Biogeosciences Discuss., author comment AC1
https://doi.org/10.5194/bg-2021-20-AC1, 2021

Reply on RC1

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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., https://doi.org/10.5194/bg-2021-20-AC1, 2021

Reviewer#1

We would like to thank Anonymous Referee #1 for her/his comments and suggestions on our manuscript. We acknowledge that the conclusion section of our submitted manuscript requires improvements. We will do our best to revise the manuscript accordingly. The suggestions for future work from the reviewer are very interesting and stimulating.

Based on in situ experiments, this paper aims to quantify the impact of global change on the microbial food web and carbon export in an oligotrophic environment (Mediterranean Sea) during atmospheric dust deposition. The hypothesis underlying this work considers that an increase in temperature and a decrease in pH should reduce the intensity of the oceanic biological carbon sink (i.e. export production), especially in oligotrophic systems.

'Mesocosm' experiments have been undertaken in three different regions of the Mediterranean Sea (Ionian Sea, Tyrrhenian Sea and Algerian Basin) following an East-West gradient of production regimes. Each experiment is built on three distinct forcing conditions with small mesocosm duplicates, during 4 days: a reference (C), a realistic dust pulse (D), a realistic dust pulse together with a 3°C temperature increase and a 0.3 pH unit decrease. Numerous observations on stocks, fluxes, and physiological parameters are made during these incubations, and the discussion is based on these observations. A companion paper presents the experimental design and discusses the results in terms of stocks, while this paper discusses fluxes and food web functioning.

The main message of the article is twofold: the impact of a dust disturbance depends drastically on the initial state of the microbial food web, and anthropogenic modifications (temperature, pH) only slightly modify the impact of this dust addition.

The article is interesting, even if the presentation of the numerous results (three experimental conditions in three different regions) can be difficult to follow. Indeed, due to the multiplicity of simulated situations and the rather small number of similar experiments (in terms of statistical robustness), the discussion is obviously complicated, with each experiment deserving specific comments. Nevertheless, within the limits of the proposed exercise, the two main conclusions of this work are robust.

There is not much to say about the data and the extensive and interesting discussion it prompts, very often based on realistic and specific hypotheses based on clues provided by the data, but which cannot be rigorously tested. Considering that this type of work is time consuming and quite expensive, the article could have ended with less "lazy" and general conclusions. In fact, the two main results of the article should have led to more directed conclusions. As an example, two sentences are rather vague, do not really come from the work presented, and do not really provide any new information.

"observations over longer temporal scales are probably required to ascertain the interactive effects of these stressors in the coming decades": What types of observations and what time scales? Are the authors interested in long time series and how do they relate to this work, i.e. short-term mesocosm experiments? Very provocatively, are these experiments useful? How do you link the different time scales ?

We fully agree that this conclusion is neither clear nor relevant. We propose to replace that sentence by:

"Although a longer experimental period would likely be necessary to clearly support an impact of future conditions on export, those changes occur on a long time scale that cannot be easily mimicked by experimental approaches. Only in situ co-located observations (atmospheric flux/export in sediment traps) over long temporal scales would be necessary to ascertain the interactive effects of these stressors at the decadal time scale."

"As a consequence of a stronger sensitivity of heterotrophic prokaryotes to temperature and/or pH, the ongoing warming and acidification of the surface ocean will result in a decrease of the dust fertilization of phytoplankton in the coming decades and a weakening the CO_2 sequestration capacity of the surface oligotrophic ocean". This is the last sentence in the article. It does not really come from the results presented here, which in fact show the opposite (no change in exports).

We do not fully agree with the reviewer here. Our results clearly show a decrease in net metabolism with warming/acidification. Although these results could not be linked to a decrease in export efficiency, most likely due to experimental bias, e.g. small fluxes compared to those related to ballasting from dust, a decrease in net metabolism undoubtedly involves a decrease in the capacity of surface seawater to pump atmospheric CO_2. The sentence was modified to make it clearer that we are referring to absorption of atmospheric CO_2 by surface waters.

On the other hand, the two main conclusions of the article should raise interesting questions for future work:

- Important role of the initial state of the microbial food web: how to address this major issue and how to draw more general conclusions about the impact of the initial state of the ecosystem? What kind of experiments or in situ observations? Is it necessary and how to obtain the preconditions for the experiments or observations, the history of the food web? It is impossible to get the whole picture if this question about the initial state is not addressed (it is not possible to perform an infinite number of experiments, and a rational approach must be proposed). It will not be possible to address the question of the minimum amount of dust (nutrients) released to obtain a "positive" response from the microbial ecosystem (i.e., an increase in export) without addressing, according to the conclusion of the paper, in a more generic way the competition between autotrophs and bacteria for nutrients, and the link with temperature/metabolism. Besides theoretical considerations and possibly modeling approaches, what to do in terms of observations and experiments?

We agree that multiplying the number of experiments will not solve the issue, this is why we choose to perform the 'same' experiment in different waters. Doing this, our results could be translated into process parameterization made possible by the variety of tested waters, environmental stressors and responses. Such parameterization shall be used in biogeochemical models coupled to ocean dynamics that can depict the spatial and temporal impacts following a deposition in surface waters which biogeochemical properties is dependent on many factors (including successive dust depositions that can be tested in the model and degree of oligotrophy). This small paragraph will be added to the revised version.

- Contrary to the expectations, the results of the experiments presented in the paper are not really convincing, as there is no real impact of global change on the export. If the basic hypothesis holds (e.g. there should have an impact in oligotrophic regimes), a different approach could be proposed, either in terms of experiments (number of tanks to get robust results, with the issue of feasibility, length of the experiments, types of observations) or in terms of sampling different regions and foodweb structures.

There is also the problem raised by multi-stress experiments. The combinatorial analysis makes the number of potential experiments impossible to address, if there are more than two/three stressors. Although the experiment with only a pH perturbation (or a temperature perturbation) was not undertaken, as the study was already quite cumbersome, the work seems to show that pH plays a minor role compared to temperature. Is there anything to be learned from this result (absolute necessity to do an experiment with only a pH perturbation, or to focus on temperature only, or on temperature and a possible additional stressor)?

We do not fully agree with the reviewer but this shows that this paragraph was not clear and will be modified accordingly. As we did not discriminate pH from temperature effects (they occur together), we cannot conclude that our "work seems to show that pH plays a minor role compared to temperature". We still believe that a dedicated study is needed to discriminate temperature from pH effects although our working hypothesis would be that temperature exerts a much stronger control on community functioning than pH (as shown in our previous study from Maugendre et al. 2015). Our experimental system now comprises 9 minicosms and we could imagine in the near future running an experiment with duplicated tanks (triplicates for controls) and 4 conditions.

The richness of the study results from the large number of parameters (stocks and flows) measured. Since the work is aimed at the export flux, a discussion based on eratio could provide a complementary perspective. As the experiments are obviously not steady state, since they are perturbation experiments, this may not be easy to do, but a balance (integrated in time during the duration of each experiment) of the different fluxes, i.e. network pathways, between the main boxes could be presented to get an overall view of the microbial functioning, and the differences between the three ecosystems.This could possibly be the objective of a complementary article.

We agree. We initially had the objective to establish a budget and present it in this manuscript. However, we felt that it already contained quite a lot of information and we finally came to the same conclusion as the reviewer that this should be the objective of a complementary article.

Last two remarks:

- Line 301: "Gross primary production (GPP) was calculated as the difference between NCP and CR". It is the sum, and not the difference.

As mentioned in the text : NCP and CR were estimated by regressing O2 values against time, and ***CR was expressed as negative values****.*

Therefore, NCP = GPP + CR, and GPP = NCP - CR

- Figure 9: it seems that this figure is not consistent with Fig. 10 of the companion paper, at least for PP and BP.

The reviewer is correct, however the two plots are not showing the same data. Fig. 10 of the companion paper is showing the "Maximal relative change (%)" while in this figure we show the "Relative difference (%) between integrated rates". We understand this might be confusing but we felt it was important in this second paper to show differences of the integrated rates instead of maximal differences.

In conclusion, the data obtained during this cruise are very rich and valuable, despite the small number of replicated experiments, the discussion, although sometimes lengthy, is interesting, the conclusions are robust, and the paper should be published with small modifications (checks of Figure 9 against Figure 10 of the companion paper should be made, if there is indeed an inconsistency). Nevertheless, one may regret that the conclusion is rather vague and agreed upon, although this work opens up real challenges and important scientific questions.

Many thanks again. We will do our best to improve the conclusion section as suggested.