the goal of this paper, which is studying the “in situ” effect of atmospheric deposition. As the authors highlighted in the introduction, these experiments are simplifying the natural system. How can we relate the results of an experiment carried out into a 60 ml bottle with what happens in the natural

environment? The authors themselves conclude that the results of the experiment cannot be compared with the “in situ” observations. I suggest removing this part.

We agree. The material and method, results and Figure describing the enrichment experiment has been moved to the supplementary section. We decided to keep it this way because the results of the enrichment experiments are cited in other ms of the special issue (Gazeau et al., 2021, Marañón et al., 2021). This enrichment experiment is now cited in the text only in the discussion phase (section 4.1) as follows:

'The dependence of hprok on nutrients rather than on labile organic carbon during stratification conditions is not uncommon in the MS (Van Wambeke et al., 2002, C ea et al, 2017; Sala et al., 2002) and has been also shown during peacetime cruise (P, or N,P colimitation, Fig. S4).'

3) It is clear that a lot of the results obtained from this cruise/project are presented in other papers that are currently under review in this issue or are being prepared.

There is a bit of confusion about some data, reported as results in this study, but at the same time citing other papers (under review or in preparation). In particular, I refer to the following:

Lines 418-421, DIP results in Pulido-Villena et al.

This manuscript is submitted this week. I can provide the ms draft on your request.

Lines 422, PP and BP in Maranon et al., 2020

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<https://bg.copernicus.org/articles/18/1749/2021/bg-18-1749-2021.pdf>

Lines 432-435, LAP results in Van Wambeke et al., 2020

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<https://bg.copernicus.org/articles/18/2301/2021/bg-18-2301-2021.pdf>

Lines 515-517, Citing the results from Fu et al.

The authors should clarify, if the results presented are already been present in the cited papers, they should be considered in the discussion section and not presented as results (and therefore also removed from the methods section). If this is the case, it would also help to make the whole manuscript shorter and more readable.

This part of the ms has been modified. As the two papers dealing with atmospheric deposition will be submitted likely in few weeks, we decided to provide in this revised version, all the information that are needed to understand atmospheric deposition estimation, i.e. details on the flux calculations from precipitation and nutrients analyses in the soluble fractions from aerosols samples in the M&M section, the estimations of N and P fluxes in rain samples. We also modified Table 3 accordingly.

Note that atmospheric wet deposition fluxes is not anymore only calculated from the time of collection and the diameter of rain funnel sampling rain on board, but rather from analysis of the soluble fraction of nutrients in the rain and precipitation estimates (in mm) from the radar data, and we refer to a published article for the methodology used (Hersbach et al., 2018). The M&M section was modified as: *'In the rains collected onboard the ship, NO₃, NH₄ and dissolved inorganic phosphorus (DIP) were also determined by ion chromatography after recovery of the dissolved fraction in the samples. Then, the wet deposition fluxes of these nutrients were estimated from the measured concentrations in the dissolved fractions of rains, multiplied by the*

total precipitation. This total precipitation was issued from the hourly total precipitation accumulated during the rain event over the region from ERA5 hourly data reanalysis (Hersbach et al., 2018). Total precipitation was obtained by adding hourly rainfall on the grid-points (0.25° x 0.25°) spanning the ship location, more or less 1° around this central grid-point for integrating the regional variability (Table 3).'

Hersbach, H., Bell, B., Berrisford, P., Biavati, G., Horányi, A., Muñoz Sabater, J., Nicolas, J., Peubey, C., Radu, R., Rozum, I., Schepers, D., Simmons, A., Soci, C., Dee, D., Thépaut, J-N.: ERA5 hourly data on single levels from 1979 to present. Copernicus Climate Change Service (C3S) Climate Data Store (CDS). doi: 10.24381/cds.adbb2d47, 2018.

4) There are way too many citations of articles that are “in preparation”, I have counted at least 17. Citing a paper that is in preparation is usually not recommended (sometimes even not allowed), the data are not available and there is no guarantee that they will be. These citations need to be strongly reduced.

Yes, some of them were repeatedly cited in the first version of the ms and we should have paid more attention to that. We cited in the first version of the ms 6 papers in preparation, all intended to be submitted the special issue: Fu et al. b 5 times, Fu et al. a 1 time, Desboeufs et al. 4 times, Bressac et al. 3 times, Pulido-Villena et al. 6 times, Ridame et al. 1 time.

Since that time, Fu et al b and Desboeufs et al are merged in only one ms (Desboeufs et al.), likely to be submitted before the end of April. This article as well as Fu et al. a are cited ‘in prep for the special issue’ but as previously said, the main information needed from these 2 papers are now reported in our revised version.

Bressac et al. in prep is not cited anymore

Pulido-Villena et al. has been submitted on the 09/04/2021:

Pulido-Villena, E., Desboeufs, K., Djaoudi, K., Van Wambeke, F., Barrillon, S., Doglioli, A., Petrenko, A., Taillandier, V., Fu, F., Gaillard, T., Guasco, S., Nunige, S., Triquet, S., and Guieu, C.: Phosphorus cycling in the upper waters of the Mediterranean Sea (Peacetime cruise): relative contribution of external and internal sources, Bigeosciences Discuss., this special issue, bg-2021-94, submitted

We don't cite anymore Ridame et al which was cited only once in the M&M section. We substituted this reference to Ridame et al. (2011), a reference which is already cited in the ms, as in this article the methodology is also described as well.

We can provide ms drafts to the referee upon request if necessary.

Minor comments:

Lines 169-172: This information is contained in table 1, they can be removed from the text and cite the table. Please also check that for stations ION and TYR the dates in the methods and table 1 do not correspond

Yes it is because on Table 1 we restricted time period to the CTDs casts sampling period, whereas in the text it was the whole ship time occupation of the sites. Anyway as suggested, we removed this paragraph in the text.

Lines 196 – 198: Define high and low frequency

At ION, it was 24 h, at FAST, the lower time lag between 2 CTDs was 7 h (at least those where both nutrients and BP were measured simultaneously). However, this part of the text was removed to reduce the ms.

Lines 405-416: This division in groups could be summarized in a table, to be more clear to the reader

The table already exists, it is Table S1. The paragraph was reduced.

Lines 425 – 427: Integrated PP and BP, how were they calculated? This information is missing in the methods

It was not written in this part but on line 379 of the previous version (the start of section 2.4) as follows:

‘Trapezoidal integration was used to integrate BP, PP and N₂fix within the ML.’

and later on in this section:

‘Daily BP, AP and LAP integrated activities were calculated from hourly rates x 24’.

Paragraph 3.4 is a mixture of results and discussion and extra information that go beyond the results

We agree, this section was reduced to focus only on results.