

Biogeosciences Discuss., author comment AC4  
<https://doi.org/10.5194/bg-2021-20-AC4>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Reply on RC4

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., <https://doi.org/10.5194/bg-2021-20-AC4>, 2021

---

*We would like to thank Anonymous Referee #4 for her/his comments and suggestions on our manuscript. We agree with most comments and modified/updated the manuscript accordingly. Below is a point-by-point reply. Briefly, the reviewer would like to see more discussion on the export. We have tried to clarify some points related to the protocol used to recover the traps and we now give more details on the fractions that were exported. Nevertheless, we do not want in this manuscript to go into much detail on the potential relationships between TEPs and organic matter export, as this manuscript contains already a lot of data (as highlighted by several reviewers) and we are currently working on a manuscript fully dedicated to this. We would like to thank the reviewer for highlighting a very important mistake in Fig. 3. T-12 data were wrong for all three stations, we believe this comes from a wrong treatment of the dataset to draw the plot (copy-paste). The data shown are now coherent with values indicated in Table 1. We apologize for this mistake.*

### General comments

The presented work provides valuable insights into the short-term response (within 72h or 96h) of the plankton community to dust input in oligotrophic low nutrient, low chlorophyll waters of the Mediterranean sea under present-day and future conditions. The authors show the time-evolution of the response leading to i) either a shift towards even stronger net-heterotrophy or, with a time lag of 2 days, ii) to a shift towards net-autotrophy of the plankton community.

In general, the authors present a rich dataset which was part of an even bigger effort, the PEACETIME project, to shed light on the role of dust input into Mediterranean waters.

The manuscript is well structured. However, I believe, the material could be further condensed and the readability and clarity increased. For example, often 'this' and 'that' are used. I suggest to be explicit to what you refer to make it easier for the reader. I tried to highlight some cases, where such clarification is needed in the specific comments - but

please consider to go through the whole text.

I have two major concerns with the presented material:

- While being aware about the enormous amount of work and the limited ship time to gain such dataset, the limited number of replicates of the experiments (using only duplicates instead of e.g. triplicates) makes it hard to draw statistically meaningful conclusions. This limits the value of the otherwise valuable experimental setup and study. I believe, this should be considered in potential follow up experiments.

*We obviously agree with the reviewer. At that time, our experimental system comprised 6 tanks and did not allow considering triplicates. We have recently developed a new clean container which contains 9 tanks and will allow considering 3 treatments in triplicates.*

- The title promises to provide insights into the response of carbon export to dust input.

First, a time evolution of export fluxes would have been certainly of value, which should be considered in a future application of the experimental design.

*Our experimental system allows for an easy collecting of sediment traps which could be done on a daily basis. This was not considered for this study as we expected the fluxes to be too low to be precisely quantified on a daily basis. We definitely agree with the reviewer that this should be considered for a future study.*

Second, a few more information and discussion on the tank design and its (potential) effect on particulate fluxes would be helpful (e.g. is the energy input comparable to typical dust event situations, how does the circulation affect aggregation dynamics, do you account for ongoing grazing and remineralization in the 'sediment trap' of the tank, do you expect TEP to collect at the surface - away from your sampling valve - if so, in how far does it matter for export?).

Third, I miss data and discussion on observables mentioned in the methods part, i.e. total carbon, lithogenic and biogenic silicates and calcium in the exported material.

*As indicated in section 2.3.5, those analyses were used to quantify the different fractions (Ca was used to determine CaCO<sub>3</sub>, a fraction that was also determined by difference between (total C and POC), similar concentrations were found. Lithogenic fraction was determined using LSi and the same proportion of lithogenic was found when calculated as*

*(total mass - organic matter + CaCO<sub>3</sub> + opal).*

*We added to figure 8 the distinction between the different fractions?*

*(see also detailed response concerning contrasted efficiency dust export in another comment below and added text).*

Further, I would have expected a stronger discussion on the aspect of export, e.g. also with regards to the ratio between POC and TEP-C production and export flux. It also seems as if there could be a relation between the community state and export flux.

We agree with the reviewer that this would be interesting to link production of TEPs to export flux. We did not want to go into too much details in this manuscript that is already quite long and contains lots of data. We are currently working on a manuscript (Guieu et al., in prep.) that gathers all data acquired from dust additions experiments including those during the PEACETIME cruise, and in situ observations to better understand POC export following dust deposition in the ocean. In particular, we focus on the lithogenic carbon pump that is responsible for a significant POC export in all studied cases and strongly linked to TEPs production.

Specific comments on the text

p.2, l.42-44 - A bit more explanation is needed on DOM as precursors for TEPs and thus aggregation of minerals - the connection between DOM and aggregation of dust particles is not immediately clear

We added this sentence to the abstract: "At ION and FAST, the efficiency of organic matter export due to mineral/organic aggregation processes was lower than at TYR and likely related to a lower quantity/age of dissolved organic matter present at the time of the seeding and a smaller production of DOM following dust addition. This was also reflected by lower initial concentrations in transparent exopolymer particles (TEP) and a lower increase in TEP concentrations following the dust addition, as compared to TYR."

p.3, l.48 - What is 'This potential. . .' referring to? - be explicit.

Corrected to: "this impact"

p.4,l.56-57 - . . . (Longhurst et al. 1995). Although phytoplankton production in LNL areas is limited . . .

Corrected to: "Low Nutrient Low Chlorophyll (LNLC) areas represent 60% of the global ocean surface area (Longhurst et al., 1995). Although phytoplankton production in these areas is limited by the availability of nitrogen, phosphorus and iron, it accounts for 50% of global carbon export (Emerson et al., 1997)."

p.4,l.60 - ...in LNLC regions and as such...

Corrected

p.5,l.85 - . . . Ridame and Guieu, 2020). However, no clear . . .

Corrected

p.6,l.108 - Since you refer to biological remineralization processes being affected by ocean warming, write ' . . . weaken the ocean biological CO2 sink in the future . . . ' (to not confuse it with physical effects on CO2 uptake).

Added

p.6,l.117-120 - . . . trophic levels. Their study was conducted under nutrient-depleted conditions (Maugendre et al., 2017b). Hence, there is still a need . . . nutrient availability.

Corrected

p.6,l.151 - I have a number of questions regarding the setup of the tanks:

- How efficient is sedimented material transported to the sediment trap? Or got material stuck to the tilted side walls and wasn't captured?

*We made sure that no material remained in the tanks while collecting the sediment collection bottles. In case some material was observed, by tapping the walls from the*

*outside, this procedure was enough to collect all the material in the traps.*

- Was the sediment trap somehow poisoned to avoid remineralization and grazing on settled material? Or do you underestimate POC sedimentation fluxes?

*The sediment traps were not poisoned, we felt it was way too risky in case of leakage for the communities.*

- How large (its area) was the propeller used, which direction of flow field was induced and how much energy was put in - how does the induced mixing rates compare to in situ conditions (also under wet dust input conditions)? And to phrase it broader: What is the propeller effect on the sedimentation flux?

*On board R/Vs, the main turbulence comes from the movement of the boat and the propellers may only have a slight effect compared to in situ dynamics. Those systems are also used for on-land experiments and the exact same gentle turbulence effect being applied to all the tanks, allows for the comparison of similar turbulence conditions in the different treatments. We agree that for in land experiments, some tests should be done to properly quantify the induced turbulence and the impact on the processes including the export.*

p.9,l.169 - Can you briefly mention the mean/median grain size?

*Following R#3 advice, we have added a paragraph describing the dust composition in the Material and Methods.*

p.12,l.239 - ...regimes as in the ...

Corrected

p.13,l.264 - We followed the time evolution . . .

Corrected

p.14,l.275 - . . . frequency as for . . .

Corrected

p.14,l.284 - Ref. for standard needed

The standard used was caffeine (IAEA-600; <https://nucleus.iaea.org/sites/ReferenceMaterials/Pages/IAEA-600.aspx>), this was added to the text.

p.18,l.357 - Which stoichiometry are you assuming to calculate the factor two? Or provide a reference.

Klaas and Archer, 2002 added in the text

Klaas, C., & Archer, D. E. (2002). Association of sinking organic matter with various types of mineral ballast in the deep sea: Implications for the rain ratio. *Global Biogeochemical Cycles*, 16(4), 63-1.

p.20,l.387 - I think, a brief description of the general environmental settings in which the experiments were carried out, would help, before going into the details of experimental results. Particularly such information of a pre-occured dust input event at TYR would aid the reader to understand the state of the plankton community.

*Added*

p.21.l.422 - I suspect you mean 'general positive trend' (as opposed to acceleration via an increasing trend)

Absolutely, corrected to positive trend

p.21,l.423 - Here and throughout the text: when speaking about variability, you seem to refer to differences between experiments and not to variability in the statistical sense as deviation from the mean. I suggest to move either to 'differences' or to define variability at the first occurrence as difference between the experiments.

The term variability in our text always refers to differences between duplicates (and not experiments), therefore we believe we can use the term variability.

p.22,l.446 - positive trends (see above)

Corrected

p.22,l.448-449 - what are you referring to? Which final values and 3 % of what?

*Modified to: "The strongest increase was observed at station FAST in tanks G where final TAA/DOC ratios were above 3%."*

p.23,l.455 - 'this parameter'  'DOC concentration' (if I am not mistaken, otherwise please fill with the right parameter name)

*No. "After dust seeding, POC concentrations did not show clear temporal trends for the three experiments although a slight general increase could be observed at station FAST. Furthermore, no impact of dust seeding and warming/acidification could be observed for this parameter." We do not see what is unclear with these sentences, however we could change to: "Furthermore, no impact of dust seeding and warming/acidification could be observed on POC dynamics".*

p.24,l.476-477 - why is it an important discrepancy? - do you somewhere come back to this statement to explain it? In the figure, to my eye, I see two times the same green color, so it's not straight forward to see, which is G1 and G2 (I assume the order matters, but it could be clearer)

*Modified to large discrepancy. The figures with bar plots were modified in order to easily discriminate between duplicates.*

p.24,l.482-485 - I am a bit confused here by increasing versus decreasing values. So the ratio between DOM and POM production shifted towards POM production and therefore the %PER was decreasing?! I guess, the sentence could be written in a clearer manner.

*The initial sentence was: "Although being also positively impacted and increasing with time, dissolved production appeared less sensitive than particulate production leading to an overall decrease of %PER at station ION following dust addition."*

*We propose to reformulate to: "At ION, both particulate and dissolved production increased following dust addition. As this positive impact was stronger for particulate than for dissolved production, this led to an overall decrease of %PER following dust addition."*

p.24,l.484 - at this station  at station ION (please re-check)

*Indeed, modified*

p.24,l.486 - at this station  at station ION (please re-check)

*No need to repeat, specified in the sentence just before.*

p.24,l.489 - at this station  at station ION (please re-check)

*Indeed, modified*

p.24,l.495 - Start with: In contrast to station ION, at station FAST was much less . . .

*Modified to: "However, in contrast to station ION, there was much less impact of warming/acidification on all measured rates at station FAST although ..."*

p.25,l.509 - incorporation into

*Modified*

p.25,l.511 - At station TYR,

*Modified*

p.25,l.512 - under present-day environmental conditions

*Not modified, we refer to present vs future environmental conditions throughout the text, we do not see why using a different terminology here.*

p.26,l.520-521 - maybe add a gray line in FAST at 72h to make comparison easier

*No data are shown at 72 h, we believe it is clear that no measurements have been done for this day at FAST.*

p.27,l.560 - refer again to Tab. 2 at end of sentence.

*Done*

p.28,l.571 - instead of a continuous increase

*Modified*

p.28 - Sec 2.4: I am missing information of how much of the dust was recovered from the 'sediment trap'. Additionally, you were mentioning the measurements of BSi etc. in the methods part and don't show it here (or anywhere else in the manuscript). I wonder why? See also my general comment.

*We agree that important information is missing. In the results section, the following sentences were added and the different fractions are now presented in Fig. 8.*

*"Only less than 30% of the dust introduced at the surface of the tanks were recovered at the end of the experiment (3 or 4 days after) in the sediment traps with TYR>ION>FAST. The composition of the exported material was quite similar for each experiment with no significant difference between D and G treatment with: 3-5% Opal, 4% organic matter, 35-36% CaCO<sub>3</sub> and 48-54% Lithogenic (Fig. 8).*

and after L793:

"The recovery of the introduced dust (traced by the lithogenic mass recovered in the traps) was low (27% at TYR, ~20% at ION and 13-19% at FAST) reflecting that a majority of the dust particles (the smaller ones that are the most abundant according to the particle size distribution of the dust) still remained in the tanks after 3 or 4 days following dust addition. This has been already observed in pelagic mesocosms (Bressac et al., 2012) as those small particles can aggregate to organic matter and eventually sink. The higher export efficiency observed (TYR>ION>FAST) is likely linked to the higher initial abundance and higher production of TEPs during the experiment (Fig. 3).

Ref: Bressac, M., Guieu, C., Doxaran, D., Bourrin, F., Obolensky, G., & Grisoni, J. M. (2012). A mesocosm experiment coupled with optical measurements to assess the fate and sinking of atmospheric particles in clear oligotrophic waters. *Geo-Marine Letters*, 32(2), 153-164.

p.28 - No mentioning and summary of Fig.9 in the results part?

No, this synthesis figure is presented in the discussion section, we believe this is the right place to show it.

p.29, l.599-602 - A rough back-of-the-envelope calculation for the flux induced by the loss of POC at TYR between t-12h and t0 (using an initial POC difference of about 15  $\mu\text{mol C L}^{-1}$  from Fig. 3) under the assumption of a homogeneous water body:

$$F = 280\text{L} \cdot 15\mu\text{molCL}^{-1} \cdot 1\text{mol} \cdot 12\text{gmol}^{-1} \cdot 1000\text{mg} \cdot 1 \cdot 106\mu\text{mol} \cdot 1\text{g} \cdot 0.36\text{m}^2 \cdot 3\text{d} \\ = 46.7 \text{ mg C m}^{-2} \text{ d}^{-1}$$

provides a very different picture from what has been found in the 'sediment trap' of the control tanks, particularly, when also considering the stoichiometry for the POM/POC ratio (which would make a factor of 2 according to your methods part 2.3). This is already of the order of sedimentation fluxes or even higher than in the dust addition experiments. I am a bit concerned about the results related to the export of particulate matter. As already pointed out in the general comments part, a few more checks and a deeper discussion might help.

We would like to thank the reviewer for detecting a very important mistake in our Fig. 3. We do not know what is the cause of this (bad copy-paste or else) but indeed T-12h values were completely wrong and not coherent with the values we indicated in Table 1. This has been fixed.

p.29,l.604-606 - A higher TEP-C content is not really visible in Fig. 3. Given the higher POC content at t0 at TYR for C1/C2, you could potentially even come to the opposite conclusion (i.e. higher TEP-C/POC ratio at FAST).

*TEP-C was on average (all tanks as it was before dust seeding) 2.29, 1.69 and 1.67 micromol C/L at TYR, ION and FAST respectively leading to an overall TEP-C/POC ratio of 0.20, 0.14 and 0.19. We agree that, although TEP-C concentrations were higher at TYR, the "larger contribution to POC" as mentioned in our submitted version is not obvious. We then removed the end of the sentence.*

*"At t0, larger and more abundant TEP were measured at station TYR compared to the two other stations (data not shown)."*

p.30,l.629 - Provided that dust input happened, it seems as if dust input frequency might play a role in determining the evolution of the plankton community. Is there anything known about it (frequency of events, expected changes in future, etc.)?

*We wrote in the introduction that for the Mediterranean specifically: "dust deposition could increase in the future due to desertification (Moulin and Chiapello, 2006), although so far the trend for deposition remains uncertain because the drying of the Mediterranean basin might also induce less wet deposition over the basin (Laurent et al., 2021)."*

*Although it is not clear whether there will be more deposition or not in the Mediterranean, this paper (Kok, J. F., Ward, D. S., Mahowald, N. M., and Evan, A. T.: Global and regional importance of the direct dust-climate feedback, Nat Commun, 9, 241, <https://doi.org/10.1038/s41467-017-02620-y>, 2018.) provides estimates of future dust loading on the global scale and regionally, including the Sahara.*

p.31,l.635 - 'optimal' or 'favorable conditions'? Looking at the dust input experiments, it seems to me that BP was not at its maximum.

*Agreed, favorable is a better term to use here.*

p.31,l.642 - at station FAST, as shown...

*Corrected*

p.32,l.664 - under present-day environmental conditions

*We prefer using present and future throughout the text*

p.32,l.670 - here and throughout the manuscript, specify the main limiting nutrient - not everyone is familiar with the biogeochemistry of the Mediterranean sea

*Done*

p.33,l.678,679,687 - instead of 'this station' specify the station explicitly.

*All corrected*

p.34,l.702-704 - Please explain a bit more detailed.

We do not want here to reproduce what is written in the companion paper from Dinasquet et al. 2021. This manuscript was not available to the reviewer but is now online as a discussion paper (under review).

Dinasquet, J., Bigeard, E., Gazeau, F., Azam, F., Guieu, C., Marañón, E., Ridame, C., Van Wambeke, F., Obernosterer, I., and Baudoux, A.-C.: Impact of dust addition on the microbial food web under present and future conditions of pH and temperature, Biogeosciences Discuss. [preprint], <https://doi.org/10.5194/bg-2021-143>, in review, 2021.

p.35,l.723-725 - Why is it higher under future conditions?

*We do not discuss future conditions in this section. As discussed in the next section, warming affects organisms by enhancing their metabolic rates.*

p.35,l.733 - simply 'more in a steady state and less stressed' or maybe 'more in balance and less stressed'?

*Corrected: "It seems that heterotrophic bacteria and phytoplankton were more in balance*

*and less stressed at the start of the experiment at FAST”.*

p.36,l.754 - I suspect the reference needs to be moved to ' . . . new nutrients (e.g. . . . ) and ' - since the study of Moutin took place before your studies

*Corrected*

p.37,l.777 - more important □ higher (or, in which sense more important?)

*Corrected*

p.38,l.790 - why don't you refer to the decreasing TEP-C as shown in Fig. 3?

*Indeed, this would be better. Corrected.*

p.38,l.799-804 - The reference in line 801 cannot really refer to your experiments, so I would suggest to reformulate this part in terms of what Bressac and Guieu 2013 found and how that relates to your study.

*Agree. The sentence was reformulated (see also comment above), a different reference was given:*

*(sentence L796-801) was replaced by:*

*"The recovery of the introduced dust (traced by the lithogenic mass recovered in the traps) was low (~30% at TYR, ~20% at ION and 15-19% at FAST) reflecting that a majority of the dust particles (the smaller ones that are the most abundant according to the particle size distribution of the dust) still remained in the tanks after 3 or 4 days following dust addition. This has been already observed in pelagic mesocosms (Bressac et al., 2012) as those small particles can aggregate to organic matter and eventually sink. The higher export efficiency observed (TYR>ION>FAST) is likely linked to the higher initial abundance and higher production of TEPs during the experiment (Fig. 3).*

*Ref: Bressac, M., Guieu, C., Doxaran, D., Bourrin, F., Obolensky, G., & Grisoni, J. M. (2012). A mesocosm experiment coupled with optical measurements to assess the fate and sinking of atmospheric particles in clear oligotrophic waters. Geo-Marine Letters, 32(2), 153-164.*

p.39,l.822 - this treatment □ future conditions (or treatment G compared to . . . )

*Corrected: "Similarly, the heterotrophic compartment was more stimulated, as BP rates increased strongly at all stations under future conditions compared to treatment D. "*

p.42,l.899 - weakening of the CO<sub>2</sub>. .

*Corrected.*

Specific comments on figures and tables

Fig.4 I cannot distinguish between the individual tanks. I suspect the order matters. Since you refer to individual tanks in the text, it would be helpful to be able to distinguish between the tanks.

*Figures 4, 6 and 8 have been updated in order to distinguish the two replicates (empty vs full bars). It was less problematic for Fig. 8 (as the names of the tanks are on the x axis, but it is important to be homogenous).*