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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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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 manuscript (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 #4:

General Comments

- Although the paper is interesting and provides a nice description of the chlorophyll profiles in the Mediterranean sea, someone who has studied general oceanographic textbooks and looked at the MEDATLAS will not be surprised by the results and may not even find much new, except for a finer description of some aspects. I thus feel there is a bit of a lost opportunity in this paper to explain the profile types as a function of such things as temperature gradient (perhaps linking to sea surface temperature and time of year) or other physical characteristics of the water column. Could the authors have used their dataset to provide predictive relationships for the shapes? Why haven't the authors used the accompanying physical datasets?

Authors response:

Mediterranean Sea is a region of great interest for scientists, being the subject of numerous publications. However, the understanding of the spatio-temporal variability of the [Chl-a], one of the most common biogeochemical variable, is very limited and that is why we thought it was important to improve it and to refine some aspects of its variability.

Nevertheless, as also suggested by referees #1 and #3, in the new version of the manuscript, some elements describing the hydrological and biogeochemical context are introduced (i.e. Table 4 and density profiles in Fig. 3). These elements allow to support the discussion on the regional differences although they do not allow us to definitively explain them.

Specific comments

- Section 4.1.1. : This section appears a bit weak to me, the authors seems to suggests that the difference between their dataset and the MEDATLAS dataset are only cause by limitations of the MEDATLAS dataset (bad averaging and sparse vertical resolution). While it may be true, that their dataset is the new standard, it is certainly not shown in this analysis. A particularly interesting difference is found in the Levantine Basin where the MEDATLAS data always shows increasing chlorophyll concentration to the surface while this is not seen in the chlorophyll profiles, it seems like bad averaging would be an unlikely explanation for this systematic difference; there is here a good opportunity to show which dataset represents the trends best. Perhaps the authors need to go back to measured profiles of HPLC (or extracted Chl) to examine which of the two dataset is right.

Authors response:

As referee suggested, we examined MEDATLAS data to understand why there are [Chl-a] increases in surface in the Levantine Sea in the MEDATLAS climatology. First, we observe that for most of seasons there is no [Chl-a] observation close to the studied point (i.e. the Levantine Sea).

http://modb.oce.ulg.ac.be/backup/medar/JPGSTATIONS/medar.winter.med.cphl.20.3.0.statio

http://modb.oce.ulg.ac.be/backup/medar/JPGSTATIONS/medar.summer.med.cphl.20.3.0.sta

http://modb.oce.ulg.ac.be/backup/medar/JPGSTATIONS/medar.fall.med.cphl.20.3.0.stations.

The large scale interpolation process (Variational Inverse Model see http://modb.oce.ulg.ac.be/backup/medar/contribution.html for details) produced then a gradient in the Levantine Sea, between the very low value of the Cretan Sea (e.g. ~0.07 mg/m3 for summer) and higher values (e.g. ~0.15 mg/m3, for summer) measured along the Lebanon coast.

http://modb.oce.ulg.ac.be/backup/medar/JPG/medar.summer.med.cphl.23.3.0.jpg

However, the incertitude about this estimation is very high in the Levantine basin (~0.1 mg/m3 for summer http://modb.oce.ulg.ac.be/backup/medar/JPG/medar.summer.med.cphl.23.3.0.error.jpg)

We think that this process may be also responsible of the [Chl-a] surface increase observed in summer in the North-West and South-West region. Indeed, the MEDATLAS database contains coastal observations in the North-Western and South-Western basins but not in the Ionian Sea.

As you suggested we better deal with this issue in the text. In the new version of the manuscript the following paragraph has been introduced:

“Another particular feature of the MEDATLAS climatology that does not show in the fluorescence-based climatology are the rises in summer and autumn surface [Chl-a] above DCM (Fig. 8, panels A, B and D). We suggest that this feature could result from the propagation by interpolation of the high surface [Chl-a] observed on coastal regions (see also Bosc et al., 2004). In addition, considering the geographical positions of the available MEDAR observations, in almost all the studied sub-basin (except Ionian) coastal observations are included in the database. They might therefore be responsible for the observed difference with the fluorescence-based climatology.” Page 17 lines 4-11.

Please, note that Fig. 8 panel D (for Levantine) is slightly changed due to the displacement westward of the studied point (now 34°N, 30°E, see Fig. 1). We wanted to avoid the influence of the Cyprus gyre.

- Figure 6 (and accompagnying text): A variation with longitude is not particularly explanatory. You will find this if you go longitudinally across any oceanic gyres. Clearly the factors driving these relationships are more important. I'm surprised that no attempts are made to calculate the light level at the DCM. It could be as simple as using the latest Morel KPAR relationship; I'm sure the authors know where to find it! Thethermocline depth could also be plotted in some way.

Authors response:

As also requested by referee #1, the deepening of DCM with longitude is further discussed in the new version of the manuscript. The discussion is based on Table 4 which gives for each region of the Mediterranean Sea: mean winter MLD, mean DCM depth, average daily PAR at DCM and which provides an estimation of the nitracline depth. The following paragraph has been introduced to discuss Table 4 and the longitudinal gradient in DCM.

"At the first order, the DCM depth variability in the Mediterranean Sea is related to the spatial component and, in particular, longitude. The deepening of the DCM along a longitudinal gradient (in the present study, DCM deepens by 1.6m per 1 degree of longitude east) agrees with the previous review, also based on observations, by Crise et al. (1999). This general deepening of the DCM with longitude covaries with the eastward increase of oligotrophy in the Mediterranean Sea (Béthoux et al., 1998). This pattern is generally attributed to anti-estuarine circulations in the Straits of Gibraltar and Sicily, which generate an eastward inflow of surface nutrient depleted waters and a westward outflow of deep nutrient rich waters. In the Eastern Mediterranean Sea, oligotrophy is also maintained by poor nutrient inputs from the boundaries (atmosphere and coasts) and by the formation of Levantine Intermediate Water, which is not the

product of deep convection but of the subduction of surface water into intermediate water layers (Robinson and Golnaraghi). As revealed by Table 4, regional changes in DCM depth, nitracline depth and averaged daily PAR at DCM are correlated in the Mediterranean Sea. The eastward deepening of the DCM depth and of the nitracline depth is accompanied by a decrease in the mean daily averaged PAR at DCM (values ranging from 1 mol quanta m-2 day-1 in the North-West Mediterranean to 0.16 mol quanta m-2 day-1 in the Levantine Sea). This trend concurs with the "general rule" that states that the DCM builds-up where there is an optimal balance between the upward nutrient flux and the downward photon flux and lies on top of the nutricline (Cullen, 2015). The large distance between DCM depth and nitracline depth in the Ionian (36m) and the Levantine (83m) basins may be considered as contradictory with the previous theory. However, according to Table 4, the estimations of nitracline depths are not likely to be good estimators of the top of the nitracline, if the nitrate gradient is not a enough sharp feature, as is it the case, for example in the Eastern Mediterranean Sea. Indeed, nitracline depths have been computed from discrete vertical profiles, using the 1μM isoline (Lavigne et al., 2013)." Page 21-22, lines 20-11

In addition, a figure 10 was introduced to support the "light driven" hypothesis for the seasonal variation of the DCM depth. The following text was also added:

"Results from Fig. 10 also show that a seasonal component contributes to explain DCM variability in the Mediterranean regions. The observed seasonal pattern of the DCM depth (i.e. deepening from spring to summer and shallowing from summer to autumn) is consistent with previous model results (Macias et al., 2014), and with individual Bio-Argo float observations (Mignot et al., 2014). Letelier et al. (2004) and Mignot et al. (2014) explain this seasonal pattern by considering that the DCM depth might be driven by the light availability and that it would follow the depth of an isolume. This observation is confirmed here by the analysis of the vertical [Chl-a] profile as a function of irradiance for the spring, summer and autumn periods (Fig. 10). For all regions, from spring to summer, PAR at DCM depth remains unchanged although [Chl-a] decreases.

Accordingly to Letelier et al. (2004), higher spring [Chl-a] may be explained by the temporal erosion of the upper nitracline from spring to summer, supporting the hypothesis of deep biomass maxima. From summer to autumn, the magnitude of DCMs remains roughly unchanged, similarly to the PAR at DCM." page 22, lines 12-24.

- Figure 7: Why so much white space. The Y-axis extends to more than 200 m while there is no data below 125 m.

Authors response:

We thank referee for this comment. The Figure 7 has been changed accordingly.

- Figure 8: I'm not sure why a comparison with the Uitz et al. 2006 profiles is not made. I understand that those are used to set the amplitude of the profiles, but surely they would be informative as a comparison of the shapes.

Authors response:

The application of Uitz et al., (2006) method on a case by case basis or even regional one is not recommended (Uitz et al., 2006). In addition, the Uitz et al. (2006) method has been developed to compute primary production from ocean color observations and not to provide patterns of the [Chl-a] vertical distribution. For these reasons, we think it is not relevant to compare our profiles with profiles derived from the Uitz et al., (2006) algorithm.

- Figure 9: This figure has multiple problems. First, I do not understand why the paper ends by presenting this figure. It is not, to me, particularly insightful or providing an interesting opening for things to come. Second, the caption is very hard to follow, especially the first section explaining the different panels. Finally, the fits just do not seem to match the data in panels B and C. In B, residuals are clearly positive at low [Chl-a]DCM and negative at high [Chl-a]DCM. Something similar appears to happen in panel C probably driven by a few low values at low dz. Perhaps looking at a running average may confirm whether or not my eye is right. Of course any discussion (i.e.

text) linked to the apparently bad fits may not provide much insights.

Authors response:

We agree that figure 9 is maybe not relevant and too much complex for the end of the discussion. So, we decided to only maintain panel A which displays the most relevant relationship (relationship between surface chlorophyll and DCM depth).

The figure 9 was then modified and the associated text was replaced by:

"The present study also shows that in the Mediterranean Sea, the specific features of the [Chl-a] profiles with a "DCM" shape have a large variability, comparable to those observed in the Global ocean, although occurring on shorter spatial scales. The most relevant indicator is certainly the DCM depth, which was observed to range between 30m and more than 150m. As expected (e.g. Cullen, 2015), the depth of the Mediterranean DCM is inversely related to the surface [Ch-a] (Fig. 9). In addition, the relationship between the DCM depth and surface [Chl-a] (blue curve on Fig. 9) is similar to the relationship reported for the Global ocean (red curve on Fig. 9, Mignot et al., 2011). This observation suggests that certain DCM properties in the Mediterranean Sea conform to the same generic properties established for the Global Ocean." page 21, lines 11 to 19

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2800/2015/bgd-12-C2800-2015-supplement.pdf

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