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Interactive comment on "The Mediterranean subsurface phytoplankton dynamics and their impact on Mediterranean bioregions" *by* Julien Palmiéri et al.

Anonymous Referee #1

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Overall assessment

In this work, the authors use a 3D coupled hydrodynamic-biogeochemical model of the entire Mediterranean basin to investigate the dynamics of subsurface chlorophyll accumulations and its relative contribution to total plankton production. They compare surface model results and satellite chlorophyll estimates (also from the surface) with integrated (300m) simulated phytoplankton values and conclude that the contribution from the subsurface levels is important and provides a very different picture of the usually accepted 'oligotrophic' Mediterranean Sea. Model simulations are also assessed against the newly available bio-ARGO data to understand to which extend simulated

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vertical chlorophyll values match with field data.

The topic here studied is highly relevant for our understanding of functioning of the Mediterranean Sea ecosystem and also to better evaluate the generic oceanographic knowledge usually provided by surface-only information, as the one obtained from remote sensing. However, and as much as I liked the topic and approach used, I have severe concerns about the suitability of the used model to address the scientific questions being asked in this work. The authors have tried hard to overcome the obvious limitations of the model which, on the other hand, is common to ALL modelling approaches but I still have some concerns as detailed on the following paragraphs.

General comments:

My major concern regards the lack of concordance between simulated and measured chlorophyll values. First for the surface chlorophyll values. From the map in Fig. 1 it could be quite obviously seen that mean simulated surface chlorophyll values are much lower almost everywhere than satellite (even if the chosen color-scale makes the comparison a bit hard). This is confirmed by the seasonal cycles shown in Fig. 3 where the sub-estimation of chlorophyll by the model at surface is plain for all investigated sites (as the authors state: model surface Chl globally under-estimates satellite values by a factor 2). In the following paragraph this difference is partially justified by known biases in satellite estimates for the Mediterranean Sea but I am totally sure that satellite information is not that far away from field measurements.

Then, this sub-estimation of chlorophyll levels is also observed for the deep structures. In this case the comparison between model and bio-ARGO data shows '..that the model underestimates the Chl concentration, not only at the surface, but also at depth by 60Further, from the comparison made in Fig. 11 it is quite clear than not even the relative chlorophyll (with respect to its maximum monthly value) is properly simulated by the model (at least for the western Mediterranean regions).

These deviations commented above are, in my opinion, large enough to prevent using

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the model for the intended analysis on the chlorophyll phenology. I appreciate the effort made by the authors to make the comparison model/data quantitative and to provide hypothesis on why the model fails to reproduce observed patterns. As stated in Appendix, the too-deep nutricline (especially for phosphate) seems to be the reason of the observed differences. Either the use of fixed internal nutrient ratios or (more likely) hydrodynamic model deficiencies being the causes of the miss-matches. The fact that another biogeochemical model coupled to the same hydrodynamic data improves the DCM simulation but worsens the surface conditions make me wonder if maybe NEMO (at least in the current configuration) is an appropriate choice for making biogeochemical simulations in the Mediterranean Sea. I am aware this model is being widely used in this basin but the results shown in this submission are somehow worrisome, at least when it comes to simulate the biogeochemistry.

I have also some minors comments on the wording of the manuscript, especially when considering the model/data deviation (as the authors are overly optimistic in my opinion) but I am not providing them in here as unless the authors could generate a new simulation in which the basic characteristic of the DCM (and of the chlorophyll in general) are better aligned with the observations I sincerely doubt that this model could be used for the objectives presented in the manuscript.

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