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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.

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Referee report on "Seasonal and inter-annual variability of plankton chlorophyll and primary production in the Mediterranean Sea: a modelling approach" by P. Lazzari, C. Solidoro, V. Ibello, S. Salon, A. Teruzzi, K. Béranger, S. Colella, and A. Crise

The paper analyses some of the results generated by a biogeochemical model simulating the dynamics of plankton food web in Mediterranean sea. The model is the Biogechemical Flux Model, or BFM, based on the functional group assumption, whose equations have been integrated off-line using physical processes and fields generated by a high resolution OGCM, Med 16.

In this paper, which is presumably the first of a series of process studies, the authors focus on the spatial, seasonal and interannual variability of chlorophyll a and of the concurrent patterns of Net Primary Production (NPP), as simulated by the model. They compare their results with satellite derived pigment distributions and NPP, and with the available scattered in situ observations of Primary Production based on C14 incubations.

The rationale for the analysis is multifold. 1. The comparison with satellite derived surface chlorophyll a distributions is an important step for validating the results of the model and its parametrization. 2. The model allows to discriminate between chlorophyll concentrations in the layer directly visible by the satellite sensor and the subsurface concentrations which are inferred from different statistics using satellite data, but are directly computed by the model. 3. The model allows to mechanistically reconstruct the processes producing the observed pigment patterns and NPP, with the obvious caveats linked to any modeling study, while NPP derived from satellite data depends on statistical correlations. 4. Changes in forcing or fluxes with the same model parametrization, which the authors do for light penetration and external nutrient inputs, allows to highlight their role in modulating NPP in the basin.

While the model has likely been implemented also for operational purposes, the authors prioritize the analysis of processes occurring in the basin more than the predictive capability of the model. In addition, their study is one of the few analyzing in detail and comparing the patterns of NPP, whereas the common habit is to analyze state variables.

On the other hand the results are discussed with less breadth they would deserve, thus hampering the impact that their study could have to better understand the functioning of the basin. Even assuming that the authors plan to address many issues in forthcoming papers, I suggest to develop a more in depth analysis on some of the points I will mention below.

BFM is a highly complex model. It includes several processes most of them being, for what this study concerns, loss terms for phytoplankton, feeding back on its growth through partial recycling of nutrients. Since more than 200 equations have been solved/integrated at each step, I assume that the main scope of the effort was to implement a stable model, producing realistic results and to validate those related to primary production and to an observable variable of phytoplankton biomass/acclimation, to test the robustness of the results. In this respect chlorophyll and PP are only a small part of the story. On the other hand this makes more difficult to understand why the model fails, when it fails. This is worth a reflection because the study did not disclose overlooked patterns in the phytoplankton dynamics, thus improving our knowledge on the basin, while this all inclusive approach prevents to get insights from the failures.

On the other hand the study provides an alternative, accessible, spatially continuous estimate of NPP for basin, which can set an alternative reference to satellite products. For this reason I definitely support the publication of the study, but I ask the authors to seriously consider and, when possible, to solve the issues raised by the comments below and by referee #1. Because I am writing this review after having read the comments by referee #1, most of which I agree with, I will focus on additional aspects not highlighted by the other referee.

The authors do not say what is the integration step they used. The reason I am raising the point is because in eq. 6 (suppl.) there is an explicit expression for photoperiod, which suggests that their integration step is one day and the irradiance in eq. 9 is the average irradiance of the day. If so, I am a little perplex on the use of Geider formulation, which estimates dynamic response to light variations. The average light of the day is only a proxy for the light intensities to which the cells are exposed during the day because of the circadian variation. I suggest to run a test on the differences between the acclimation to the average light and the acclimation to the same integrated irradiance but following a typical sinusoidal pattern and make explicit the difference, if any.

The authors compare surface chlorophyll from the model with surface chlorophyll from the satellite. What did they consider as surface ? In general chlorophyll from satellite is the optically weighted pigment concentration, not just the 0 or the 0-10 m value. I do not expect significant changes in regions of low biomass, but the difference can be significant when the biomass is high.

One of the differences that came out from the model results is the presence of higher variability (more than one peak) in the Western Basin vs. a more regular growth season in the Eastern basin. Can the authors comment on this, analyzing in more detail the simulations ?

Chlorophyll accumulation reflect an increase in net growth. Considering the highly detailed formulation of the grazing, could the authors provide a first order estimate on how much of the carbon is grazed in different seasons and how much is exported ? In other words to what extent phytoplankton accumulation is controlled by grazing ?

P.9 LL.9-10 (see also P.14 LL.3-6) Why is consistent? Do the authors assume that the entrainment of IW in the AW in the vicinity of the Strait enriches the MAW with nutrients,thus enhancing phytoplankton new production? Or is it the MAW contribution to the stability of the water column? Or what other mechanism emerges from the model ? Typical MAW has a very low nutrient content.

I found very interesting one of the results, namely the almost negligible impact of atmospheric and terrestrial inputs on the production of the basin. The authors rightly stress that the impact is low in respect to the total production and not to the new production. However, the values they obtained correlate with Nitrogen inputs, which considering that the model follows Liebig rule, but allows for unbalanced growth, should imply that exported particulate should contain excess nitrogen, possibly for fast P recycling. Would it possible to discuss this point in more detail. How the nutrient fields match the reality at the end of simulation?

The most critical point has been also stressed by referee #1: the mismatch in the

WMed spring blooms. Indeed the accumulation during mixed layer deepening [P.10 LL. 23-25], is not impossible, but certainly unusual. For example, It contrasts with the analysis made by Behrenfeld [Ecology 91(2010)977] in North Atlantic. In this respect the anticipation is not only a problem of creating the conditions in the wrong moment but also to create the wrong loss term in the moment under focus. In addition to the comments made by referee #1, which likely pinpointed the key problem, I would conduct an in depth analysis on why the model fails to reproduce a key process in the basin. A better simulation of the spring bloom is not, from my point of view, a prerequisite for the publication of the paper, but it would be much more helpful for the authors and the readers to understand which is the problem.

P.13 LL.23-29 Likewise, could the authors analyze more in depth the areas/times when subsurface production does not show the typical correlation with surface production and discuss the mechanisms behind the phenomenon ? It can provide useful insights on the functioning of those areas.

Minor issues

P.1 L.20 'shows' instead of 'indicates' ? P.1 L.26 'resolving spatial and temporal variations' instead of 'adopting a spatial and temporal description'? P2 L.3 'role of external fluxes and light penetration' instead of 'the role of ecosystem boundary conditions'? P3 L.23 'features' instead of 'consideration'? P.3 L.24 'system instead of 'picture'? P.6 LL.10-14 Please rephrase. It is not clear the meaning of 'does not enhance the effects of nitrogen-limiting...' P.7 L.3 'formulation' instead of 'approximation'? P.7 L.20 'can be considered/taken as' instead of 'can be used'? P.7 LL.23-28 Is the station located outside Gibraltar? P.8 L.18 'estimates' instead of 'estimations' ? P.11 LL.8-9 was the integral really computed down to sea floor? Why? P.14 L.15 I would rephrase as 'could be classified as a . . ...system' It is a classification not a paradigm.

Supplement

The definitions of symbolism in the opening paragraphs are confusing. Eq. 1 for-

mulates carbon accumulation in phytoplankton. The right subscripts are not always consistent with what is written in the second paragraph of the supplement. Sometimes they are sources, some other times they are sinks. In other words the flux is not always from the variable in the subscript to the variable in the derivative. I suggest to change 'the flux i directed from C to A' to 'C and A are the variables among which flux occurs'.

Why semi-labile and refractory parts R_sup(2)_sub(C) have the same superscript (p.2 of supplement)?

Assuming that refractory is R_sup(3)_sub(C) what are R_sup(4)_sub(C) and R_sup(5)_sub(C) hinted form the running index j in eq. 1?

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