Biogeosciences Discuss., 12, C2777–C2782, 2015
www.biogeosciences-discuss.net/12/C2777/2015/

Biogeosciences
Discussions

Interactive comment on "On the vertical distribution of the chlorophyll a concentration in the Mediterranean Sea: a basin scale and seasonal approach" *by* H. Lavigne et al.

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Received and published: 14 June 2015

We have modified the manuscript according to your suggestions and to those of the three other reviewers. We think that the new manuscript has been accordingly improved.

Although we answer to each referees separately, in the following points we resume the main modifications of the manuscripts (considering all the reviewers comments):

> A better qualification of the limits of the non photochemical quenching correction method in case of stratified water column.

> The consideration of climatological density profiles in the description of [Chl-a] vertical profiles (cf. Fig. 3).

> The quantitative analysis of some characteristics of the standard shape of profiles. A new paragraph (Sect. 3.2.1) and a new table (Table 3) have been introduced. These results are also discussed in the section 4.1.2

> A new table (Table 4), which aims to highlight differences between Mediterranean regions, has been added. The new table allows to better discuss the observed differences between seasonal cycles of [Chl-a] vertical profile in the Mediterranean Sea (Sect. 4.2.1) and the regional differences in DCM depth (Sect. 4.2.2).

> A new figure presenting [Chl-a] vertical profiles as a function of light has also been added. It allows supporting our hypothesis on the impact of light on seasonal variability of the DCM depth.

In the following, we answer to the specific comments of the referee #1:

General Comments

- A) First, the authors need to discuss the Chl profiles in relation to the hydrography. The conventional explanation for a DCM is that there is nutrient depletion in the mixed layer, and that summer time production is supported by a flux of nutrients across the pycnocline. At the end of the summer, vertical mixing destroys the DCM, and the water column enters a well-mixed regime. Under such an explanation, Chl should show a DCM coincident with the pycnocline until autumn, when it is eroded by vertical mixing – and maybe lack of light. The authors' figure 3 shows Chl(z) climatology for 4 different regions. All four regions show a summer time DCM and more mixed profiles in winter, but there are substantial differences in the seasonal evolution at the four locations, that are probably explained by the water column density cycles. For example, the northwest region shows a near-classic spring- and autumn-bloom scenario. Starting in winter with deep mixed layers, there is then a near-surface spring bloom, followed by summer DCM, which in turn is followed by an autumn bloom and then the profiles revert back to winter conditions. The DCM emerges in May with the deepest DCM in August of about 50 m, and then a shoaling DCM that is disappears by November. In contrast, the region to the south shows a DCM all year around, even in winter, with deepest DCM >100 m in September. Presumably these differences are forced by different physics at each location, and they need to address questions such as why do these locations have such different climatology – do the differences in the annual Chl cycles at each location reflect differences in vertical mixing at each location, leading to differences in mixed layer depth (MLD), etc. They need to ask how does the Chl structure reflect the background density. For example – is the DCM always found at the pycnocline? re the HSC profiles found during deep mixed layers – or do they reflect stratification in the water colomn. The discussion of Fig. 6 also needs to discuss the water column hydrography – I presume the longitude variation in DCM depth reflects longitude variation in pycnocline depth? Similarly, the discussion of Fig. 7 needs to be done in context of the different hydrography at each location.

Authors response:

We agree with referee that a discussion on the regional differences on the DCM and CHL profiles and on the role played by the hydrological patterns in their shaping was missing in the old version of the manuscript. Climatological vertical profiles of density have been then added to the figure 3. A new table 4 was also introduced in the new version. The new table includes both hydrological and biogeochemical parameters at regional level. The text has been accordingly modified to discuss the changed figure and the new table: In the results section: p12, lines 6 to 28 In the discussion section: page 19, lines 7 to 15, pages 19-20, lines 33 to 12 and pages 21-22, line 20 to 11.

- B) The second thing the authors need to do is a comparison of the vertically-integrated Chl (hereafter Ctot) with Surface Chl (hereafter C0). Interestingly, the authors set the reader up for such a comparison (p 4143, line 20) but fail to do so . This is of extreme interest, because C is measured with ocean color satellites (Seawifs Modis) but it is

the water column integrated biomass that determines the oceans productivity. Many authors estimate Ctot from C0 using Ctot = MLD times C0 and this relationship goes into estimates of total biomass, net primary production algorithms, etc. This article shows that Chl is rarely homogenous in the mixed layer, and (From Fig. 2), it becomes fairly obvious that the relationship Ctot and C0 is different for each type of profile (DCM, HSC, etc). Thus there will not be a universal easy relationship between Ctot and C0. The plots shown in Fig. 7 (DCMdepth vs C0, DCMdepth vs C(DCM), C(DCM) vs DCM width) describe the structure of the DCM, but are of relatively little interest to the real issues relating to water column production. The authors should perform similar regressions, but comparing Ctot vs C0. The authors should explore when Ctot is correlated with C0, and when it is not - for example they could regress Ctot vs C0 by region and month.

Authors response:

We fully agree with referee #1 that the relationship between Ctot and C0 is of extreme interest. However, we think that our calibration method does not allow us to properly investigate this point, especially at the seasonal scale, as the vertically-integrated chlorophyll-a over 1.5Ze is derived from the satellite surface [Chl-a] observation. Indeed the log-log relationships between the surface [Chl-a] value and the vertically integrated [Chl-a] proposed by Uitz et al., (2006) have been used for calibration (see Lavigne et al., 2012 for further details). In these conditions, the analysis of Ctot versus C0, as suggested by referee #1 should have little interest. Nevertheless, we get round this limitation and we analysed, from non-calibrated profiles, the ratio between the vertically-integrated [Chl-a] over the 20 upper meters to the total water column (Fsurf/Ft). This ratio with other parameters (MLD, surface [Chl-a], Ze) was calculated for each type of standard shape and is presented on Table 3 and section 3.2.1 (pages 14-15, lines 30 to 5).

Specific comments

- Overall, the figures are good, and for the most part the English is good, although it could use a little editing from a native English speaker.

For example, the 6-line sentence on page 4143, lines 4-9 is a struggle to read.

Authors response:

Sentence line 6 has been modified accordingly: "Indeed, focusing on ocean color observations, D'Ortenzio and Ribera d'Alcalà (2009) confirmed the presence, in the Mediterranean Sea, of surface [Chl-a] annual cycles, displaying similarities with subtropical or with temperate regions. The authors demonstrated that a subtropical-like [Chl-a] seasonality (highest [Chl-a] during winter and lowest during summer) encompasses most of the basin whereas a temperate like seasonality, marked by a high peak of surface [Chl-a] in spring (in March/April), is recurrently observed in the North-Western basin and occasionally in other Mediterranean regions."

- Pg 4143 Line 15 - the authors miss some of the most important controls of primary production – mixing due to winds and/or vertical overturn.

Authors response:

The section 1.2 has been restructured and the sentence corresponding to page 4143 line 2 has been modified accordingly:

"As discussed in a recent review by Cullen (2015), there is no unique DCM and its dynamics result from the interactions among external forcing, e.g., the penetration of light in water, the intensity of vertical mixing and subsurface nutrient distribution and biotic processes, e.g., photoacclimation, grazing, phytoplankton composition."

- Fig. 3 and elsewhere – the authors need to compute the number of independent profiles – two profiles taken on the same day, for example, are not independent, and only show climatological profiles computed from a significant number of profiles. For example, in Fig. 3 the April climatology derived from one profile for location B is meaningless – and it is misleading to plot it (even though the authors do label it as a mean

of one profile).

Authors response:

We agree with the referee that when the number of profiles is low or when profiles are close (in time and/or space) they are not independent, and then that the average profile (as shown for example in figure 3) is not strictly "climatological". We are convinced, however, that, given the low number of profiles during some seasons and for some regions, showing the mean profile could be interesting for Mediterranean scientists. However, to highlight the problem for the reader, and to prevent any misinterpretation of the figures, we added some text.

Page 11, line 23-26: "Although, in the following, we refer to these time-series as "climatological", certain average profiles result from a low number of fluorescence profiles (sometimes less than 10, see numbers on Fig. 3) and therefore do not strictly represent a climatological pattern."

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2777/2015/bgd-12-C2777-2015-supplement.pdf

Interactive comment on Biogeosciences Discuss., 12, 4139, 2015.