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

Anonymous Referee #1

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The authors analyse data from a glider to show that the spring bloom in the Mediterranean 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 mechanisms 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 stratification that can form in frontal regions once wind mixing and convective overturn shutdown.

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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 $<1 \text{ mgCm}^{-3}$) 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-sea 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 induced by frontal instabilities after the shutdown of wind mixing and convective overturn. 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 compare 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).

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.

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Overall, I find that the authors make the reader work too hard to try to understand their results.

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.

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.

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?

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 deep MLD reflects the centre of an eddy. Similarly, the deep dip seen on 2103-2-21 at 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.

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?

6) Similarly, how do the authors interpret the presence of anticyclones A1 and A2 ? From independent data? From Fig 4? – if so, all they 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,

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whereas A2 is low in Chl. It would be easier to interpret these data if they were all plotted with the clear longitude labelling.

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.

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!

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

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.

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 ?

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) ?

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13) It 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.

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.

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 chlorophyll 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.

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