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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.

Anonymous Referee #1

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Review of "On the vertical distribution of the chlorophyll a concentration in the Mediterranean Sea: a basin scale and seasonal approach" by Lavigne1 et al.

This article provides a statistical analysis of chlorophyll a (Chl) profiles in the Mediterranean Sea. They compile ∼7000 quality controlled fluorescence profiles from the region. These profiles are biased a little to the central and western sea. The authors then classify the profiles into 5 types principally, homogeneous, deep chlorophyll maximum (DCM) and three variants. They then illustrate the annual cycles of Chl profiles for 4 locations and then present histograms of the profile types for 5 subregions of the sea. As expected, these show that for most of the year (spring through summer), Chl is characterised by a DCM, but that from autumn through winter, Chl profiles be-

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come more vertically homogenised. The authors then present an analysis of the DCM – showing a longitude trend and that the DCM tends to be deepest in late summer. Finally they present scatter plots of the DCM depth as a function of surface Chl and as a function of the maximum DCM value. They also present plots of the width of the DCM as a function of its peak value.

This article addresses an important topic, which is the relationship between the vertical distribution of phytoplankton (proxied by Chl) and the surface values. The surface values are relatively easy to measure by satellite, but it is the vertical stock of plankton that controls the total water column primary production with ramifications for the entire food web and other biogeochemical processes such as carbon uptake.

The authors appear to recognise this (page 4143 line 20 ff) and the main effort of this article is to provide a statistical analysis of the vertical profiles. I think this is a good start, but think that this statistical description of the Chl, concentrating on the DCM, is of limited use. The paper would be substantially improved if the authors - A) describe the vertical distribution of Chl in context of the background density structure, and B) look at the relationship between vertically-integrated chlorophyll and surface chlorophyll.

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

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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.

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

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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.

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.

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

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).

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