Biogeosciences Discuss., 12, C1138–C1142, 2015
www.biogeosciences-discuss.net/12/C1138/2015/

Biogeosciences
Discussions
Open Access

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.

Anonymous Referee #3

Received and published: 9 April 2015

This manuscript presents the method used to calibrate a large dataset of fluorescence profiles compiled and collected from various sources for much of the Mediterranean Sea (1994-2014). The main objective is to better understand seasonal and regional variations in vertical [Chl-a].

Based on visual assessment of shape, 5 types of vertical Chl-a profile are established. An algorithm is then used to categorize all profiles from the calibrated dataset. For four key regions of the Mediterranean Sea, monthly and seasonal climatologies are constructed and examined, the latter relative to the MEDATLAS climatology. This manuscript shows spatial and temporal variability in DCM characteristics. Variations in depth are attributed to light. DCM characteristics from the Mediterranean are also

compared with global patterns.

General comments:

Despite the really interesting content, I found it difficult to navigate this paper.

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

- 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, 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.

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.

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

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.

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

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 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.

(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.

(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 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.

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

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)?

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)).

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