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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 #4

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The paper presents descriptive analysis of a large dataset of chlorophyll profiles obtained from in situ fluorometers in the Mediterranean sea. The authors were careful to correct these profiles using the latest approaches for non-photochemical quenching processes. The paper thus provides an interesting glimpse into the spatial and temporal variability of the chlorophyll profiles in the Mediterranean. A nice aspect of the paper is the identification of different chlorophyll profile types and figure 5 showing how these profiles types changes with time.

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?

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.

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! The thermocline depth could also be plotted in some way.

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

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.

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.

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