Ocean Sci. Discuss., 10, C1017–C1027, 2014
www.ocean-sci-discuss.net/10/C1017/2014/

Ocean Science
Discussions
Open Access

Interactive comment on "Observations of phytoplankton spring bloom onset triggered by a density front in NW Mediterranean" *by* A. Olita et al.

A. Olita et al.

antonio.olita@cnr.it

Received and published: 24 April 2014

Dear Editor, first of all I'd like to thank you and the two anonymous referees for the careful reading of the paper and for the precious suggestions. I'm happy that referees substantially enjoyed the manuscript and that found the data original and of "great value". I think the paper was substantially improved by following referees suggestions.

Hereafter a point-by-point reply to referee's comments follows. Author's replies are marked with "R: "

** REF #1 The authors analyse data from a glider to show that the spring bloom in the Mediter- ranean Sea coincides with the shoaling

mixed layer along a density front. They suggest that bloom initiation occurred because of re-stratification due to frontal instabilities after the shutdown of intense wind forcing and the shutdown of convective overturn. The authors start this paper with a short summary of the current thinking of the mecha- nisms leading to the formation of the spring bloom in global oceans. In particular, they discuss the current hypotheses that the spring blooms initiate in near-surface strat- ification that can form in frontal regions once wind mixing and convective overturn shutdown.

The then discuss the physical data (temperature and salinity) derived from a 40-day roundtrip glider flight in the Balearic Sea. Chlorophyll a sections derived from the glider's fluorometer show relatively high (but still <1 mgCm-3) on the outbound trip that coincides with two regions where the pycnocline is shallowest. On the return trip, chlorophyll concentrations were higher than 1 mgCm-3 and again highest chlorophyll was found in regions where the pycnocline was shallowest.

The authors use data from MODIS and the output from a numerical models to derive wind stresses and air-sear heat fluxes for the region.

The authors conclude that the phytoplankton bloom is triggered by shoaling of the mixed layer along a density front. The bloom initiated because of restratification in- duced by frontal instabilities after the shutdown of wind mixing and convective over- turn. They speculate that prior to the bloom formation, nutrient levels may have been increased by large vertical velocities along the front.

I find this work very interesting, and encourage publication of the results.

I do, however, think that the work would be significantly improved by a major revision.

The figures are poorly drafted and/or at low resolution. There is no easy way to com- pare one set of data with another for example the winds are plotted against days (1-40) whereas the other data are plotted with dates (e.g., 2013-2-17).

R: Figure have been redrawn and axes homogenized.

For the most part, the English is understandable, but there are many odd constructions (e.g., ": : :recently refused/revised the validity of the classical: : :"that make the text difficult to read. In some places, the text says the opposite of what is meant (e.g., "Taylor and Ferrari (2011a) : : : show that frontal restratification inhibits vertical mixing favouring the bloom.") this should be ": : :vertical mixing, and thus favours the bloom"). I suggest the paper would be made a lot easier to understand with considerable editing by a native English speaker. Overall, I find that the authors make the reader work too hard to try to understand their results.

R: Odd contructs have been revised. The entire paper was revised by a native english speaker, as suggested.

I have the following specific comments:

1) The paper needs a map with all the geographical place names on it. For those of us who are not familiar with the Mediterranean Sea we need to know where the Gulf of Lyon is, etc.

R: Figure changed accordingly. Toponyms have been added to the reference map.

2) The discussion of the physics (section 3.1) could be made much simpler if the authors included a TS diagram showing the difference between LIW and MAW.

R: Figure added accordingly

3) The figures are low-resolution (at least on the file I downloaded from the web) and it is difficult to read the dates and scales " particularly the density scale in Fig. 3. Also the dates across the top are at irregular intervals - why?

R: You right. Some figures are in low-res, even if nominal resolution is ok, because eps were transformed from raster format in order to save space. Now the problem was fixed. Dates are at irregular intervals because the glider does not proceed always at the same speed, so we can have that it traveled for longer distances for a given time span. If this is not a big problem I would prefer to preserve the time step (every 2-3 days)

for plotting dates, instead of maintaining a fixed distance between dates in the plot. The latter could cause two consecutive date-prints having the same date or that, on the contrary, having dates too far: this could make more difficult the data interpretation. However I changed a little bit the interval and fixed the problem of overimposed graphs. Also, the year stamp was removed from the date.

4) The authors need to rethink their use of a density difference (compared to the surface) of 0.2 as a definition of MLD. This particularly illustrated by the difference between the water column on 2013-2-13 and 2013-2-17 on the outbound leg. Both times show comparably deep MLD under the authors"definition, but the water column could not be more different. On the first date, dense water has been upwelled or advected into the transect, whereas on the second date, the deed MLD reflects the centre of an eddy. Similarly, the deep dip seen on 2103-2-21at the beginning of the outbound leg reflects the fact that the shallow near surface warming seen overlying the eddy was marginally less dense at that time.

R: We chanded a little the algorithm. We solved the problem by adding some line to the algorithm which now equals the MLD to NaN if the mean density of the MLD overpasses a given threshold (suggesting the presence of an upwelling like the one in date 2-17). In such cases we could assume the MLD close to zero (i.e. not deeper than the depth from which the algoritm starts the iteration: -15 m). In other words in such a case the MLD is not computed by the alghorithm. We added a brief sentence to detail this point in methods section. ("The simple density-based method for MLD identification also assumes....")

5) What satellite imagery do the authors mean (p 1566 line 7ff)? If they mean the MODIS image shown in Fig 4, there is no a priori reason that ocean colour would reflect the presence of eddies. Do they have SST for the glider track?

R: We refer to the top left panel of Fig 4, at the begin of the outbound track. Yes we have also SST but it does not provide any addictional information for the reader, so we would prefer to not insert it in the final paper. On the contrary, our experience is that, above all in given periods like end of winter when surface waters show in west mediterranean very weak horizontal temperature gradients due to the intense surface cooling, optimized and well "equalized" ocean color images provide better information on dynamics than SST can do. The present case also supports such an evidence.

6) Similarly, how do the authors interpret the presence of anticyclones A1 and A2 ? From independent data? From Fig 4? " if so, all the can do is note that there is some chl structure that could be consistent with eddies, and if so, then A1 is a high in chl, whereas A2 is low in Chl. It would be easier to interpret these data if they were all plotted with the clear longitude labelling.

R: Presence of anticyclone A1 is suggested "just" by modulation of isolines as detected by glider data while A2 is quite clearer. Further, the presence of two (and not only one) eddies is suggested by the upwelling area at about 2-14 that is a typical feature generated in the area by interaction of two anticyclones. Once data provided such important hints about mesoscale activity, we looked for eddies through satellite imagery and actually we found their signature in ocean color images. A clear longitude label was added.

7) There is no calculation of Ekman depth (line 10, p 1566). The winds would be easier to interpret if they were included in Fig 3 - so we can directly see what the windstress is along the glider track.

R: The sentence about ekman was deleted. Wind series from skiron have been moved to a subpanel of figure 3. Heat fluxes and wind stress have been left in the "old" position (last figure) as we think that this is functional to the discussion and to the rationale.

8) The discussion on page 1567 is particularly difficult to read. Again, the heat fluxes should be plotted in the same figure as temperature to make the discussion easier. If I understand correctly, the heat flux was out of the ocean during the entire glider track until 2013-3-3. This would explain the general cooling between the outbound

and inbound tracks, but not the formation of the shallow near surface warm layers on ∼2013-2-21. Also, the MLD, according to their calculations appears to shoal over the 2nd half of the return leg. In fact, I think the authors are correct in their interpretation, that the vertical mixing is inhibited by the halocline, but it took some work to get this!

R: The entire period was revised by a native english speaker to simplify constructs.

9) Perhaps the authors cold use the computed heat fluxes (fig 6) to calculate the watercolumn cooling and see if this is consistent with the observed temperature changes.

R: The fluxes are computed, through bulk formulae, by an oceanographic model that of course also computes SST. So we also have the model outputs in terms of SST that are consistent to what was observed by the glider. However I would avoid to heavy the paper with another graph, that in my opinion is un-necessary.

10) I don't understand the sentence "Despite the shoaling of the ML, we did not observe in this area a clear biological response in respect to the outward trip."To my eyes, there are large differences between the chlorophyll on the outbound and inbound legs. In fact the authors go on to describe these increases in Chl.

R: You right, sentence is a little bit confusing, as we refer to relative quantities. The response is effective in terms of distribution but is in any case weak in respect to what we observed in the westernmost area of return trip. Such an aspect is made clearer by observing the new plot of the integrated chl. Sentence has been reworded to better explain what we mean and to include your observation about the general response in terms of chl to the shoaling of the MLD.

11) The statement "Here, accordingly to Taylor and Ferrari (2011a), frontal instabilities promote the re-stratification of the water column when the wind forcing is sufficiently low to lower turbulent mixing."Is this speculation ?

R: Yes. It is part of the rationale that can explain, in our opinion, the observed phaenomena.

12) Chlorophyll looks to have a subsurface maximum in several places " e.g., 2013-1-31, 2013-3-8. Is this due to the quenching? There is no mention of fluorometer quenching in the article. Did the authors deal with quenching, or are the fluorometer data only from nighttime (I don't think so) ?

R: We disagree about such subsurface minima. I suspect, also by looking specific comment #15, that referee was misleaded from the Brunt Vaisala frequency plot which color palette resembles that one used for Chl-a, and that also misleaded the #2 referee. Further we forgot a,b,c lettering that now was added. Concerning quenching phenomenon, it was neglected as in winter period it is less relevant than in summer and, considering the basically "qualitative" approach of the study, we preferred to retain a larger amount of data than sacrify half of them. A sentence about quenching problem, with appropriate citation, was added in the methods.

13) I seems to me that the Chl data (fig 3) are the core data of this paper, and more should be done to analyse them. The chl data should be plotted with selected contours of density overlaid. Then one could rapidly see whether or not there is an easy relationship between chl and density.

R: We added in figure 3 the density contours to chl section.

14) It would be interesting to see what vertically-integrated chlorophyll looks like. If the critical depth is greater than about 150 m, then one might expect positive production over the entire glider track. And this looks to be the case " even in regions of deep MLD (e.g. 6-6.5) the total chlorophyll looks higher during the return leg.

R: Plot with integrated clorophyll was added. Actually it provides interesting information that are in good agreement with the mechanisms we suggested.

15) My alternative interpretation of chlorophyll, which I am putting up as a strawman that the authors need to test (noting that it is difficult to disentangle spatial and temporal content of the Lagrangian measurements) is " At the beginning of the cruise there

is a deep chlorophyll maximum (DCM). Which is typical of late summer. This is mixed up and down by the winds on days 1-15 of the track, hence in these regions chlorophyll is vertically well mixed. About 2-15, the winds drop, allowing surface stratification to appear (despite the negative heat flux " does this mean errors in the heat flux estimate??). About this time, the glider is crossing between eddies A1 and A2, and you can see chlorophll appearing at the edge of the eddy (2-15). Maybe nutrients are depleted in the eddy so there is no growth there (2-18) "or maybe there is still deep mixing but at the edge (2-19) there are nutrients and some stratification (also coastal effects)? When the glider returns there has been some capping of the eddy, with higher chlorophyll in these regions(2-20 and 2-23). As the glider returns, it passes through regions where there is still deep mixing where chl is well mixed to the halocline (e.g. 2-28, 3-4) but starting about 3-3 the heat fluxes and windstress turn off so that after this time, chl can stratify. Over most of the track, except, perhaps near (2-28), the vertically-integrated chl increases from outbound to return. Shallow sfc chl features such as seen on 3-1 are intermittent and associated with brief periods of low wind stress.

R: I think you have been misleaded by the BVF plot and by absence of lettering above subplot, so part of your intepretation on Chl is related (I think) to the distribution of BVF and not to Chl-a. For instance there are NOT evident DCM (it would be weird considering the winter-spring period). In any case I took in consideration your detailed interpretation on features and dates and I tried to integrate it in the discussion.

********* REF #2

Major comments: This paper studied the phytoplankton bloom in the NW Mediterranean Sea and the density front in the area that is probably related to the occurrence of the bloom. Glider and satellite observations were used, which can provide insights to the three-dimensional structure of ocean properties and evolution and development of the bloom. The glider data are unique and of great value. I would like to suggest major revisions before publishing the paper in OS.

Specific comments: 1. If the authors can polish the language, it can add value to the paper. E.g., in Page 4 Line 9, "show" should be changed to "showed".

R: Language was polished by a native English speaker.

2. I would like to suggest the authors adding the major purposes in the introduction section, which can be easier for readers to follow the logics of the authors.

R: We totally agree. A "main aim" sentence was added at the end of the Introduction section.

3. In Page 5 Line 23, it is confusing by saying "Pitch angle was about 20 degrees". What is the pitch angle for? The sensors? Or the glider? Please specify.

R: Pitch angle is the angle of glider dives in respect to the horizontal plane. It was specified in the text.

4. In Page 6 Line 11, which method is used for interpolation?

R: Linear interpolation was used. Specified in text.

5. In Page 6 Line 16, I would suggest authors rephrasing the sentence "The glider..." since what the WetLabs eco-triplet fluorometer measures is chlorophyll-a fluorescence and chlorophyll-a concentration is then calculated.

R: Sentence has been reworded.

6. In Page 6 Line 18, was the sensor calibrated by the manufacturer or by yourselves?

R: By manufacturer. Sentence changed.

7. How long did the glider maneuver for the whole mission?

R: Glider maneuvered for about 40 days. There is a short sentence about that at the beggining of the results.

8. Did the authors do quality control for the data, especially for chlorophyll-a? In Fig. 3,

I assume the right panels are for chlorophyll-a. Have the authors thought about what caused the patchiness?

R: Yes, a quality control was performed to identify outliers and spikes as usual. Bad data have been cutted off. Further a specific glider processing toolbox has been applied to filter out bad data, as specified in methods. However in the old Fig.3 right panels were buoyancy (Brunt vaisala) freq. and not Chl-a (bottom panels). I added letters to the panels in order to disambiguate, as I noticed that this generated a little bit of confusion among panels for both the referee. I also changed palette for BVF.

9. In Section 2.2, what did the authors post-process MODIS/Aqua data for? They mentioned SeaDAS. Which version of SeaDAS did they use? For comparing satellite and in situ glider measurements, how did they do the matchup? This must be solved. They also need to describe how many satellite images were used and the time span for those images.

R: Post-processing maybe is misleading. I changed the word to "re-projected and mapped" as acually I used seadas to reproject and equalize images. Seadas version (7.0) was also explitcited.

10. In Fig. 1, the authors need to specify the directions of the tracks. Only saying outward and return sections is not enough. What is the spatial resolution of the bathymetry? What is the source of the bathymetry data? From NOAA or from somewhere else?

R: In the caption the direction was specified. Spatial resolution of the represented batimetry is one minute and is from US Navy. Specified in the caption.

11. In Fig. 2, there are some texts overlapping in top panels. What are the units for salinity and temperature?

R: Overlapped text was solved by labeling every given longitude interval instead of Temperature is in C$^\circ$ degrees. Salinity has no unit, accordingly to last international dispositions and to editor.

12. In Fig. 3, similar questions mentioned for Fig. 2 should be answered. In the text, authors indicated that the glider data were interpolated. Then what caused the gaps in the two bottom panels. I have no idea about which panels is for what. Also for scientific papers, each panel in a figure should be assigned with a letter, such as (a), (b), ... This suggestion applies to all figures.

R: Interpolation (linear) was done for Salinity and temperature and derived density field. For chl, also given the lower frequency of acquisition of the data in the return lag, we preferred to show the "raw" profiles. Letters were added to subpanels.

13. In Fig. 4, what do "A1"and "A2" mean? How do you define the bloom? In one figure containing satellite images for different days, the satellite images should cover the same area? Otherwise, it would be misleading as shown in Fig. 4.

R: A1 and A2 refer to two eddies we observed in "results". We added the reference to eddies als in caption of fig 4. The area of interest for the glider is actually well represented in the upper panels but we opted to show a larger view in the bottom image for two reasons: observing what happens in other areas helps the discussion (in the deep convection area of the provencal basin, as well as along large eddies margins off the African coast); in the former two images cloud cover do not allowed to show larger areas, while in the third image there were almost no clouds. In any case the palette is the same for the three images, which makes the three img comparables.

14. In Fig. 5, what is x-axis for? Days of what? Very confusing.

R: Axes changed, labeled, dates changed and figure moved to a sub-panel of Fig.3.

Interactive comment on Ocean Sci. Discuss., 10, 1559, 2013.