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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 #1

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M. Sammartino and co-authors investigate the seasonal and interannual variability of three phytoplankton size classes (pico-, nano and micro-plankton) in the Mediterranean Sea, using SeaWiFS observations and an empirical model that relates total chlorophyll to the three phytoplankton size classes. The authors start by evaluating the performance of two models that estimate phytoplankton size classes from total chlorophyll, using in situ observations of phytoplankton pigments in the Mediterranean Sea. Based on this evaluation, the better performing model is chosen and used to estimate phytoplankton size classes (fraction of each size class to total chlorophyll) from SeaWiFS estimates (entore mission 1997-2010) of total chlorophyll in the Mediterranean Sea, estimated from a regional bio-optical model developed to account for both case 1 and

2 waters, and tailored for the Mediterranean Sea. The paper then describes regional, seasonal and inter-annual variations in phytoplankton size classes and total chlorophyll in the Mediterranean Sea over the 1997-2010 periods. These changes are discussed in the context of changes in physical forcing and current oceanographic understanding in the Mediterranean Sea.

There is increasing effort use ocean-colour remote sensing for deriving additional information about the phytoplankton, other than chlorophyll biomass, such as size structure. This manuscript addresses an important issue of interest to ocean-colour scientists and to the wider marine community. Information on phytoplankton community structure is important to biogeochemical modellers, for ecosystem studies and for understanding the marine carbon cycle. Whereas, recent studies have looked at phytoplankton community structure in the Mediterranean Sea, such as that of Uitz et al. (2012, Global Biogeochem. Cy., 26, GB2024) and Navarro et al. (2014, Remote Sens. Environ., 152, 557–575), to my knowledge, the abundance-based approaches for estimating size-fractionated chlorophyll from satellite have not been tested (directly) in the Mediterranean Sea. Therefore, there is potential in this work.

However, I feel that the paper requires major work to strengthen aspects of the analysis and to improve the grammar of the paper. My comments are partitioned into general comments, which focus on particular aspects of the paper which I feel require addressing, and specific comments aimed mainly to improve the grammar of the paper and address some more minor issues. I hope you find my comments useful.

General Comments

A) The comparison between the Brewin et al. (2010) and the Hirata et al. (2011) algorithms is not a fair comparison for a number of reasons:

(1) the parameters of the Brewin et al. (2010) algorithm were developed from data collected solely in the Atlantic Ocean (assuming you are using the parameters from the Brewin et al. (2010) model? Are you?), whereas the Hirata et al. (2011) algorithm was

tuned to a global dataset inclusive of measurements collected in the Mediterranean Sea (see their Fig 1). The Mediterranean data used by Hirata et al (2011) is from SeaBASS and, considering your dataset is also from SeaBASS, your evaluation of this algorithm is not independent!

(2) The method used to estimate size fractions from pigments follows that of Brewin et al. (2010) and not Hirata et al. (2011). A fairer assessment would be to use the HPLC method to determine each size class developed for each approach. Note for instance, Hirata et al. (2011) uses a fucoaxanthin adjustment, which probably favours the Brewin et al. (2010) for the fraction of microplankton (e.g. note the better fit to the fraction of microplankton at low chl-a in your Fig. 1 a). The reason the Hirata et al. (2011) algorithm produces higher estimates of nanoplankton is that this fucoaxanthin is attributed to nanoplankton at low TChl-a (not done by Brewin et al. 2010) and also that chl-b is attributed to nanoplankton (attributed to picoplankton by Brewin et al. 2010). The fact that the Hirata et al. (2011) fits the data better than the Brewin et al. (2010) model, despite using the Brewin et al. (2010) HPLC method is strange, and if anything, tells you how different these relationships may be in the Mediterranean Sea.

B) A fairer way of inter-comparing these size-class models would be to partition your Mediterranean data in two datasets: one for algorithm training (say 80% of the data) and one for model validation (say 20% of your data, judging by Fig 1 it looks like you have lots of data?). Then re-tune these algorithms (simple least-square fits) to the training datasets, and evaluate their performance on the validation dataset. This is what Hirata et al. (2011) did? To me it seems a little naïve to assume these algorithms would work automatically in the Mediterranean Sea, when considering they have been developed using datasets from many different regions of the ocean, and given the unique bio-optical characteristics of the Mediterranean Sea? This would also:

1) likely result in more realistic satellite products of size-fractionated chl-a in the Mediterranean Sea (Figs. 3-9)

2) provide an additional novelty to your paper, as to my knowledge, the Brewin et al. (2010) and Hirata et al (2011) algorithms have not been tuned to the Mediterranean Sea before.

If you do this (and also with respect to your initial comparison in the paper), be careful when comparing the Brewin et al. (2010) and Hirata et al (2011) algorithms directly, as one should also consider the number of model parameters used in the approaches. The Hirata et al (2011) model uses 8 parameters to compute the three size fraction (3 for microplankton and 5 for picoplankton (8 for nano as it is computed as the difference between the two)) whereas Brewin et al. (2010) only uses 4 parameters (2 for pico, 2 for micro (4 for nano)). This can be done using the Akaike Information Criterion, which evaluates if there is a significant improvement in model performance when using additional parameters.

C) Certain sections are poorly written (e.g. Introduction and Section 7), while others are better (e.g. abstract and discussion). The writing needs to be improved in certain areas. I have listed many cases in my specific comments below, but there are likely to be examples I have missed. Please go carefully over each sentence of the paper and make sure it is grammatically sound. There are many cases throughout the manuscript where there are one line paragraphs? Was this intentional? If so it is not good grammatically? Or was this a formatting issue?

D) There may be seasonal and inter-annual changes in the relationships between size fraction and total chlorophyll presented by Hirata et al. (2011) (and Brewin et al. (2010)). This is something that needs to be discussed, considering the application of the models to the entire SeaWiFS time-series (1997-2010). If there are shown to changes with season or with years, this may cause biases in your results.

Specific comments

Page 163: Line 20: Racault et al. (2012) paper focuses on phenology (derived from total chl-a) and not phytoplankton community composition.

Page 164: Line 18: suggest changing “the climate change” to “climate change”, i.e. remove “the”.

Page 164: Line 23: suggest changing “the flow cytometry” to “flow cytometry”, i.e. remove “the”.

Page 164: Line 27-28: suggest changing “it is possible to collect a considerable dataset of” to “there exists considerable data on”. Also be careful here, none of these datasets provide dimensions of the phytoplankton. In all cases dimensions are inferred from the in situ data (flow cytometry (scattering and fluoroscence signatures); HPLC (pigments); spectrophotometry (light absorption)). There are also other methods, for instance, filtration of water through filter-pads of a known size together with in vitra fluorometric chlorophyll-a extraction is one of the oldest methods of measuring size fractionated chlorophyll.

Page 165: Line 6: after the word “backscattering”, I would suggest adding “, derived from remote-sensing reflectance,“

Page 165: Line 9: suggest changing “the recent years” to “recent years”, i.e. remove “the”.

Page 165: Line 10: change “the optical variable” to “optical variables”

Page 165: Line 18: “radiance measure the chlorophyll a effect” does not make sense, please clarify.

Page 165: Line 19-20: Absorption and backscattering are not single variables, these coefficients vary with wavelength!

Page 165: Line 23: Suggest changing the word “sunders” to “partitions”

Page 165: Line 26: These models were not all applied at global scale (Fujiwara et al. 2011, for instance, was based in the Chukchi and Bering Sea)!

Page 166: Lines 4-5: This sentence does not make sense? Please re-write.

Page 166: Lines 13-14: I don’t like the use of the word “shows singular” here, I would suggest changing to “has unique” and add “when compared with other regions” after the words “water column”.

Page 168: Line 11: the word “through” is spelt wrong.

Page 168: Line 12: “well performs with” does not make sense, I suggest changing it to “performs well when compared with”.

Page 168: Line 13: Suggest changing “the most of the” to “most”.

Page 168: Line 14: What units are RMS and bias in? Are they log10 units? Please clarify?

Page 168: Lines 17-23: It would be useful to have additional information on the in situ data here? How many measurements are they? What is the distribution of the in situ data? A table detailing this would be useful?

Page 169: Line 19: This is not true, the Brewin et al. (2010) model does not compute each size class using a separate exponential expression. The model only is fitted to two size classes (pico- and pico+nano).

Page 170: Lines 2-5: You have already described the datasets in section 2.2?

Page 170: Lines 12-14: Not surprising considering the Hirata et al. (2011) model was parameterised using a fucoaxanthin adjustment on the HPLC data for micro, that was not used on the in situ data here?

Page 170: Line 15-18: Worth considering the reason why the Hirata et al. (2011) model has a different shape to the Brewin et al. (2010) model for nanoplankton, irrespective of a comparison with the Med data. This is because in the Hirata et al. (2011) approach fucoaxanthin is added to nanoplankton at low chl-a, and chl-b is included in nanoplankton, not done in the Brewin et al. (2010) model.

Page 170: Lines 19-21: Worth considering differences in the number of parameters

in the two models used to compute picoplankton fractions (2 for Brewin et al. (2010) model whereas 5 for the Hirata et al. (2011) model).

Page 170 and Table 1: Is the improvement in the Hirata et al. (2011) model significant? When considering also the number of parameters in the models?

Page 171: Line 5: "10-log" should be "log-10"

Page 171: Line 10: "a zoom"????

Page 173: Line 5: Suggest changing the word "rising" to "transfer".

Page 173: Line 29: "A widespread of micro component" does not make sense?

Page 174: Line 3: Suggest changing the words ". . .which is at west of the.." to ". . .which is west of. . ."

Page 174: Line 5-6: "..due to a new mixing of the water afterwards the break of the thermocline.." please improve English (e.g. "due to the water column becoming mixed after the breakdown of the thermocline")

Page 176: Line 3: Not surprising really, considering the model uses Chl-a only as input?

Page 177: Line 23: Remove the words "that is" and change "bioregions" to "regions".

Page 179: Line 1-3: Please re-write these sentences.

Page 179: Line 28: "In Fig. 9d and a constant increase.." does not make sense please re-write.

Page 180: Line 8: Suggest adding the word "unique" in front of the word "optical" in this sentence.

Page 180: Line 16-18: I am not sure that your analysis "revealed that the Hirata model, even if developed for the global ocean, can be used also in the Mediterranean Sea without tuning its empirical coefficients." It is very likely that the model will improve in

the Mediterranean if tuned to data in the Mediterranean!

Page 183: lines 20-21: Change "river run off grows up" to river run off increases"

Page 184: lines 13-15: I am not convinced that you "clearly demonstrated that pico, nano and micro classes often coexist and their relative contribution to TChl a cannot be neglected", these are relationship fixed in the Hirata et al. (2011) model?

Table 1: I am not sure how useful the RMSE and MBE are in linear space. Is your dataset not log-normally distributed with respect to total chlorophyll? For instance, in oligotrophic waters a RMSE of greater than 0.15 mg m-3 is very large when considering the concentrations of pico-, nano- and micro- are likely much lower? I would suggest providing statistical results in log-space, if your dataset is log-normally distributed? These statistical tests assume normal distribution, and I strongly doubt your dataset is normally distributed in linear (normal) space.

Figure 1: Requires units on the x-axis.

Figure 2: Requires units on the x-axis (assuming this is degrees?).

Figures 5 and 6 are very difficult to see? Also I can barely read the units *unless I zoom right in on the PDF).

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