

Interactive comment on “Impact of dust enrichment on Mediterranean plankton communities under present and future conditions of pH and temperature: an experimental overview” by Frédéric Gazeau et al.

Anonymous Referee #1

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General comments: I consider the contribution by Gazeau et al., to be an extremely interesting and relevant study attempting at addressing important questions regarding the role of atmospheric dust deposition as an alternative source of bioavailable nutrients for ocean productivity in the warmer and more acidified ocean projected for the future.

The design of the experiment is excellent and very well-conceived. I particularly like the fact that it was run at 3 distinct environmental settings along a W-E gradient, providing an enormous potential for exploring changes/differences related to spatial/regional

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variability along the Mediterranean Sea.

This important study highlights how complex and diversified can be the responses of distinct biological groups to environmental variability, in this case, dust-born nutrient addition and ocean warming and acidification. Results from this work stress the urgent need for monitoring modern marine ecosystems and their spatiotemporal relationship to environmental parameters in order to better understand how they are/will respond to climate change. The study submitted by Gazeau et al., clearly has the potential to provide a robust discussion on these important and timely topics.

However, in its present form, the manuscript is not ready yet for publication, whereas a major revision and structure reorganization is recommended. Overall, I find that the paper lacks focus, and the interpretation of the results could benefit from some more maturation. The discussion, in particular, is hard to follow, bringing difficulties in getting a clear overview on how the different abiotic and biological parameters varied throughout time and space, and how the several hypotheses proposed for the observed trends are entangled/linked. The ideas could be better organized and entangled: whether you decide to compare each variable along different treatments/sites, or you decide to compare different sites in relation to all the variables (i.e. you provide a “general and integrated picture” for each site and there compare the three), whatever logic you use, please stick with it along the entire ms. for consistency. Overall, the main structure of the discussion could be organized in terms of the main differences abiotic and biotic observed along W-E gradient, and how such gradients in the initial/original in situ conditions are likely to modulate/be modulated by dust deposition and ocean warming and acidification.

Given the larger number of parameters and biological groups, for which you used different sub-sampling and analytical approaches, the set-up and procedure should be described more clearly, maybe adding an experiment schematic timeline.

The manuscript could also benefit from a review in terms of written English.

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Specific comments:

Section 4.1 The discussion would greatly benefit from being shortened and written in a more focused manner, instead of often repeating the results and providing too many unnecessary details that prevent the reader from getting the “general picture”.

You should (a) provide a clear and focused global picture of the initial conditions along the FAST-TYR-ION transect, while (b) directly addressing potential causes/explanations for the most important/relevant patterns and observations described in the results.

Section 4.2 I understand that the authors are discussing the strengths and limitations of their experiment, but I am not sure if they need an entire section for this, and especially not before discussing the results. What matters the most is the “story” they can tell from their spatiotemporal experiment, and what kind of insights on the effects of dust deposition and ocean warming/acidification they can extract from this story.

Section 4.3 Overall, this section could also benefit from more focus. The arguments should be better entangled while comparing a) different treatments, b) different sites, and c) with previous studies. Some parts are very confusing and difficult to follow.

It is not easy for the reader to reach the end of this section having a clear picture on which groups benefited the most from dust, where and why, and how that translated into the whole ocean trophic chain and export of organic matter. Although this is the center-theme of the paper, it is not written in a very clear/straightforward manner. The authors should be able to discuss the difference between dust deposition along a W-E gradient along which the abiotic conditions vary. Potential impacts for the biological carbon pump could also be discussed.

Section 4.4 This section has similar issues than the previous sections, concerning its somewhat confusing and poorly organized writing/discussion. Furthermore, I also find it very similar to what is presented in the results. Following a clear and to-the-point

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presentation of the most striking differences between D and G treatments, the authors should take the chance to really discuss/reflect on the meaning of such differences in the light of ongoing ocean warming and acidification. The section revolves too much about details and differences amongst different sites/treatments but does not provide an actual “big picture” of the experiment nor a reflection on what the latter means.

Conclusion: I like this chapter because it is written following a more systematic, logic and to-the-point order. The discussion chapters would greatly benefit from being re-written in a similar manner. In order to do that I would suggest the following approach:

- present a clear discussion of the initial conditions: you need to provide a clear abiotic and biotic spatial picture of your experiment. The environmental background is often not even mentioned. This is a pity since you have data for comparing three distinct environmental settings along a W-E gradient. A good discussion on the differences amongst different stations following dust/warming/acidification should take into account the environmental differences in their initial conditions.

- present a clear discussion on the effects of dust compared to the control, taking into account the differences in the initial conditions amongst the three sites. Differences between duplicates in each station and the potential causes for them should be referred after. Take the chance to discuss important questions such as: will increasing dust deposition increase ocean productivity, CO₂ fixation and CO₂ sequestration? If so, which groups are likely to contribute the most and where? Are dust effects likely to influence the entire marine trophic chain or not? Etc.

- present a clear discussion on the effects of dust in the projected future ocean compared to the modern ocean. Are the differences in the initial conditions a relevant factor for the observed differences and/or similarities? Can you say anything about ocean productivity and the biological pump of the future based on your observations? Will dust help to counterbalance the nutrient-depletion from ocean warming? What can you say about the effects of acidification on calcifying groups and does that matter?

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Etc.

Abstract Line 26: caused by climate-driven enhanced stratification

Line 29: the potential impact (. . .) was investigated

Line 36: maybe “analysis” instead of “processes”?

Line 38: impacts of dust seeding with and without addressing the effects of climate change

Line 44: I would provide more information regarding such differences and regarding the conditions at the three studied locations.

I would include more detailed information on which “biological stocks” you refer to, and which approach/method you used to explore the different biological groups.

Introduction Line 59: Please specify whether this Chl-a concentration refers to the surface, DCM or an average for the entire photic zone

Line 65: can you explain/frame what are dust emissions from anthropogenic origin?

Line 76: how did these authors justify this lack of agreement?

Line 62-76: I would break this paragraph in two.

Line 78: in that part of the basin

Line 78-80: present a higher impact as a source of bioavailable fertilizing nutrients compared to dry deposition, as confirmed on. . .

Line 79-80: which parameters and processes were those?

Line 81: using both micro- and mesocosms

Line 83: while also modifying. . .

Line 84: In addition, besides. . . also modified the. . .

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Line 87: can you explain what you mean with “budgets”?

Line 89: ... heterotrophic biological behavior in these oligotrophic waters

Line 94: how can dust aggregate and ballast OM that is dissolved?

Line 101: ...nutrient cycling in the open ocean is being and will continue to be perturbed in the next decades, very likely to result in regionally variable impacts. . .

Line 104: ... due to thermal stratification-linked reduction of nutrient supply... (...) to which an increasing role of atmospheric dust deposition might contribute to compensate as an alternative source. . . .

Line 106: Whether or not plankton communities will respond differently to dust deposition under more acid and stratified conditions due to ongoing ocean warming and acidification, both globally and regionally in the Mediterranean Sea (refs.) is still largely unknown.

Line 111: this sentence sounds strange; I would never expect that severe nutrient limitation and enhanced warming would ever lead to increase PP... or did you mean heterotrophic production?

Line 119: what about calcifying plankton, which have an important goal for both the organic and inorganic carbon pumps?

Line 144: this “and/or” is not very clear. . . .

Line 145-147: do you mean other papers which were also submitted to this BG issue? “companion paper” does not sound well. . . .

2. Material and Methods 2.1. General setup

Line 150: Six experimental tanks were installed in a T-controlled container, allowing to finely control the irradiance spectrum and intensity to fully reproduce a future scenario of enhanced ocean acidification and warming conditions.

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Line 153: were trace-metal free? Or ARE trace-metal free?

Line 180: how long did it take between sub-sampling for initial conditions and dust seeding? Can you trust that the conditions inside the tanks did not change during this interval (nutrient-consumption, grazing, OM degradation, etc)?

Line 188: “for many parameters” sounds a bit vague..

Line 205: I would only show the parameters that are used in the results/discussion of this paper.

Line 205: are 3 days enough? What were your criteria to define the time-interval for the experiments? Which biological groups were you focusing on? It would be interesting to monitor the ecological succession of distinct plankton groups following the addition of dust.

Line 210: what do you mean by filtered “online”?

2.2. Analytical methods 2.2.2. Nutrients

Line 239: again the expression “filtered online”.

2.2.4. Flow cytometry

Line 269: did you get any to details on the taxonomy of autotrophic nanoplankton?

2.2.5. Micro-phytoplankton and -heterotrophs

Line 284: If the filling of the tanks took 2h, and if you have sampled such surface seawater while filling the tanks... why do you call this sampling time “t-12h”? The temporal sequence of the (1) filling, (2) filled-tanks, (3) dust seeding, (4) sub-sampling moments, and (5) which parameters you sub-sampled and when, is not fully clear to me...

2.2.6. Mesozooplankton Line 290: why was FAST longer? When exactly was the dust added to the tanks? T?

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2.3. Computation What do you mean by “Computation”? Line 303: The maximum percentage of dust-born dissolved N and P. . . Line 311: How many times did you sub-sample for nutrients over those 6h following dust seeding?

Line 312: not sure I understand the way you determine the CONCdust. . .

Line 325: . . . unites was done . . .

Line 331: what about Chl-a?

3. Results 3.1. Initial conditions

Line 335: the part: “when pumping seawater for the experiments” sounds repetition of the first sentence. Just directly start describing what were the initial conditions.

Line 365: maybe this section should be titled “Experimental conditions at t0” Line 366: in the control tanks Lines 388-391: this part is not fully clear. Please specify to which tanks/treatments you added 13C and dust, and how was the sequence (which did you add first?)

Line 425: At TYR, while concentrations remained stable in control tanks, . . .

Line 437: . . . temporal dynamics showed very different patterns amongst the three studied stations.

Line 449: I would replace “at the exception of” by “with the exception of” throughout the ms.

4. Discussion 4.1. Initial conditions

Line 499: typical stratified

Line 500: please specify which period of the year you are referring to.

Line 501: . . . were even lower than the ones. . . during spring

Lines 514 and 515: please replace “whether. . . or” by “both “. . . and”

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Lines 498-516: Maybe you don't need to provide such a long description given that your nutrient values are not directly comparable to those from the referred studies. It might be enough if you simply argue that in spite of not being possible to directly compare them, your results suggest highly oligotrophic conditions at the start of the experiment, in line with previous studies in the region and neighbor areas, and as typically expected for the open Mediterranean sea during spring. Basically, what you eventually say in lines 516-517.

Line 520: what do you mean by "enrichment experiments"? you mean experiments dealing with enrichment in DIP, nitrates, etc? And combination of what? Please explain.

Lines 520-527: it is not clear whether you are referring to experiments that you did or whether you are referring to studies by other authors (refs?). If I understood correctly, you are saying that productivity in the initial conditions were marked by N- and P-limitation but not by dFe...?

Line 528: Total concentrations of Chl-a were in line with low Chl-a levels found. both driven from remote sensing satellite images ... and from in situ measurements provided in a database from.

Line 531: ... low Chl-a concentrations around. . .

Lines 533-538: this part sounds more like a description/repetition of the results rather than its actual discussion.

Line 540: ... a fingerprint of LNLC areas in general, and of surface Mediterranean waters during spring in particular.

Line 554: you should only cite works "submitted" or refer to them as "unpublished data" or "person. communication"

This section could benefit from being shortened and much more focused. As it is, the arguments are not easy to follow (often presented in "circles") making it hard to get a clear picture of what were in fact the initial conditions in the studied sites. For more

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details, see the comments below.

Line 568: Not sure if the title fits the content of this section; I would maybe call it “Environmental conditions during the experiment” or something.

Line 569: . . . have been successfully validated in previous studies. . . to investigate the biological effects from the input of. . . and resulting export of organic matter. . . under pre-defined close-to-abiotic conditions,

Line 574: which ones ate “these”? please clarify.

Lines 574-575: . . . no control of atmospheric CO₂ was, however, performed, resulting in a rapid increase of the pH levels in the acidified filtered seawater due to CO₂ outgassing. . .

Line 576: In order to avoid this, we improved. . . reactors). This allowed us to significantly reduce CO₂ outgassing while maintaining pH levels. . .

Line 580: Still, as illustrated in Fig. 5, the regulation of atmospheric CO₂ was. . . compared to G1, possibly due to a potential. . .

Line 583: I know what you mean here, but this should be said in a different way and properly supported by the relevant results.

Line 588-592: too much descriptive. Please be more concise and directly address the potential causes for the observed discrepancies.

If the lids are the same, why would the intensity vary? And what do you mean by “PARs sensors sensitivity”? Are you referring to enhanced levels of turbidity when you refer to “amount of particles”? And why only for treatments G?

Line 597: you had the same type of unforeseen variability in terms of temperature? Only for G, or to all the treatments? This part should be better explained and discussed to what extent it might lead to misleading conclusions.

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Line 599-600: "... but the new results are not yet available" (I am not sure what you want to state in the last sentence....)

Line 601: this is already well established at this point, there is no need to repeat this.

Line 605-612: Some of these sentences are too long. I am also not sure if this "introspection" about the criteria/available conditions for the design of your experiment is really relevant here. I would focus on providing a good discussion on the conditions prevailing during the experiment, how much they mimicked the settings that you wanted to mimic, how well the experiment conditions were in line with previous similar studies, and what is it that is new. You can refer to the main difficulties and challenges during its implementation, but always having in mind whether the latter allowed you or not to realistically (enough?) simulate/test your hypothesis. You should stick to discuss the quality of your experiment based on your observations/results. Anything other than that creates dispersion and makes the reading more difficult.

613-616: I understand this! However, this highlights the importance of providing a clear, to-the-point description discussion of the initial conditions, such that the reader can easily perceive what has changed throughout the experiment and whether such changes might "modulate" the final results. I would expect that the time interval between filling the tanks and the start of the experiment could have exerted some effect in changing the initial conditions amongst different sites. But assuming that the 6 tanks were filled at the same time at each site, it is important to explain the reason why only 1 of the tanks/treatments changed.

Line 625-629: I don't understand what you mean here.

Line 638: I wouldn't say that it was "opposite" since DIP also decreased, only more abruptly compared top NOx.

Line 642: a dry Saharan dust deposition event was simulated. . .

Line 643-645: is it relevant to refer/repeat this? I would rather take this chance to

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argument that your results appear to confirm previous notions that wet dust deposition is a more efficient source of bioavailable nutrients compared to dry dust deposition. . .

Line 645: Furthermore, based on previous studies reporting the During the atmospheric transport of dust particles. . .

Line 650-654: this could be better explained. . .

Line 657: I would discuss first the drastic differences that you have found amongst the 3 sites before start comparing your observations with previous studies. The first sentence does not seem related to the sentence where you refer the study from Guieu and Ridame, 2020).

Line 659: I think you can safely say that the fertilization effects were much higher at ION and FAST compared to Guieu and Ridame, 2020.

Line 662: . . . with the largest NO_x decrease observed in our study, which occurred at station FAST.

Line 668: . . . that, based in the analysis of several aerosol addition studies, *Synechococcus* had generally weak responses to aerosol addition. . .

Line 673: do you mean that *Synechococcus* was the group that increased the most in ION and FAST, compared to pico- and nano-eukaryotes?

Line 682: this is not clear: at the end of the experiment or following dust addition?

Line 689-692: It is not clear whether you are referring to clear evidences from your study or to info previously published. And what do you mean by “bacterial variability” in this context?

Line 685-718: I find this entire part very confusing and difficult to follow. . . please be more straight-to-the-point in what you think might be the explanation(s) for the lack of autotrophic response vs. positive heterotrophophic response to dust. And why was this the case for TYR but not FAST and ION. . . ?

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Line 725: a similar enhancement?

Line 748: what do you mean by an “abiotic dust experiment”?

Lines 757-758: This sounds contradictory with your previous sentence. If I understood correctly, you are saying that (A) while ocean warming and acidification do not seem to play any role on the amount of nutrient release from wet dust deposition in all three sites, (B) there are however differences amongst FAST, TYR and ION regarding the biological response to dust-born nutrients under ambient vs. warmer/acidified conditions. If this is the case, please explain it more clearly in the ms. And please justify/illustrate why you argue (A).

Line 760: why do you think you had larger variability amongst the duplicates at ION and TYR, but not at FAST? Was it due to methodological issues/limitations and/or to natural causes?

Line 763: I think you should discuss why you had enhanced biological stocks and metabolic rates at FAST under warm/acid conditions compared to ambient conditions before you start discussing why the differences in the DIP dynamics are harder to explain. . .

Line 763-771: this part is not clear. First you say that DIP was fully consumed until the end of the experiment at all 3 sites under future conditions, in contrast to ambient conditions. Immediately following, you seem to contradict yourself by saying that DIP dynamics at ION was similar between D and G treatments after all. . . . So, was there a difference in the DIP dynamics at all three sites or not?

Line 770: so you are saying that the dust-driven DIP was consumed much faster at TYR and FAST under future conditions compared to the present?

Line 774: throughout the ms. you often refer to “in preparation, this issue”. If it is still in preparation than it is not “this issue”. . . only after being submitted.

Line 786-787: are you referring to previous studies now? Please improve the transition

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from your data to previous studies and vice-versa throughout the ms.

Line 795-797: Why is this evidenced by the lack of changes in the meso-zooplankton abundance? How exactly are the two related?

Line 810: why “excess production” instead of “enhanced production”?

Line 834: do you mean “under initial conditions”?

FIGURES

Fig. 1 – I would include lat/long in the location figure. The labels of most of the figures could be enlarged. Table 1 – It is not clear when exactly you have introduced the dust. Also, regarding the “related manuscripts”: do you mean “in preparation” or “submitted”? Not sure you can/should quote studies that haven’t been submitted yet.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-202>, 2020.

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