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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.

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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 #3:

General Comments

- Editing of English would be useful for clearer understanding. I strongly suggest this.

Authors response:

We agree, the new version of the manuscript was proofread by a native English speaker.

- It might be useful to be more explicit about what this study seeks to achieve and what it contributes to future work. You have painstakingly calibrated a database, built vertical climatologies and shown really interesting variations of vertical [Chl-a]. As I understand it, one key outcome is that regional and seasonal variations in DCM depth are potentially light-driven. This is discussed within the context of previous work, but not strictly assessed beyond what is shown in Figure 9(a). It may help to clarify in the introduction that turbulence, nutrients and grazing may contribute to vertical dynamics,

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but will not be assessed in this paper. Further, to state that the work done here is important for future studies addressing the more specific physical, chemical and biological questions.

Authors response:

We agree with the referee comment statement. Section 1.2 was restructured in the new version. In particular, the following text was added page 7 lines 5 to 9.

"The scope of this paper is essentially restrained to the description of the variability of [Chl-a] vertical profiles, as they result from the interactions between many factors that can be complex as well as poorly documented. This variability will be only discussed with regard to Mediterranean hydrology and light fields."

As suggested, the following sentence was also added to conclusion:

"Although it is a first and necessary step for a better understanding of processes which impact seasonal variability of [Chl-a] vertical profiles, it would be interesting to further study certain particular cases showing, with a high frequency, annual series of vertical [Chl-a] profiles." (page 23, line 20-23)

Specific comments

Introduction:

- Figure (1) might benefit from the addition of seasonal subplots showing (a) winter and (b) summer surface Chl-a. Otherwise, maybe summarise section 1.1 into one explanatory paragraph (to give section 4.2.1 context) and focus more on the vertical story.

Authors response:

We agree with this comment and add two subplot in the Fig. 1 to display average surface [Chl-a] in summer and winter.

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- PG 4142, line 14: As you are including all seasons and all regions, perhaps 'oligotrophic' is a more robust description of the entire basin.

Authors response:

We agree "ultra-oligotrophic" has been changed to "oligotrophic".

- PG 4144, line 3: I'm a little uncertain about this. Firstly, Mignot et al. (2014) attribute DCM patterns in the Mediterranean to a combination of photoadaption and biomass, and Macias et al. (2014) base their DCM on model data (which you later show does not agree with in situ data in the eastern basin). While some (or even most) of these DCM may be due to biomass maxima at depth, their contributions to vertically integrated primary production may also not necessarily be limited to this distinction. DCM generated by photoacclimation (Chl-a packaging) are not to be discounted. For this reason, I'd be cautious about how explicitly you link high primary production to biomass-DCM. However, biomass-DCM are important for structuring food webs, so this is interesting from that perspective and leads me to my next comment.

Authors response:

As mentioned above, the section 1.2 was fully modified. In the new version, DCM dynamic is discussed within the following text:

"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." Page 5, lines 6-12.

- PG 4145, 23: The variability of fluorescence to [Chl-a] is indeed compounded by

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environmental conditions, as well as taxonomy. It is good that you have mentioned this; however, you should say more especially IF you want to maintain the previous assumption about these DCM being deep biomass features. Although the fluorescence to [Chl-a] ratio can be affected by packaging (as you say), the Chl:C (cell) relationship is profoundly altered. This may simply have been lost in translation, but the links between fluorescence, [Chl] and biomass need to be carefully described.

Authors response:

We agree with the referee that the complex relationships between fluorescence, [Chl] and biomass are critical and still not fully understood. Any passage from fluorescence to chlorophyll and from chlorophyll to biomass is submitted to hypothesis that could be different following regions, time and environmental conditions (including species composition). In the present manuscript we deliberately decided to address the [Chl-a] variability in a specific oceanic region, the Mediterranean Sea. Consequently, we discussed more the assumptions relating fluorescence to chlorophyll (because fluorescence is our primary source of data) than those relating chlorophyll to biomass. Moreover, at basin scale and in a climatological approach, we lack most of the data allowing to estimate the autotrophic organic carbon content. We then decided to restrain the scope of our analysis to the [Chl-a] and not to deal with the phytoplankton carbon variability.

Data processing and Calibration:

- PG 4148, line 27: With regards to your quenching correction procedure, I have a few suggestions:

(a) You will need to look at surface values (5-10m) of fluorescence measured during the day (potentially quenched), compared with surface fluorescence measured at night (not quenched). I would suggest doing this for each region and each season separately. If there's no measurable difference between day and night surface fluorescence, you may very well have support for your DCM being mostly deep biomass features. However, if

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you do see suppression of fluorescence yield in surface waters during daylight hours, then you cannot do as Mignot et al. (2014) did and effectively ignore quenching. The bad news is that if your MLDs are shallow and the water column is stratified, you may not be able to correct quenching.

Authors response:

We acknowledge you for your suggestion. However, given the large diversity of sources for [Chl-a] vertical profiles, we were sometime poorly confident in the time data (when available) of fluorescence profiles. Thus, the analysis you suggested was impossible to carry out. However, to better assess the impact of NPQ in case of stratified water column. We performed the following analyses:

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.

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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 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.

- (b) You do acknowledge the limitations of the correction method of Xing et al. (2012) but you do not mention the proportion of your data that is stratified. If it's a small proportion, it might be better to discard your quenched plus stratified profiles.

Authors response:

We agree with the referee, although stratified profiles represent more than 50% of our dataset and then we cannot discard them. However, as explained in the previous point, in the new version of the manuscript, we provide an estimation of the error induced by the NPQ method.

- (c) In winter and spring, the deep MLD and potentially high turbulence appears sufficient to generate more homogenous mixing. Having said that, it might not be accurate to assume homogeneity. When the MLD is deep, consider correcting from Zeu rather than the depth of the mixing layer (Biermann et al., 2015). This may help conserve

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heterogeneous features between the 1% light level and MLD. I strongly suggest this step because of the presence of winter subsurface maxima in the MEDATLAS and sometimes fluorescence-based climatology (Fig.8 and Fig. 3C). These features may not be artefacts as you suggest, and there is a risk of masking them when correcting from MLD.

Authors response:

We thank the referee for this suggestion, which should have generated a bias in our results. In the generation of a climatology, however, we are convinced that a unique data processing method is preferable as the averaging of profiles treated in different ways could introduce artifactual bias. We maintained then the Xing et al. (2012) method for the processing of all Chl profiles. However, to check the referee point we carried out an additional analysis of our data set. For profiles with an available estimation of Zeu (satellite matchup) and satisfying the condition : Zeu < MLD, we observed that the extrapolation depth (for the NPQ correction) is shallower than Zeu for 82% of profiles. A visual control performed on the remaining 18% of profiles, does not showed subsurface maxima between Zeu and MLD. The use of Zeu and the method of Biermann et al. 2015, although certainly more corrected from theoretical point of view, doesn't improve substantially the analysis of our data.

To advise the reader about the referee concern, we added some text in the new version of the manuscript.

"Although Biermann et al. (2014) proposed an improvement of the method for profiles with euphotic depth above MLD, we preferred to use a unique data processing procedure, to avoid the introduction of an artificial bias due to a heterogenic data treatment." Pages 8-9, lines 30-1

- PG 4149, line 10: Please explain why you remove profiles where MLD is "deepest (deeper?) than the deepest fluorescence observation"? I see these make up a tiny part of your dataset, but why is this step required? MLD should have no impact on

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removing instrumental offset, but it appears this is part of that process?

Authors response:

As explained in the text, the step 2 consists in removing instrumental offset in order that fluorescence value is 0 when there is no chlorophyll (i.e. at depth). To determine this offset, we should focus on regions of the profile where we can expect an absence of chlorophyll. We can expect an absence of chlorophyll for deep depths where there is no light and where mixing does not entrain surface phytoplankton cells. If MLD is deeper than the deepest observation depth (as for example during deep convection events as observed in the North Western Mediterranean area), it is impossible to estimate the offset coefficient (beta). In these cases, profiles are not removed but the coefficient beta is set to 0.

Text was slightly modified:

"Profiles in which MLD was deeper than the deepest fluorescence observation were not processed but not remove of the database (1.1% of data set)." Page 9, lines 16-18

In the text "deepest" has been changed to "deeper", thank you for having identified this mistake.

- PG 4149, line 27 Please explain why you impose that integrated fluorescence content (surface to 1.5Zeu) should match surface Chl-a measured by satellite? I may simply have missed something obvious, but this makes no sense to me. For one, you're measuring over all seasons and the depth of the 1% light level will change enormously between summer and winter. Furthermore, shouldn't surface values be imposed on surface values? Would it not be more useful to impose this match-up from one optical depth, thus integrate from 1/kd(490)?

Authors response:

We agree with the referee that this paragraph was not totally clear. In the sentence "A multiplicative coefficient (α coefficient in Eq. (1)) is applied to the fluorescence

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profile, imposing that the integrated fluorescence content matches the value derived from satellite", the term "value" refers to the integrated chlorophyll content over 1.5Ze estimated from satellite [Chl-a] using empirical relationships (Uitz et al., 2006) which is mentioned just above and not to the surface [Chl-a] value.

To avoid any confusion, the sentence was modified accordingly: "A multiplicative coefficient (α coefficient in Eq. (1)) is applied to the fluorescence profile, imposing that the integrated fluorescence content matches the integrated chlorophyll content derived from satellite". (pages9-10, lines 32-1)

Discussion:

- PG 4160, line 15. I am inclined to agree that the depth of the DCM is driven by light. Longitudinally: Higher surface [Chl-a] in the western basin would cause high light attenuation (self-shading) and shallower DCM. The opposite is true in the eastern sector with very low surface [Chl-a], deeper light penetration and, thus, deeper DCM (discussed for Southern Ocean waters in Holm-Hansen et al, 2005). Anonymous Ref#1 suggests the depth of the pynocline contributes. Either way, I think this is a key point! This part of the discussion should be clarified and Figure 9(a) given more prominence. It's a really interesting part of both the seasonal and basin-scale story. It's also globally relevant in that DCM in the Mediterranean and DCM in other oceans appear to be driven/controlled by similar processes (Fig. 9(a)).

Authors response:

As also suggested by referee #1, we provided in the new version of the manuscript a discussion on the potential environmental causes of [Chl-a] and DCM regional variability. This discussion is supported by the Table 4 (new version of the manuscript). As indicated in the text, for regions were winter mixing hardly reach DCM and nitracline, the eastward deepening of the DCM would be mainly explained by the eastward increase in oligotrophy characterized by lower nutrient concentrations and a deeper nitracline.

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These aspects are further discussed in the text page 21 line 20 to page 22 line 11.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2789/2015/bgd-12-C2789-2015-supplement.pdf

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