

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Frédéric Gazeau et al.

Author comment on "Impact of dust addition on the metabolism of Mediterranean plankton communities and carbon export under present and future conditions of pH and temperature" by Frédéric Gazeau et al., Biogeosciences Discuss.,
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We would like to thank Anonymous Referee #2 for her/his comments and suggestions on our manuscript. On one hand, we acknowledge that the conclusion section of our submitted manuscript requires improvements. We will do our best to revise the manuscript accordingly. On the other hand, we do not really understand what is meant by the reviewer when arguing that: "Maybe it would be better to focus on some aspects of the processes described in order to achieve clarity and remove the impression of vagueness, both in the results and in the conclusions, which should be clearer and not indefinite, as they appear.". We understand that this manuscript contains quite a lot of data, however it seems very difficult to focus more on, for instance, phytoplankton metabolism without fully discussing the effects of dust/warming/acidification on bacterial activities. As we do not know exactly what was suggested, and as this issue was not raised by the other reviewers, we will keep all data as presented in the submitted version of the manuscript.

First of all I wish to congratulate the authors for the huge quantity of data and measurements they have presented, which makes the article interesting and stimulating.

Many thanks, much appreciated.

Nonetheless some observations are to be made in order to improve the manuscript and make its reading and interpretation easier.

The data presented are maybe too many and the entire manuscript is too long and difficult to follow.

Maybe it would be better to focus on some aspects of the processes described in order to achieve clarity and remove the impression of vagueness, both in the results and in the conclusions, which should be clearer and not indefinite, as they appear.

See general comment above.

The paper often reminds to the other article (Gazeau et al. 2020) where the general situation of the stations studied is described, but this makes difficult for the reader to have a clear image of the situation, unless studying the other article. A very short description could be useful, especially for the phytoplanktonic community, and a short description of the different characteristics of the three stations, as the authors say the change in the community depends on their initial state.

A brief paragraph will be added at the start of the discussion section to present the biogeochemical characteristics of the tested waters as discussed more in detail in Gazeau et al. (2020). However, as mentioned by several reviewers, the manuscript is already quite lengthy ("too long and difficult to follow" as written by the reviewer) and we would like to keep this section, which is fully detailed in the companion paper, as short as possible.

Data show a general great variability, even between the replicates, as the authors themselves underline. Is there an explanation for this? In certain cases it is quite high and might invalidate the whole experiment. If there isn't a reason to keep the replicates separate, cannot you consider to average the two replicates? This can help in the understanding of the figures, especially fig. 2, 3, 4, 6, 7.

Axis title should be enlarged to simplify the reading.

We acknowledge that variability is, for some parameters and experiments, at least visible. The choice for a duplicate approach is fully discussed in the companion paper (unfortunately still not readable in its final revised version...); section 4.2. Critical assessment of the experimental system and methodology: "The relatively low number of experimental units that could be installed inside an embarkable clean container restrained our possibility to consider more than two replicates per treatment. Fortunately, as already said, differences between duplicates were, for the vast majority of studied variables and processes, lower than differences between treatments and appear acceptable considering the difficulty to incubate plankton communities for which slight differences in their initial composition can translate into very important differences in dynamics (Eggers et al., 2014)."

Unfortunately, duplicates do not allow averaging and even less calculating standard deviations which will lead to an obvious mathematical error and a loss of information (we do not want to hide this variability).

Larger figures will be provided in the revised version in order to improve visibility.

The projection of what the dust impact could be in the future is very interesting, but clearer conclusions should be addressed.

We agree with this assessment that is shared by the other referees and will clarify and extend the conclusion section in the revised version of the manuscript (see reply to RC1 for more details).

Finally, the paper presents data of current interest, and deserves a clearer presentation, so it is worthy publication in Biogeoscience with the improvement suggested.

Many thanks again for your positive comments.