I want to apologize with the Editor and the Authors for the delay in sending this report, due to family reasons overlapped with the unusual period we are going through because of the pandemics.

Referee report for the manuscript bg-2020-411: **Influence of atmospheric deposition on biogeochemical cycles in an oligotrophic ocean system** by *France Van Wambeke, Vincent Taillandier, Karine Desboeufs, Elvira Pulido-Villena, Julie Dinasquet, Anja Engel, Emilio Marañón, Céline Ridame, Cécile Guieu*

This paper is clearly one among several contributions for a special issue of BG illustrating and discussing the results of the basin scale experiment PEACETIME, carried out during summer 2017 in the Mediterranean sea to assess the impact of atmospheric depositions on the functioning of plankton food web in the basin. Therefore it describes a small set among the numerous processes that have been studied during the cruise, namely, the weighted support to phytoplankton and heterotrophic bacteria production and nutrient assimilation in the surface layer by different forms of bioavailable Nitrogen (N) and Phosphorus (P). The obvious focus is on the forms derived by the atmospheric deposition. This is just a piece of a mosaic whose whole picture, I assume, will be published either in this special issue or somewhere else. The key question behind the experiment was the extent to which and by which mechanisms atmospheric deposition modulates biogeochemical processes in the basin. This is also reflected in the title and the manuscript addresses a key phase: the first processing of new nutrients in the surface layer. Therefore the results represent the short time response, even though they likely embed also responses due to processes occurring over some time before the event (see below). To better dissect the processes in the surface layer the authors divide it in two sublayers, the Mixed Layer (ML) proper which is the layer directly affected by air sea interactions and, therefeore, the entry point of atmospheric inputs, and a second layer spanning from the ML Depth and the depth of the nutriclines (N or P) identified as the depth of the ‘heaviest’ isopycnal with Dissolved Inorganic Nitrogen (DIN) or Phosphorus (DIP) values equal to zero. The latter determined by extrapolation in DiX-density diagrams. Considering what is reported in Table S1 I assume that the zero value is obtained plotting the concentration determined with the segmented flow analyzer (a clarification on line 333 would help). This allowed them to discriminate among the different sources or sinks of surface DiX to better identify the weight of the atmospheric component. Different N and P chemical forms where analyzed and linked to the different sources and sinks, namely DIN derived from N-fixation, DIN from atmospheric inputs, in situ Total Dissolved Nitrogen (TDN) and Total Hydrolizable Aminoacids (TAA) for Nitrogen and DIP from atmospheric inputs, total Dissolve Organic Phosphorus (DOP), and its labile component. Three stations were sampled for longer times, namely those where wet deposition events were occurring or had occurred just before sampling, which allowed, with the caveat of advective processes going on, of better reconstructing the vertical dynamics of N and P in relation to pico-plankton, the dominant component in the sampled area, activity. The main oucome of the paper is a quantification of the fraction of N-demand and P demand by pico-planktonic autotrophs and heterotrophs supplied by the atmospheric inputs as compared with the other sources of recycled or existing forms. An additional outcome is the difference in the N and P dynamics in the surface layer after input which may hint to a differential response of the picoplankton community to the supply of the two elements as weel as to particle scavenging.
The paper contains many useful data and tries to condense a big experimental effort. The need to present them in full, makes the text a bit heavy to read (e.g., section 3.1). In the discussion the authors make the commendable effort to examine all the possible processes, already reported in the literature, which may explain the patterns they observed but this, at the end, does not provide the reader with an answer, or at least the answer preferred by the authors. The fact that in such large experiments with so many scientists involved, there is a need to divide the informations in many papers weaken a bit some of them because many questions that arise reading one are likely answered in other papers of the same

issue. Of course I support the publication of the paper for the valuable information it provides, for the fact that is one piece of a large picture, and because the methods used are robust.
My suggestion to make the paper more punchy are the following:
1. Let the tables to summarize the data and shorten the description highlighting the most significant result. The text is definitely long.
2. Do not include the enrichment experiments. They do not add too much unless you discuss them integrating them with the other information.
3. Discuss what are your conclusions about the significant variability among the different sites having in view the possoble impact at basin scale. The present conclusions discuss other aspects.

Additional comments

l.215-218 "*The concentrations used were averages from PILS-IC analysis obtained during the occupation of each short station, and averages between two successive casts during site occupations, except for ST1 where concentrations were issued from filter by IC analyses after water extraction."* It is not clear to me why for dry deposition it was taken a quasi-instantaneous value instead of an estimate of the deposition during a few hours before the sampling. It would be intersting to mention here the variability of dry deposition along the cruise track which, should be part of the Fu et al. (in preaparation) paper and to discuss how representative are quasi-instantaneous values. Likewise for filter samples. This aspect is indeed considered only for the stations that were sample for many days because of wet deposition events (see l.628-632). I am also wondering, not being an expert, if the ions solubilization efficiency and time of aerosol particles in PILS is the same than that of sea water.

l.258 space after (...Dickson)

l.345 For what I understood the advective flux is an entrainment/detrainment mechanism sensu *Cullen, J. J., Franks, P. J., Karl, D. M., & Longhurst, A. L. A. N. (2002). Physical influences on marine ecosystem dynamics. The sea, 12, 297-336*. If true, an additional sentence may better clarify which processes the authors refer to.

l.441-443 It is a little confusing putting together the rates of opposite processes, the demand, which is the theoretical intake given certain compositional ratios, and the increase in availability. Since the sentence that follows clarifies the point I would remove the part of the sentence realated to the release rate or I would write "*... (hprokN demand), which is confronted by an in situ LAP…)*

l.629 this phrase may be misleading. Indeed what is measured is not the integral but the time-space weighted average of the fluxes

l.447 correct *dominatedby*

l.739 and the following. Please rephrase, since you do not *deliver* neither *accumulate* concentrations, as it is more properly written on l.744.

References

Pulido-Villena et al (2019) is cited on l.243 but is not included in the refs.

The following papers are in the References but are never cited in the text

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Karl, D. M.: Microbially mediated transformations of phosphorus in the sea: New views of an old cycle. Ann. Rev. Mar. Sci. 6: 279–337, doi: 10.1146/annurev-marine-010213-135046, 2014.

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