



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-478**

Anonymous Referee #2

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Referee comment on "Phytoplankton reaction to an intense storm in the north-western Mediterranean Sea" by Stéphanie Barrillon et al., EGU sphere,  
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In this work, Barrillon et al. characterizes the response of phytoplankton to a storm event in the NW Mediterranean Sea. In my opinion, this work is truly relevant. There are few studies out there that compare the before and after of phytoplankton response to short-term anomalous events that disrupt the ecosystem. Since there is a real possibility that such extreme events may become more frequent in the future, there is a dire need for more studies on this topic. Unfortunately, most of these works occur as a reaction to a given extreme event and, thus, lack a comprehensive methodology that may evaluate the impact it had, which is understandable. This is not the case of this manuscript, as Barrillon et al. clearly tried to use as much as they could to characterize this event: in-situ ship-based sampling before and after the storm, a glider sampling during the storm, as well as remote sensing and modelled data to complement the in-situ data. Therefore, this is an important work and a good example on how various sources of data should be integrated to study a short-term event.

While methodology is sound, the writing is overall good and its conclusions are relevant and supported by the results, I do have a few gripes with the manuscript that I believe should be resolved before being accepted. Therefore, for my part, I recommend major revisions.

I will now list my main questions or areas where I think the manuscript could be improved.

- In the introduction, the goals of the work are not explicitly stated. There is a large paragraph detailing the FUMSECK cruise, some overall methodology and its aims, but these are the cruise's aims, not this work's aims. Clearly stating the objectives and linking them with the methodology and results would help the reader navigating through the substantial number of results described in this work.

- For a work in which its conclusions revolve around the “role of storms on the biogeochemistry and ecology of the Mediterranean open sea (...)”, I saw very few references in the introduction to works focusing on other than phytoplankton abundance or biomass changes. For instance, the authors could have discussed other studies that have approached the potential impact of such short-term events on carbon export (e.g., Hamme et al., 2010; Henson et al., 2012, 2013; Ferreira et al., 2022). Regarding this matter, I would be curious to see some remote sensing POC images/data before and after the storm. These may even tie in nicely with the conclusions of the article, if they reveal something interesting.
- Some paragraphs or portions of the manuscript are a bit verbose and could be shortened or even removed. For instance, in lines 53-64, there is an exhaustive description of the FUMSECK cruise. This description could be shortened and most of it could be integrated into the Material and Methods section to avoid redundancy. Also, passages such as 66-69 and 73-76 are redundant. There is no need to state what the results section will show after the material and methods because that will become clear for the readers as they continue to read the manuscript. I recommend looking at such situations across the manuscript to keep the text as straightforward as possible for the reader.
- I think the sampling scheme could be clearer. For instance, there are underway surface water measurements of ADCP, SSS, SST and chl-a via fluorometer throughout the entire cruise (30/04/2019-07/05/2019). Then there is also an MVP which was deployed along seven different transects (only two are shown, as far as I noticed) and again sampled temperature, salinity, chl-a via fluorometer. It also included a plankton counter. Yet we only know the timing and duration of transect 1 (30/April) and 7 (5-6/May). Figure 6 is exhibiting the transects and its measured variables, but this should be clearly stated. Furthermore, while I was reading the manuscript, I was frequently unsure if what is being shown is the temperature/salinity data from the MVP or from the underway system. Inorganic nutrients were also sampled at 26 locations (or, at least, 26 samples were collected) and chl-a again (now via laboratory fluorometer) was sampled at 20 locations, all at surface. Judging from Figure 7, I think most of these chl-a and nutrients samples match, but, again, it should not be necessary for the reader to carefully compare figures and count stations to understanding the overall picture of what was done and how. What are the seven stations presented in Figure 1? Did multiple-variable sampling occur in these stations or are they just the location where the ship turned and began a new transect? For instance, in Figure 7, does the discrete in-situ sampling stations match the seven stations in Figure 1? Overall, I suggest revisiting the methodology section. One idea that may help could be including a table that lists all variables sampled, abbreviation and the source (ship underway, MVP, glider, discrete).
- Finally, I think the discussion can be improved since it seems slightly superficial. For a large body of results (pages 9-21, including figures), a ~1 page discussion is quite short, particularly when the results are good. I feel the discussion lacks a comparison to other works on storm events, both in the Mediterranean and other areas. The authors do briefly compare some results with the OSCAHR cruise, yet this cruise occurred in November and did not sample a storm event (as far as its mentioned in the manuscript). Therefore, why would the results be directly comparable? This is not to say that this comparison is not valuable, but a better contextualization should be included. Also, some conclusions within the discussion feel rushed and could do with better contextualization and arguments. For instance, in lines 317-318, the authors state that “This suggests that cells did not have time to photo-acclimate or that different species were involved” after comparing the ratio of chl-a between chl-a in “cold” and “warm” waters. First, this information is not enough to make these statements. Secondly, this is the only mention of photoacclimation in the manuscript, except for line 46 in the introduction. Finally, this conclusion is quickly forgotten since the paragraph moves on and compares the increase in chl-a with a previous work from 2000. Again, in lines 333-334, the authors now suggest the drop in carbon/chl-a ratio

is a “clear signature of a sudden change in phytoplankton cell physiology and translated the unadapted configuration of the cells to high light condition”. Why is it a clear signature? Why is one thing related to another? It is up to the authors to make the ‘bridge’ between the results and the conclusions, not the reader. Moreover, the paragraph ends with this sentence, without any comparison to other studies or without a discussion of its implications.

**Minor comments (lines on the left):**

3: Please remove or change ‘violent’ for a more adequate term (e.g., intense).

4: NW is written as ‘north-western’, yet the title includes ‘northwestern’. Uniformize.

8: missing of: factor of two

9: missing of: and of seven

24: missing have: have combined

26: what does ‘have evidenced pico-nanophytoplankton abundance and biomass responses’ mean? Did it increase, lower?

29: remove have: ‘have studied’

34: Are you suggesting that no previous cases of storms shaping primary production and phytoplankton community structure have been reported? It is not clear if this only refers to the NW Mediterranean, the entire Mediterranean or if it also includes other systems.

38: missing the: overpass the phytoplankton growth capacity

38: you already have north-western written in line 27, you can already use NW

41: This area

45: Add 'may': 'the mixing of the water column may bring microorganisms from deep to surface layers and affect their photophysiological properties (...)'

62-64: methods?

91: were performed

95 and 102: please specify that these are surface-only samples

Figure 2 caption: Orgnano and Unidentified particles groups have the same colour (green dots). Use light and dark green, for instance, to differentiate them in the caption.

149-150: It should have been calibrated prior to the cruise. Nevertheless, how good is the agreement with ship-based chl-a? Since the glider is the only source of data during the storm, this should be presented as supplementary material or, at least, the R, p-val, error and N should be indicated in the text.

156: swap SSH and sea surface height

157: swap SST and sea surface temperature

157: there is no such thing as sea surface chl-a. Satellite chl-a does not capture only surface chl-a.

159-160: please provide a bit more detail on the satellite products instead of just referring to another paper. You may leave the citation, but please add a brief description, just mentioning the name of the products or sensors and their resolution.

167: reference for the ECMWF model?

174: techniques instead of sources

179: in this context, this R2 could be higher.

189: again, remove sea surface.

191-192: the comparison period should actually be much shorter since the main ocean colour sensors overpass occurs between 10h-13:30h, depending on the sensor (see section 3.1 in Sathyendranath et al., 2019; Remote Sensing, 19(19), 4285). I would try rerunning the comparisons with a shorter period, it is possible the results may improve.

Figure 3:

- how does the R between MEDOCL3 and MEDOCL4 is equal to 1, but the R between MEDOCL3 and Chl\_insitu is 0.84 and MEDOCL4 and Chl\_insitu is 0.65?
- Where does the N=4555 come from when comparing satellite and in-situ data? Satellite data should be, at most, daily data unless the authors are working with geostationary sensors.
- The colour palette for the correlation plot should be changed to a more uniform one (e.g., R=0 white, R=1 dark red)

197-199: these are not results

201: why did you opt for MEDOCL4 when the relationship between satellite and in-situ was much better for MEDOCL3?

204: I recommend changing the Chl-a units from ng/mL to either ug/L or mg/m<sup>3</sup> since these options are more commonly used.

212: I recommend using m/s for wind speed. Also, the same units should always be used throughout the text (see line 222).

212: are these average or maximum intensities? Not clear.

215-218: Again, these are not results from this work, unless you include them as supplementary material. Thus, this comparison would be more suitable in the discussion.

224: The final sentence of the paragraph can be removed.

237: rose instead of rised up

299-300: this should also be in the discussion.

302: the water column was

311-312: add percentages or values when comparing