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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.

H. Lavigne et al.

hlavigne@ogs.trieste.it

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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.

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> 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 #2:

General Comments

- I would have a native English speaker proofread the manuscript, as I found several typos (for some of them I added corrections).

Authors response:

We agree, the manuscript was proofread by an English native speaker.

- The manuscript would be stronger if you could provide (in section 1.2) a better justification as to why it is important to understand the dynamics of the vertical distribution of chla.

Authors response:

We agree and restructure the whole section 1.2. In particular, the last paragraph of this section should provide a better justification of why it is important to understand the

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dynamics of the vertical distribution of Chla.

“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. To assess which and how many DCMs exist in the Mediterranean sea because of its known geographical and dynamical gradients, a starting step is to produce a quantitative characterization of their shapes and their seasonal evolution, which is one of the main scope of this contribution. In addition, a good appreciation of seasonal changes in vertical [Chl-a] distribution, the other objective of this study, is a first step towards a better understanding of mechanisms controlling seasonal phytoplankton development. It is also essential to better interpret changes in surface [Chl-a] as detected by satellite sensors. This study will allow for the integration of the biogeographical characterization of the basin built on surface [Chl-a] patterns, thus paving the way to focused area studies based on in situ sampling or autonomous vehicles.”page 5, lines 6-18

- The analysis is based on fluorescence data corrected for non-photochemical quenching (NPQ) using a previously published method, which is based on extrapolating the maximum fluorescence value in the mixed layer to the surface. I would expect this method to be insufficient to correct for NPQ in most of the Mediterranean Sea, where relatively shallow mixed layers and clear waters would allow NPQ to affect fluorescence profiles much deeper than the mixed layer. I think it would be important to address and discuss this issue.

Authors response:

As also suggested by referee 3 and 4, we further discussed the impact of the limits of the NPQ correction method in case of stratified water column.

To assess this impact, two analyses were carried out.

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1- From calibrated [Chl-a] profiles (1998-2014 database) we compared the surface satellite [Chl-a] estimations with the surface [Chl-a] concentrations derived from calibrated fluorescence profiles. Our results showed that surface [Chl-a] can be underestimated for profiles with MLD lower than 50m. In the worst cases (MLD around 10m), the underestimation is of a factor 2.5.

2- From the climatological [Chl-a] profiles displayed on Fig. 3, we calculated profiles of instantaneous PAR, using the monthly MODIS climatology for the instantaneous PAR at profiles geographical position. The equation of Sackmann et al., (2008) has been then applied to estimate the relative error, which could be introduced by NPQ. Results showed that for depths deeper than 60m, the error on [Chla] is always lower than 10%. In the worst cases (surface in summer), this error is up to 60% (equivalent to an underestimation of a factor 2.5).

We are convinced that the above results provide an estimation of the limits of NPQ correction method that we proposed in the manuscript. This estimation proves also that the NPQ correction has only a minor impact on our results and their interpretation. For most of the "DCM" profiles, the surface [Chl-a] are enough low that doubling or tripling their values does not induce any substantial variation of the vertical shape. Our main concern is for the estimation of Fsurf/FT ratio (surface Chl-a content to total Chl-a content, see Table 3 in the new version of the manuscript) for the profiles of the "DCM" category. We estimated a value of 6% and we are now convinced that this value is underestimated (a more realistic value is probably 12-15%), although the interpretation that we gave is not substantially changed. We thank the referee for this highlight and we added, in the new version of the paper, the following paragraph to explain the new analysis and to advise the reader:

"By applying the equation proposed by Sackmann et al. (2008) on monthly averaged light fields, the impact of NPQ was observed to be significant only above 60m, thus leading a two-fold underestimation of surface [Chl-a]. Considering this result, the weak efficiency of the NPQ correction method in stratified conditions should not have major

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consequences on the present study. Only the analysis of the surface to integrated content chlorophyll ratio (see Table 3) should be considered with caution." Page 9 lines 5-11.

- I would restructure the Conclusion section so that it summarizes the most important findings. As it stands now, it seems like a continuation of the Discussion

Authors response:

We thank the reviewer for this suggestion and modified the conclusion in order to better sum-up main [Chl-a] patterns in the Mediterranean Sea. In particular, the following paragraph was introduced:

"The present analysis, in agreement with previous satellite results (D'Ortenzio and Ribera d'Alcalà, 2009), demonstrates the coexistence of two main types of dynamics (i.e. subtropical and mid-latitude dynamics) in the Mediterranean Sea. Mid-latitude dynamics are observed in the North-Western basin. Their main specificity is the high occurrence of "HSC" profiles in March and April, whereas this type of shape, associated to bloom conditions, is nearly absent elsewhere during this season. The subtropical dynamics encompass most of the remaining basin. It is characterized by an omnipresent DCM from spring to autumn and by a large variety of [Chl-a] vertical shapes during winter. The present analysis also demonstrated that the [Chl-a] pattern in the Mediterranean Sea is not uniform. Even among regions with subtropical dynamics, a strong variability was observed in [Chl-a] values or DCM characteristics. At the basin scale, this variability follows an eastward oligotrophic pattern." Page23, lines 3-14.

- I have also added some minor comments to the original text: see attached pdf file.

Authors responses

We thanks the referee #2 for all of his comments and corrections. We have changed text accordingly.

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Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2783/2015/bgd-12-C2783-2015-supplement.pdf

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