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Biogeosciences Discussions

# *Interactive comment on* "Seasonal and inter-annual variability of plankton chlorophyll and primary production in the Mediterranean Sea: a modelling approach" *by* P. Lazzari et al.

**Anonymous Referee #1**

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The paper "Seasonal and inter-annual variability of plankton chlorophyll and primary production in the Mediterranean Sea: a modeling approach" by Lazzari et al. proposes an exhaustive analysis of the seasonal and interannual variability of chlorophyll and primary production in the Mediterranean Sea. The study is based on the outputs of a specifically developed model, which was further adapted to the Mediterranean characteristics. The authors focused mainly on the horizontal and vertical gradients of surface chlorophyll and primary production as well as on the vertically integrated properties. I found the paper really interesting. It presents a "science" model (rather than an "operational" model), which allows exploring the present days uncertainness in our knowledge of the Mediterranean basin ecosystem. I was only slightly disappointed as the tool that



authors developed could be exploited more in depth to address fundamental questions of the Med (I will give some lines in the next). However, I understand, and I hope, that further publications will follow. I then suggest publication with minor revisions, which are indicated in the next.

General Comments:

1. one of main concerns about the paper conclusions and results is the role of the mixed layer in structurating (vertically, horizontally and temporally) primary production in the Med. Authors discussed this point in different parts of the paper (particularly for fig 10). However, in my opinion, a more general discussion should be done. The point is not trivial. For example, I supposed that results obtained on the Alboran Sea are strongly sensitive to the accuracy of the modeled 4 dimensional variability of the mixed layer depth. Again, as the authors noted, the timing of mixed layer stratification and bloom start is crucial to realistically simulate phytoplankton variability. Indeed, a relatively slight error on the mixed layer evolution (which should have low or zero impact on the simulated physical characteristics of the basin) could strongly impact on the chlorophyll and primary production estimates. I suggest a discussion on the role of the mixed layer on the observed gradients. I also suggest a better description of the physical model characteristics (i.e. which surface fluxes have been used to force it?? At which resolution??) and performances (i.e. does seasonality and depths of the mixed layer of the model matches with existing data? What is the role of advection?), in particular for the surface and sub-surface layers. The two papers cited to indicate performances of the model are, at my knowledge, more focused on water mass formation than on the surface layer.

2. the other point I suggest authors should improve concerns the use of the light attenuation coefficient from satellite to constraint model. If I well understood, authors used satellite maps to derive K values, which are then used to propagate surface irradiance at depth. Again, authors insist on the high sensitivity of the system to this parameter (pag 14 line 9). Ok (but see later, specific comments). On the other hand,

one of the main results of the paper is that, in the Mediterranean, surface (i.e. satellite derived) PP fields are not uniformly consistent with integrated (i.e. vertically integrated) PP fields. In summary, if I well understood, the spatial distribution of the int-PP of the model strongly depends on the surface satellite k products, although, in general, satellite surface values are considered not consistent with integrated PP. Could the authors be more precise on this, probably only apparent, contradiction??

3. the last point I suggest (related to the first one) concerns the discussion about the Longhurst models on the Mediterranean Sea. I found contradictory that authors discussed only one model of Longhurst when they demonstrated that Mediterranean dynamics is, conversely, characterized by several gradients and different behaviors! I suggest to better exploit model outputs (using more intensively MLD information, see point 1) to analyze the application (or not) of the Longhurst model on the ecoregions defined by the otained gradients.

Specific comments

Pag 4, line 12. Please specify LTER acronym

Pag.5, transport terms paragraph. Please indicate surface forcing data used to force the model; their spatial and temporal resolution and, if possible, their impact (i.e. sensitivity) on the simulated mixed layer depths. Please also indicate the years simulated.

Pag. 7. At my knowledge, satellite standard product is the attenuation coefficient at 490 nm. How the authors calculated Kpar from K490 (Ksat)?? What ocean color products are used?

Pag. 9. Lines 29-32. I was surprised of the 1 month delay of the annual peak of integrated chlorophyll between DYFAMED data and model outputs. The authors seem minimizing this point, although it is the only validation point of the model with in situ data. Satellite data seem, conversely, well reproduce the timing of the DYFAMED data (i.e. peak in march-avril). Please, try to better explain the discrepancy observed. Maybe



I'm repetitive, but I strongly suppose that mixed layer dynamic is the main responsible.

Pag. 11. Lines 8-15. Discussing on the Alboran Sea, authors neglected the role of the Atlantic Water on the phytoplankton dynamic. The layer of fresh water of atlantic origin avoids any biomass growth in the area, which is observed only when important vertical velocities (i.e. Alboran Gyres) exists. How Atlantic Water spreading is reproduced in the model?? What is the principal source of nutrients in the area to sustain the patch of int-PP depicted in figure 7c?? Please specify.

Pag. 13, lines 23-29. Looking at figure 9, my impression is that two main clouds of points exist. Trying to impose a unique linear relationship is evidently not suitable. However, maybe seasonal relationships are more informative. Have the authors tested seasonal regressions?? Could authors plot points in the fig. 9 scatter plots following different colors for different seasons??

Figures 4. Please add regions labels on x-axis?

Figure 5. Legend. Dyfamed is not a mooring as indicated in the paper. Please rectify.

Figure 6. Very interesting figure. Why limited to only three regions? I suggest to modify y-axis or use log axis to better illustrate oligotropgic region (not easy to understand LEV panel)

Interactive comment on Biogeosciences Discuss., 8, 5379, 2011.