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Interactive comment on "Merging bio-optical data from Biogeochemical-Argo floats and models in marine biogeochemistry" *by* Elena Terzić et al.

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Referee comment on "Merging bio-optical data from Biogeochemical-Argo floats and models in marine biogeochemistry" by Elena Terzić, Paolo Lazzari, Emanuele Organelli, Cosimo Solidoro, Stefano Salon, Fabrizio D'Ortenzio, and Pascal Conan

The paper discusses the results of the analysis of ∼1300 Biogeochemical ARGO profiles (Temperature, salinity, Chlorophyll fluorescence, downwelling irradiance at three wavelengths and downwelling PAR) generated in different regions of the Mediterranean sea, though covering a large portion of it, by 31 profilers in the years 2012-2016. The analysis is based on the comparison among measured profiles and profiles derived by merging different bio-optical models and 1D biogeochemical simulations based on a 3D

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coupled biogeochemical model, the OGSTM-BFM (see text for refs). The wide scope motivation is that (P.2 L.22-24): Specific studies are required to demonstrate to what extent the assimilation of radiometric data can improve the model skill in simulating key biogeochemical variables (e.g. nutrients, primary productivity).

More specifically the authors want (P.2 L.32-34):

1) to show how it is possible to integrate BGC-Argo float bio-optical data and a simple 1-D model to investigate chlorophyll vertical dynamics; 2) [how] to use such a tool on a sufficiently large data set in order to test different bio-optical models

The text is unclear on a few key issues related to the protocol followed for the simulations (see below). Each simulated profile is generated using the vertical distributions of physical and chemical variables without considering horizontal processes, as the authors write on P.4 L.13-15 ..therefore implying that mass exchanges due to horizontal diffusion and baroclinic components of the (upper ocean) advection field are assumed to be smaller compared to vertical processes and biogeochemical dynamics The impact of this assumption depends on the time scale of integration and on what are the initial conditions of each run, which is not clearly explained.

A complementary scope is (P.5 L.13-14) ..[to assess] the possibility of using biogeochemical models also when [underwater] PAR measurements are not available, [comparing] the skill of different bio-optical models, which it is generally the rule.

The indicator for testing the performance of the models is the DCM depth, that obtained by the simulations vs. the observed depth, while a minor relevance is given to the DCM amplitude.

The main results of the study are: 1. an assessment of the performance of different formulations and/or parametrizations of the light penetration in the water column in relation to the concentrations of optically active components and 2. that PAR is more important than mixing and nutrients in determining the capability of the model in reproducing in situ chlorophyll profiles.

Indeed, testing different formulations and parametrizations in a model is useful not only to find the best performing model but, more importantly, to analyze the interplay among different mechanisms in generating observed pattern or dynamics. This part is often lacking in the discussion. For example the reason why different optical models produce different depths of the DCM varying with the area is not discussed.

More important, there is a key conceptual issue in the manuscript, at least from what I could grasp from its present version. The authors compare the chlorophyll vertical profiles, obtained from different bio-optical models and with different values of turbulent diffusivity, with those measured in situ, without discussing the impact on the profile of nutrients, phytoplankton loss due to grazing and all the other processes simulated by the OGSTM-BFM. I believe that the rationale for this is the assumption that the biogechemical module is always the same and then any differences in the results would depend only on the change of the specific driver tested. Even ignoring any possible non-linearity in specific processes, e.g., the nitrogen dependence of the photoacclimation by phytoplankton, the best performance of the model in reproducing the depth of the DCM cannot be attributed only to the tested drivers since equally important processes are in the background and not discussed at all, besides some mention to phosphate which is substantiated only by the model outputs. This makes me thinking that the authors consider the 'geochemical' fields produced by the OGSTM-BFM as real data instead than simulated data. This might be a reasonable assumption for large scale patterns but it is a little weaker for daily simulations in single sites that are moved in time. The effectiveness of a bio-optical model should be tested against IOPs or AOPs, as it is already been done also for BGC-ARGO proflers, not via an end product, i.e., chlorophyll a, whose concentration depend on many other processes. This would also help in clarifying which mechanisms drive the differences reported in Figs. 10 through 12.

In addition, it not clearly explained, or I might have missed where, if all the state variables simulated by the model were reset each day to the 3D model values for that day and that site, as one might guess from lines 30-33 on P.5 or if, as in a normal 1D simulations, they are produced by the model. In either case I guess some discrepancies should arise, which are neither mentioned nor discussed in the paper.

While acknowledging the effort invested in the study it looks a bit empirical and I am not convinced that it adds new knowledge to the existing one.

Besides solving a couple of issues mentioned in the detailed comments, I suggest to revise the paper analyzing in more detail what are the mechanisms driving the simulated differences and discussing in more detail the extent to which the OGSTM-BFM drives the DCM depth which is the prognostic variable that the author use to test the performance of the different sub-models tested.

Detailed comments

Abstract (It should be substantially re-written. Following are some suggestions).

L.3-4 ...Data set comprised of ..Argo Floats does not seem correct. I suggest to rephrase as: The present work is based on a dataset comprised of 1314 0-1000 m vertical profiles of biogeochemical and optical data measured by 31 Biogeochemical (BGC) Argo floats in the Mediterranean Sea from 2012 to 2016.

L.4 The data set was integrated in ...sounds a little confusing since the simulations are 1D. I suggest to rephrase as: 1-dimensional model simulations, using measured photosynthetically available radiation (PAR) profiles as light input, were then carried out for each profile along the trajectories of the floats.

L.6-7 The simulations were aimed to be consistent with data measured by float sensors, especially in terms of the deep chlorophyll maximum (DCM) depth. I suggest to rephrase as: The simulations were aimed at reproducing the profiles measured by float sensors, especially for what the deep chlorophyll maximum (DCM) depth concerns.

L.7-9 I suggest to rephrase as: We tested several light models to estimate their impact on modeled biogeochemical properties taking into account self-shading, derived from vertical chlorophyll distributions, and colored dissolved organic matter (CDOM) concentrations.

L.9-11 I suggest to rephrase as: The results, corroborated by the comparison with in-situ BGC-Argo profiles, illustrate how PAR penetration and vertical mixing modulate the dynamics of primary producers along the water column.

L.12 Highest?

L.13 Simulation results show also that...

L.14-15 After reading the paper I am not convinced that The approach here presented serves as a computationally smooth solution to analyse BGC-Argo floats data and to corroborate hypotheses on their spatio-temporal variability.

Intro

P.2 L.6 Density? More clear the high number of active BGC-Argos

P.2 L.7 ..numerical experiments of that kind. Unclear. Better: to analyze the predicting capability of bio-optical models, if this is the scope

P.2 L.19 ones

P.2 L.6-24 To better clarify the scope of the study it would be better to invert the sequence of the arguments. If the scope is to: ..to demonstrate to what extent the assimilation of radiometric data can improve the model skill in simulating key biogeochemical variables (e.g. nutrients, primary productivity) which comes as a possible improvement of what already done and sketched before, then this statement should come first. Then all the motivations for using Med data as a test case. If, alternatively, the scope is to improve our understanding of Med functioning then then all the paragraph should be changed accordingly. Reading the manuscript the first possibility seems to hold true.

Methods

P.3 L.17 ..were then vertically interpolated to a resolution of 1 m in the upper 400 m. Do the authors mean 'fitted'? If the sampling resolution was 1 m why to interpolate them? What about the data below 250 m? Were they extrapolated?

P.3 L.19-21 Could the authors be more explicit on which part of the Baird et al (2016) model they used and with which input variables? This can go in SI.

P.3 L.21 A second approach. There is no first before.

P.3 L.25 please rephrase as: ..measure Chl a concentration using as a proxy its fluorescence emission in the red band (690 nm) after blue excitation at 470 nm (Holm-Hansen et al., 1965)

P.3 l.27 remove it

P.5 L.20 ..levels

P.5 L.25 ..characterized regarding..? ..quantified using?

P.5 L.35 ..allow a gradual increase. . . decrease?

P.7 eq.1 I might be wrong but as written and with sigma-MLD = 0.3 the first term becomes negligible at the depth of 2 m

P.9 L.5-10 The whole paragraph is a little confusing because the authors introduce the seasonal mixing due to de-stratification without clarifying that this is likely taken into account by the measured change of the MLD and not by their formulation of mixing (Eq. 1).

P.10 L.29 remove as

Fig. 5 The legend could be compacted and the three figures could become one three multipanel figure

P.18 L.2 are hardly what? Constrained?

P.20 L.23-28 Do the authors implicitly assume that CDOM concentration is higher in

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the WMed? This could said more explicitly.

P.27 L.12 The most fitting? May be: The best alternatives to fit the data.

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