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## ***Interactive comment on “Spatio-temporal variability of micro-, nano- and pico-phytoplankton in the Mediterranean Sea from satellite ocean colour data of SeaWiFS” by M. Sammartino et al.***

### **Anonymous Referee #2**

Received and published: 25 March 2015

Sammartino and co-authors investigated on the seasonal to inter-annual variability of the algal size community structure over the Mediterranean Sea using SeaWiFS data. The authors firstly compared, against in situ data, the performances of two existing abundance-based approaches (i.e., only based on chlorophyll concentration) developed for global scale applications. Then, they applied the best performing algorithm to the entire mission of chlorophyll concentrations retrieved from SeaWiFS data and estimated using an algorithm specifically developed for the Mediterranean Sea. Spatio-temporal variability was finally discussed and interpreted along with the physical forcing.

General Comment Several bio-optical algorithms have been developed for improving

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the knowledge on phytoplankton distribution over the world's oceans. However, determination of phytoplankton size classes by remote sensed data is still a debate as algorithms can perform with a different accuracy depending on the optical properties of the water body. The effort pursued by the authors is greatly appreciated as the Mediterranean Sea generally falls in that category where globally-validated algorithms generally need to be regionally validated and adapted. The presented exercise has a potential for studies related to biogeochemical cycles or to climate changes in the Med Sea. However, when I read this submission, many concerns regarding the validation and application of the models, the analysis of seasonal and inter-annual variability and also the grammar arose into my mind. In the present form the paper is not suitable for publication.

My major concerns:

1) Validation of the Hirata and Brewin models against in situ data is the weakest point of the manuscript. The authors said that the fraction of each size class was computed according to Brewin et al. (2010). Then, they used these fractions for validating both Brewin and Hirata models. But, the distribution of taxonomically significant pigments among the 3 size classes is different between these two models. Fucoxanthin is associated to microphytoplankton in the Brewin model and to both micro- and nano-phytoplankton in Hirata et al (2011). Chlorophyll b is attributed to nano-phytoplankton in the Hirata model while it is associated only to pico-phytoplankton in the model by Brewin et al. (2010). This means that the validation of the Brewin model is right while the validation of the Hirata et al. model cannot be trusted. So, it is not sure that the Hirata et al (2011) model is really the best performing algorithm. A good validation of this model should be performed by computing pigment fraction in the same way of Hirata et al., which is possible from the dataset the authors have. In addition, the dataset used by authors for validation seems to include a part of data used by Hirata et al. for developing the algorithm. If it is the case, the validation is not independent.

2) About the analysis of the inter-annual variability: although Sammartino et al. had

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a long-time series of PFT data, all the analyses referred only to 2 months (April and August). Why? There is variability in the size community structure during the other months? In addition, the authors just described the results from the maps (Figures 5 and 6) without being helped by any statistics or techniques generally used in the analysis of time-series. For instance, a good analysis could be to look at the anomalies of each year from the mean both at the basin and sub-basin scales. Such a analysis could be helpful also for studies connected to climate changes.

3) In the case of the grammar, I suggest the authors to ask a English speaking person to review the manuscript before any other submission. As it is, the manuscript is difficult to follow. Some paragraphs are chaotic, others consist just in 2/3 lines.

A list of minor comments:

Page 163 lines 7-18: Many concepts in a few lines.... this makes the paragraph chaotic. In addition, no references are used! I suggest you to refer to appropriate papers.

Page 163 lines 19-22: what do you mean?

Page 163 from line 23 to the end of the paragraph: This is a key paragraph of your introduction, so you should develop it better.

Several parts in the introduction: you introduced several concepts which have been observed in previous works. You have to cite these papers. Several times you miss to cite.

Page 164 line 13: two papers commonly cited about pigment packaging effect are Morel and Bricaud (1981, Deep Sea Research, 28: 1375-1393) and Bricaud et al. (2004, Journal of Geophysical Research 109, C11010). The packaging effect depends also on pigment cellular concentration and not only on the cell size. "Dimension" is not appropriate, please use "size".

Page 165 lines 9-14. I suggest you to split the existing methods into 2 categories

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(according to Brewin et al., 2011, Remote Sensing of Environment, 115, 325-339): spectral-response based approaches and abundance-based approaches. Then, I suggest you to explain more carefully why you chose to test abundance-based approaches instead of the other type.

Page 166: you say that the Mediterranean has peculiar optical properties. This is true. However, my feelings are that you need to explain it better. 3 main hypotheses are generally assumed to be the cause of the deviation of the Med Sea optical behaviour from global bio-optical models for case 1 waters: high CDOM content (Morel and Gentili, 2009, Biogeosciences, 6, 2625-2636); Saharan dust (Claustre et al., 2002, Geophysical Research Letters, 29(10), 1469); and a higher abundance of coccolithophores with respect to other algal groups (Gitelson et al., 1996, Journal of Marine Systems, 9, 283-290). A reader who does not usually work on this area maybe could not understand.

Page 168 line 5: I suggest you to explain how this algorithm is specific for the Mediterranean Sea. Does it consider, for instance, the high CDOM contribution occurring in the Med Sea?

Section 2.2: In general when you describe a data set, it is useful to add more information in the text or using a table. It is important to know for instance: number of samples, period/season, depth of sampling, location, sources. In addition a map would be also useful to display the sampled stations. Methods of analysis are also important. I suggest you also to write the formula you used to calculate pigment fractions.

Section 4: In addition to my concerns about validation, I found this section poorly written. In effect, the statistics you calculated is reported just in the table and it is not used in the text. Saying “falls well” or “fit better” is not appropriate, you have to strengthen what you are observing and persuade the reader using statistics.

Page 171, Lines 1-14: why here?

Section 5: I found very difficult to follow the results on Figure 2. It is a too busy figure.

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Maybe, a map should be better. In addition, when you describe the trends across the Med Sea you refer to the various sub-basins. However, you don't provide any indication (at least not before figure 8 and section 7) about the location of the various sub-basins across the Med Sea. I suggest indicating them at the beginning of your results, hopefully using a map. This could help the reader.

Section 5.3: why did you focus only on the North Aegean and Adriatic Seas. There is a specific reason?

Section 6: I expected from this paragraph to understand if there was variability or not in PSCs among the years you studied. You just described the variations. Statistics could be helpful to understand if the very small variations you observed (in Figure 7) are significant or not.

Page 177 line 1: "relevant processes": which ones?

Section 7: It could be useful to compare you results with those found in the same areas in other studies.

Page 180, lines 16-18: Not sure you can say this. To corroborate this aspect, I suggest you to adapt the model to the Med Sea and then to analyze differences with respect to the original version.

Figure 2: it is very difficult to read.

Figures 5 and 6: they are too small.

Figure 7: I suggest using the same scale for April and August.

Figure 8: For example, which one is the Alboran Sea among the 4 red squares?

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Interactive comment on Ocean Sci. Discuss., 12, 161, 2015.

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