

Dear Editor,

please find here our reply to Referees' comments, uploaded as a single pdf file in the Supplement.

First of all we would like to thank the Referees for their comments: we considered very carefully their suggestions which prompted additional analysis and discussion, which might be partially incorporated in a revised version of the paper. In the pdf file you will find a detailed point-to-point reply, where Referees comments are in italics, and authors replies are indented. Figures' references for the original manuscript are indicated with Fig.#N, while references to figures included in the present reply are indicated as Fig.R#N.

REPLY to Referee #1

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 uncertainty in our knowledge of the Mediterranean basin ecosystem.

Thank you.

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.

Indeed this is meant to be the first of a series of papers dedicated to specific topics.

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 structuring (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.

[1] We agree with referee comments. These points are discussed in points [3] and [5] below.

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.

[2] The ECMWF daily forecasts are used to force the OGCM; the net climatic heat fluxes, the Reynolds SST, the freshwater E-P flux and the wind stress, are as described in Barnier et al (1998). --> REFERENCE come fatto per Lalaurette?

Between March 1998 and October 2000, the resolution of ECMWF was 60km. Then from November 2000 to January 2006 it was decreased to 40km (T511). See F. Lalaurette, Changes to the operational Forecasting system, ECMWF Newsletter N 89 Winter2000/01 (<http://www.ecmwf.int/publications/newsletters>).

For what regards MLD comparison with data, see point [3].

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.

[3] We added a longer description of the MED16 OGCM (see point [7] in specific questions) and we compared the climatological maps of MLD of the model with the ones published by d'Ortenzio et al. (2005).

As shown in Fig.R1 Model and data climatological maps presented in d'Ortenzio et al. (2005) are in very good agreement [compare with d'Ortenzio et al. (2005), Fig. 1]. In fact, the mixed layer depth is reduced in summer to values lower than 10 m depth and it can reach 30 m depth in the south-eastern Mediterranean. In winter, the mixed layer depth progressively increases, with values between 80 and 110 m depth in December, and reaches its highest values in February in areas of dense water formation. Despite the fact that the first layers of the model are 6 m depth only, the deepening and restratification processes are well simulated in the model giving confidence on the present study using these fields to transport the biogeochemical properties.

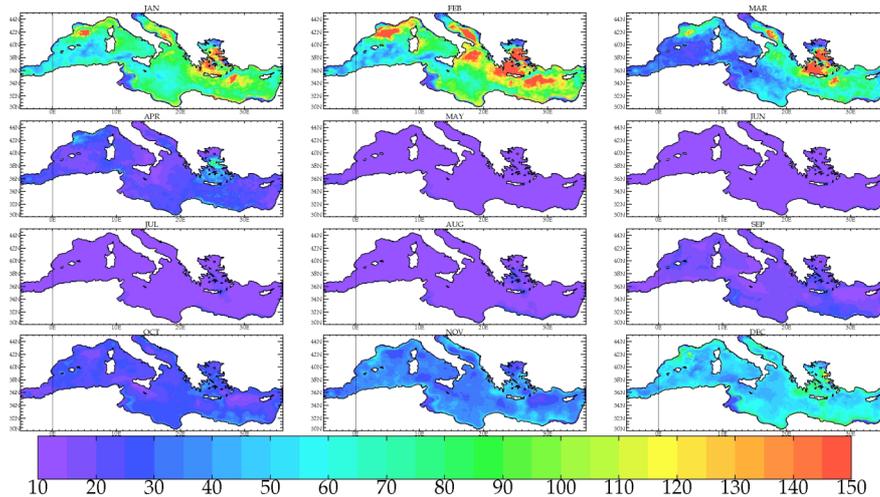


Fig. R1 . Mixed layer depth climatology over the period 1999-2004 obtained using the turbocline criteria (Blanke and Delecluse, 1993).

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??

[4] In the present manuscript, two kinds of surface/vertically-integrated properties intercomparisons are considered:

- A. horizontal maps of primary production, Fig.7 b-c in the manuscript;
- B. scatter plots, Fig. 9, indicating temporal variability of primary production.

The analysis of points A and B shows that the spatio-temporal variability of surface or integrated properties can be different. Therefore the characteristics visible from satellite cannot be extrapolated along the water column, however, as Referee #1 states, satellite maps of extinction coefficient are used, in the simulations, to propagate light along the water column.

As a matter of fact, the sensitivity is not so high in particular for the subsurface layers where the absolute error in Irradiance becomes exponentially small, as indicated by the following relation for the error propagation:

$$\Delta I(z) = (-I_0 * e^{-kz} * z) * \Delta k$$

the absolute error will decay along the water column with an exponential rate, and it can be considered negligible for sub-surface layers.

The low correlation results obtained considering surface and integrated concentration (scatter plot Fig.9 of the submitted manuscript) and Fig.R6 of the present reply are mostly concentrated during summer period (red dots in Fig. R6), when, due to the stratification, the dynamics in the upper layer is decoupled to the surface layer. In those situations the chlorophyll concentration values are low and so is the impact of Δk .

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 obtained gradients.

[5] We considered to introduce the Longhurst model (Lm) in order to produce synthetic schemes of the simulations performed. Among the bioprovinces introduced by Longhurst (1996) for the Global Ocean we found that the Lm 3 was the most representative for the result of the modelling simulations presented; Lm 3 was also associated by Longhurst to the Mediterranean Sea. This does not imply an homogeneous situation, in fact Longhurst classes are qualitative and each one encloses a spectrum of possible quantitative trends. In particular, zooming from the Global Ocean to the Mediterranean Sea, more details are required to characterize each region. In figure R2 we reported the Longhurst diagrams for the regions considered and for the deep

convection area. From the analysis of the Longhurst diagrams the west-east gradient in chlorophyll accumulation (green lines, Fig.R2) is evident. The same trend is reflected in the int-NPP seasonal cycle (red lines, Fig.R2). It is interesting to note that there seems to be no relation between MLD intensity and trophic regime. For example if we consider diagram d) - NWM region -, and diagram g) - Levantine Sea – the MLD is similar but the accumulation and productive graphs are different. To improve the descriptive capability of the Longhurst diagrams we added the depth of a reference phosphates concentration (0.05 mmol P m⁻³, black dotted line) and the grazing on phytoplankton (g C m⁻² y⁻¹, GRZ red dashed line). This shows that in the western regions even a shallower –than in the eastern reaches- MLD has the capability to lift-up waters masses with higher nutrient content. Another interesting aspect appears from the diagrams: the accumulation phase starts when the NPP is still decreasing, this indicates that the triggering process is the reduction in grazing (losses). After this initial triggering phase the full blooming period is fuelled by the nutrient flux from the deeper levels (MLD deepening) and corresponds to a decoupling of production and grazing rates (the first increases, the latter decreases).

This makes the Longhurst diagrams (with the phosphates and grazing informations here introduced), useful to explain the presence of several spatial gradients observed in the simulations. However, the Longhurst classification would need to be revised to account for the peculiarities of the Mediterranean Sea subregions.

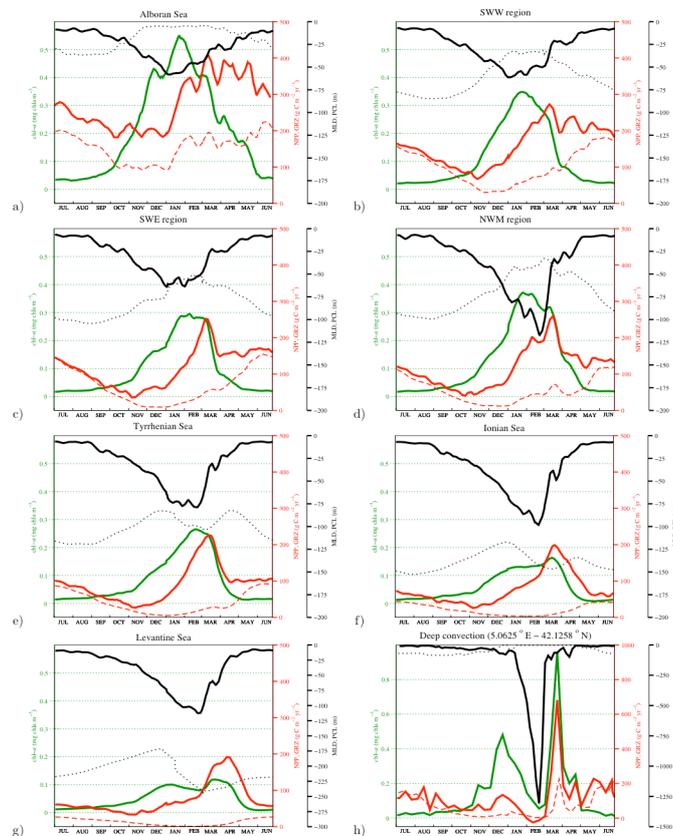


Fig. R2. Longhurst diagrams for the regions considered in the model (a,b,c,d,e,f,g) and the deep convection area (h). Red solid lines are int-NPP (g C m⁻² y⁻¹) and dashed red lines are grazing of phytoplankton (g C m⁻² y⁻¹), green solid lines are surface chlorophyll (mg chl m⁻³), black solid lines are mixed layer depth (m) and black dotted lines are the depth of the 0.05 mmol P m⁻³ isophosphate (m).

Specific comments

Page 4, line 12. Please specify LTER acronym

[6] We indicated the meaning of the acronym (Long Term Ecological Research).

Page 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.

[7] The reply [3] will be included in the revised manuscript and the info about the year simulated (1999-2004) will be included in the text. We added the following descriptive part of the physical model to the manuscript:

The dynamical model is called MED16, and is a regional configuration of the primitive-equation rigid-lid numerical model Ocean PArallel (OPA) [Madec et al., 1997] for the Mediterranean Sea [Béranger et al., 2005]. Its horizontal resolution is $1/16^\circ$ in longitude and $1/16^\circ \cos(l)$ in latitude (l is the latitude) that corresponds to about 6 km, with 43 vertical levels on a stretched grid with layer thickness increasing from 6 m at the surface to 200 m at the bottom. The initial state of the simulation is the climatology MODB4 from Brankart and Brasseur (1999) in the Mediterranean and the climatology of Reynaud et al. (1998) in the Atlantic Ocean. A buffer zone is applied in the Atlantic domain.

The horizontal eddy momentum and tracer diffusivity are parameterized a bi-harmonic operator (coefficient equal to $-3 \cdot 10^9 \text{ m}^4 \text{ s}^{-2}$). The vertical diffusivity for tracers and momentum is modeled with the Turbulent Kinetic Energy closure scheme proposed by Blanke and Delecluse [1993]. In case of vertical static instabilities, the vertical diffusivity is increased to a threshold value of $1 \text{ m}^2 \text{ s}^{-1}$. The mixed layer depth is diagnosed as the depth at which the vertical diffusivity coefficient corresponds to the threshold value taking care of vertical static instability.

Starting from rest, the MED16 model was forced by the reanalyzed fields from ERA40 (Uppala et al. 2005) over the 1989 to February 1998 period, and then by the ECMWF analyses from March 1998 to mid-2006. The period studied here is 1999 to 2004 which is representative of the general circulation and which does not include, in particular, year 2005, known for the Western Mediterranean Transition (Schroeder et al. 2008). Although ECMWF products have the disadvantage to include some changes in the model resolution and physics due to the operational way (Lalauette, 2000), it is an adapted product for the Mediterranean allowing realistic representation of the ocean circulation because of its relative high resolution (Béranger et al. 2010; Sorgente et al. 2011).

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?

[8] We included the following explanation of the satellite algorithm used to calculate the ksat:

The light attenuation term (k) is derived from SeaWiFS data (ksat) adopting the diffuse attenuation coefficient at 490 nm (K490). ksat consists of seasonal climatological measurements over the 1998–2004 period, which were spatially interpolated onto the model grid with a 5-day temporal frequency.

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.

[9] In the first version of the manuscript the chlorophyll data were represented as histograms (Fig.5) and were taken from the article on the DYFAMED station measurements by Marty and Chiavérini (2002), Tab.1. Now we elaborated the DYFAMED data to compare them to the measured MLD and to establish whether MLD and integrated chlorophyll derived by in situ data are congruent with the OPATM-BFM model results. We elaborated the MLD and chlorophyll data published in Marty et al. (2002) considering the seasonal cycle for each year (Fig.R3). To compare models and data, instead of monthly averages we considered monthly medians and inter-quartile ranges, (Fig.R4a, b), those are more appropriate indexes since data are not normally distributed. From the analysis of the in situ data we can summarize that the accumulation process (corresponding to the chlorophyll bloom) has a complex dynamics and a high inter-annual variability. The accumulation of phytoplankton biomass process, evidenced by chlorophyll concentration, starts with the deepening of the MLD, and the accumulation maximum is reached with the maximum deepening of the MLD. This dynamics, clearly evident for the year 1998, is congruent with what was observed by Behrenfeld (2010) for the North Atlantic and with the dynamics reproduced by the model. In the simulations the accumulation process is synchronized with the MLD signal.

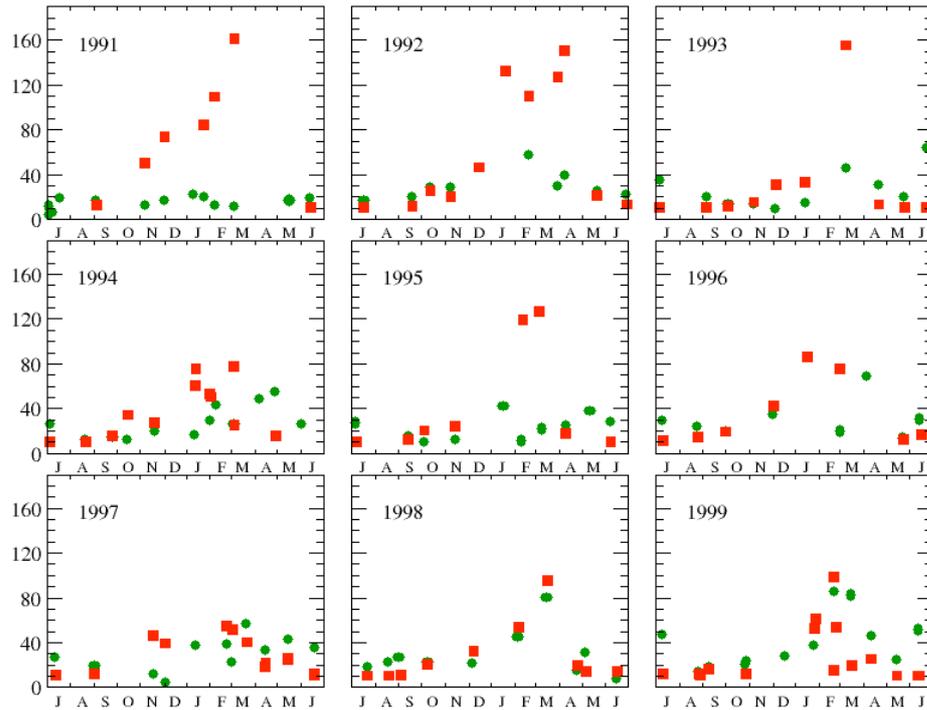


Fig. R3. Integrated chlorophyll (mg chl a m^{-2} , green dots), and mixed layer depth (meters, red squares) from the DYFAMED dataset published by Marty et al. (2002). (Note that for each year the month order is permuted in order to have the winter-spring period located at the center of the temporal axis).

In Fig.R4a chlorophyll data (red boxes), show a marked seasonal cycle with higher median values in January, February, March, April. January, February and March present higher accumulation in model results (black boxes), with February corresponding to the maximum peak. If we restrict our attention to the median, the maximum peak is in February both for the data and the model. The inter-quartile range for March is higher in DYFAMED data than in the model.

The dynamics shown is related with MLD cycle as suggested by Referee #1. The main discrepancy between data and model simulation is related to late bloom period (Fig.R4a). In April the model shows a fast decrease in concentration, whilst the data show higher concentrations. In the same period modeled MLD present lower inter-quartile range values than what observed at DYFAMED station, Fig.R4b.

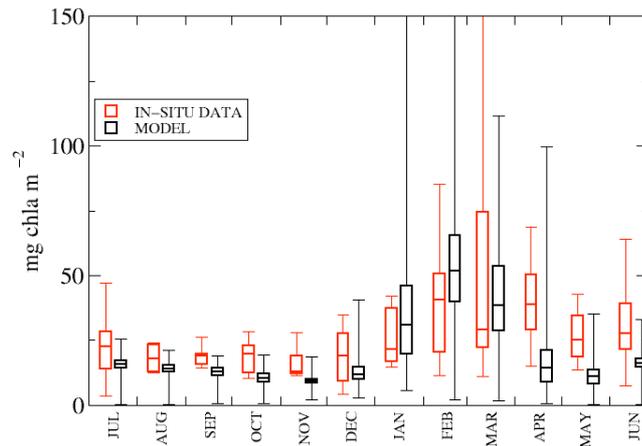


Fig. R4a. Seasonal cycle of integrated chlorophyll derived from DYFAMED dataset (years 1991-1999) presented in Marty et al. (2002), here aggregated by monthly medians. The same analysis is applied to model data 1999-2004 (data are aggregated by spatial-temporal medians on a 1 degree box centered on the DYFAMED station coordinates). Boxes values represents, from lower to higher values, minimum, 25th percentile, median, 75th percentile and maximum.

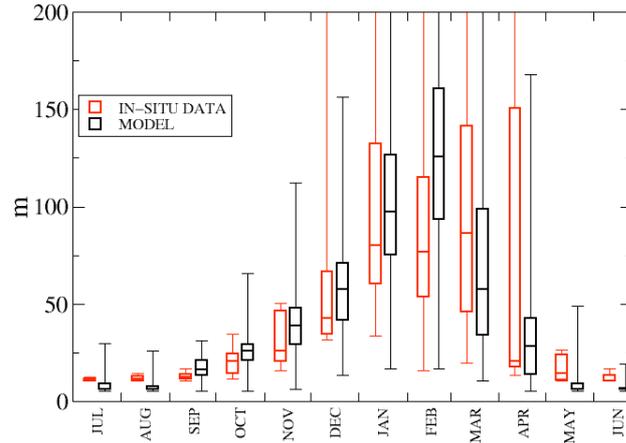


Fig. R4b. Seasonal cycle of MLD derived from DYFAMED dataset (years 1991-1999) presented in Marty et al. (2002), here aggregated by monthly medians. The same analysis is applied to model data 1999-2004 (data are aggregated by spatio temporal medians on a 1 degree box centered on the DYFAMED coordinates). Boxes values represents, from lower to higher values, minimum, 25th percentile, median, 75th percentile and maximum.

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.

The Alboran Sea production is correlated with the circulation patterns present in the area, see Figs.R5a, b, c, d. Vertical velocities enrich the surface Atlantic Waters with nutrients that are subsequently advected horizontally through the gyres present in the area. The principal sites of vertical flux are located in the Gibraltar Strait and along the northern coast of the Alboran Sea.

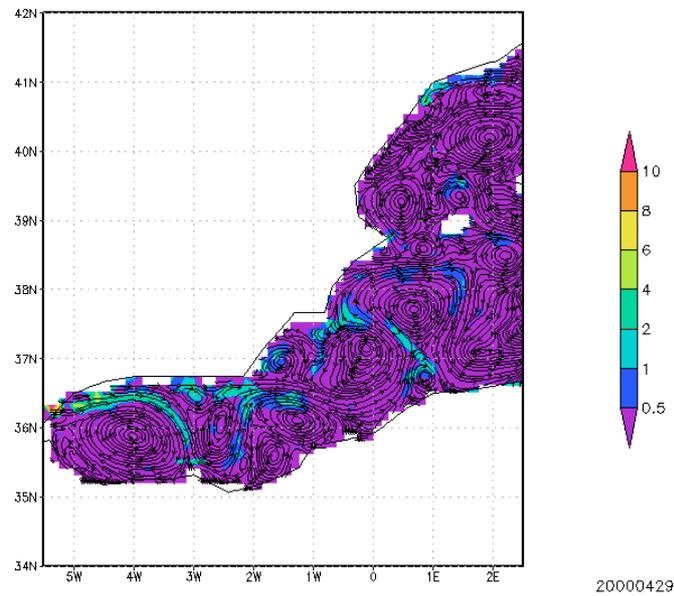


Fig. R5a Horizontal current velocity field (stream lines) and vertical velocity field (shaded, $m d^{-1}$) on the upper model level (end of April).

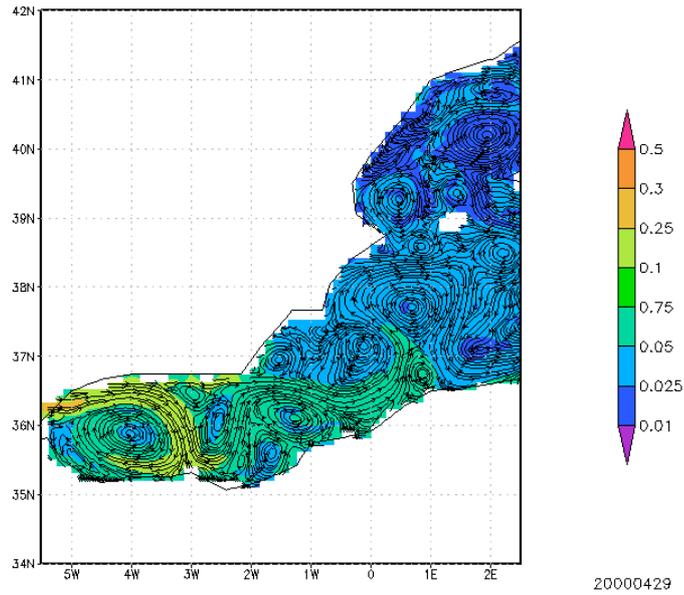


Fig. R5b. Horizontal current velocity field (stream lines) and phosphate concentration (shaded, mmol P m^{-3}) on the upper model level (end of April).

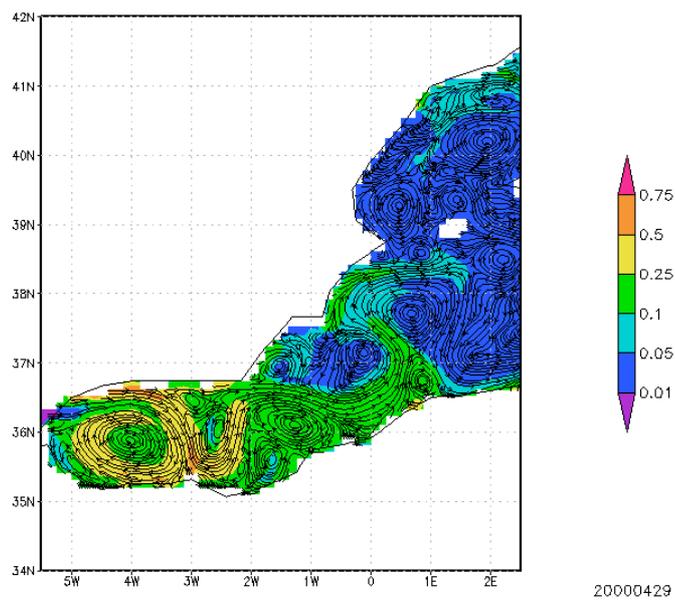


Fig. R5c. Horizontal current velocity field (stream lines) and chlorophyll concentration (shaded, mg chl a m^{-3}) on the upper model level (end of April).

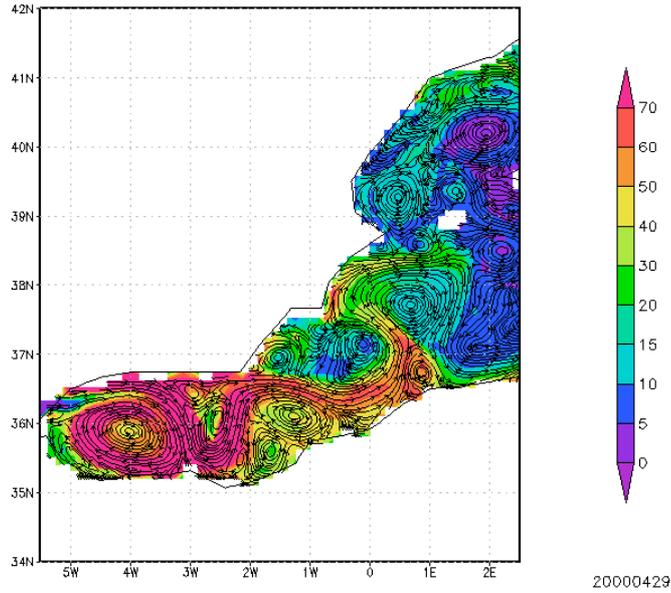


Fig. R5d. Horizontal current velocity field (stream lines) and npp concentration (shaded, $\text{mg C m}^{-3} \text{d}^{-1}$) on the upper model level (end of April).

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??

[10] The result is plotted in Fig.R6. As suggested by the Referee, when considering seasonal distributions additional information emerge. In general the correlation is higher during winter period (January-February-March) and lower during summer period (July-August-September): this can be interpreted considering that during summer the stratification decouples the surface and sub-surface dynamics.

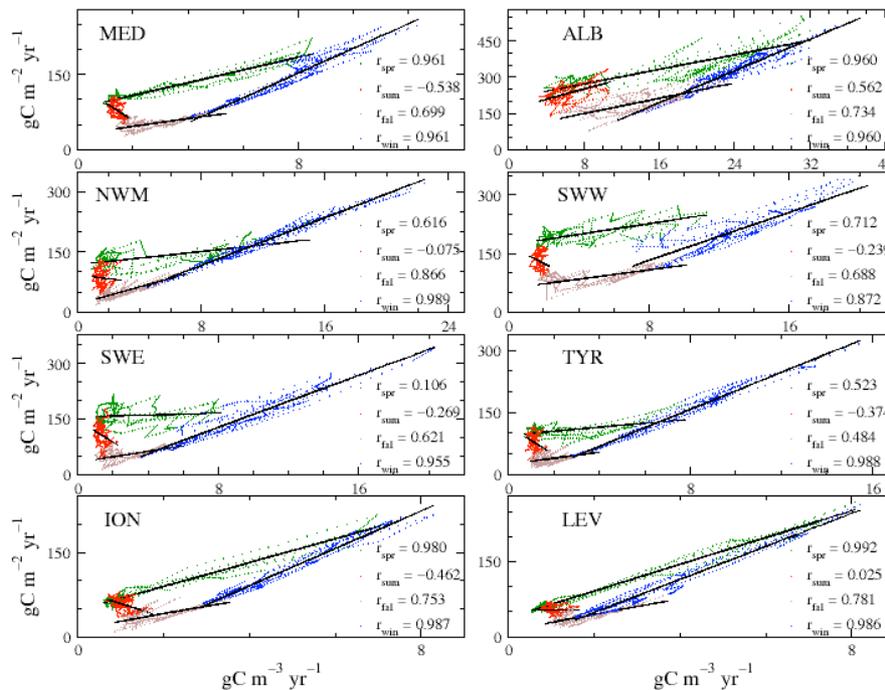


Fig. R6. Scatter plots of integrated ($\text{g C m}^{-2} \text{yr}^{-1}$) versus surface ($\text{g C m}^{-3} \text{yr}^{-1}$) net primary production: each point represents the 10-day regional average in the period 1999-2004. Points are colored according to seasons: winter (blue, January-February-March), spring (green, April-May-June), summer (red, July-August-September) and fall (brown, October-November-December). Black lines represent the linear regressions, whose coefficients are also shown.

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

We added the references for the sub-regions in the transect plots:

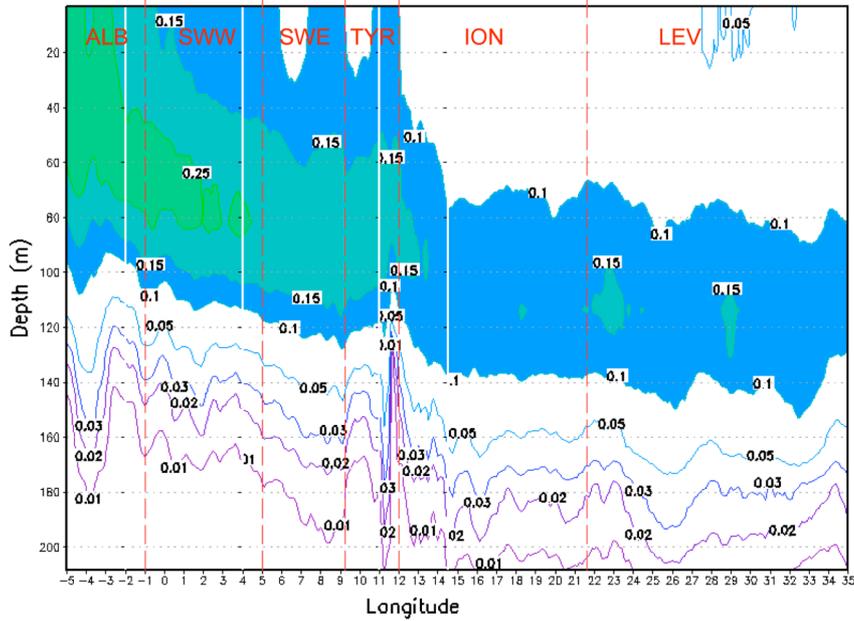


Fig. R7a. Vertical section of average chlorophyll (mg chla m^{-3}) along the T1 zonal transect (see Fig. 1) from OPATM-BFM REF run averaged for the period 1999–2004, crossing lines between transect T1 and regions are indicates by dashed red lines.

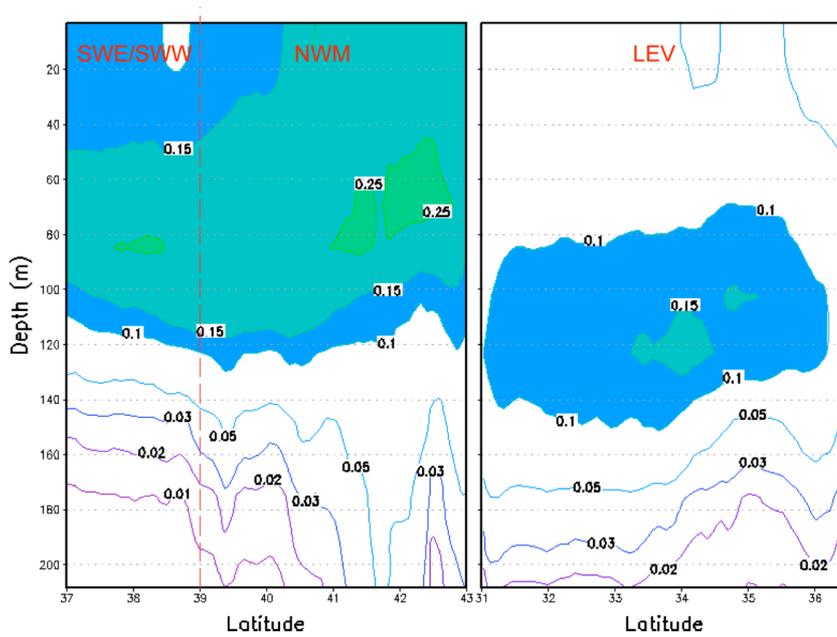


Fig. R7b. Vertical section of average chlorophyll (mg chla m^{-3}) along the T2 (left) and T4 (right) meridional transects (see Fig. 1) from OPATM-BFM REF run averaged for the period 1999–2004, crossing lines between transect T2 and regions are indicates by dashed red lines.

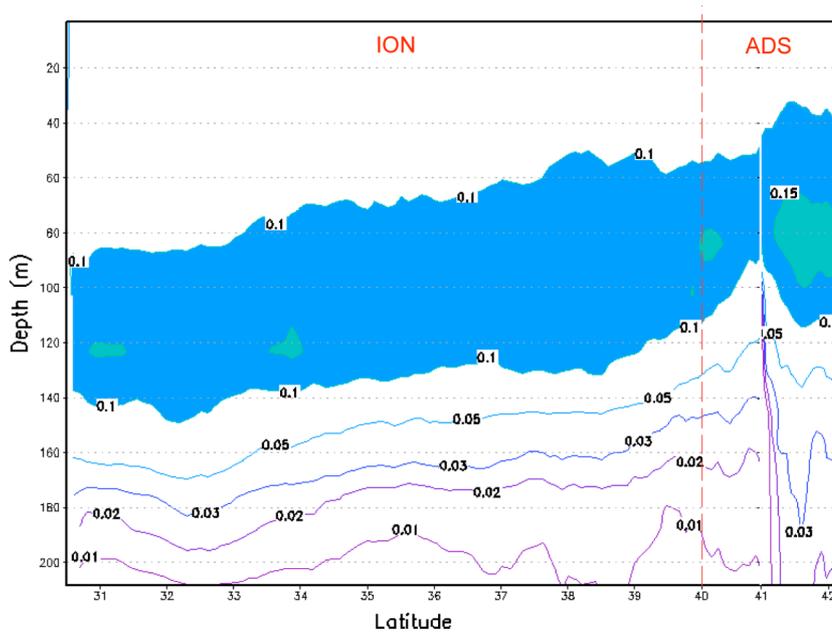


Fig. R7c. Vertical section of average chlorophyll (mg chla m^{-3}) along the T3 (meridional transects (see Fig. 1) from OPATM-BFM REF run averaged for the period 1999–2004, crossing lines between transect T3 and regions are indicated by dashed red lines.

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

[11] We substituted “mooring” with “station”.

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

[12] We agree to modify the plots as suggested by the Referee and to include all the sub-regions, Fig.R8.

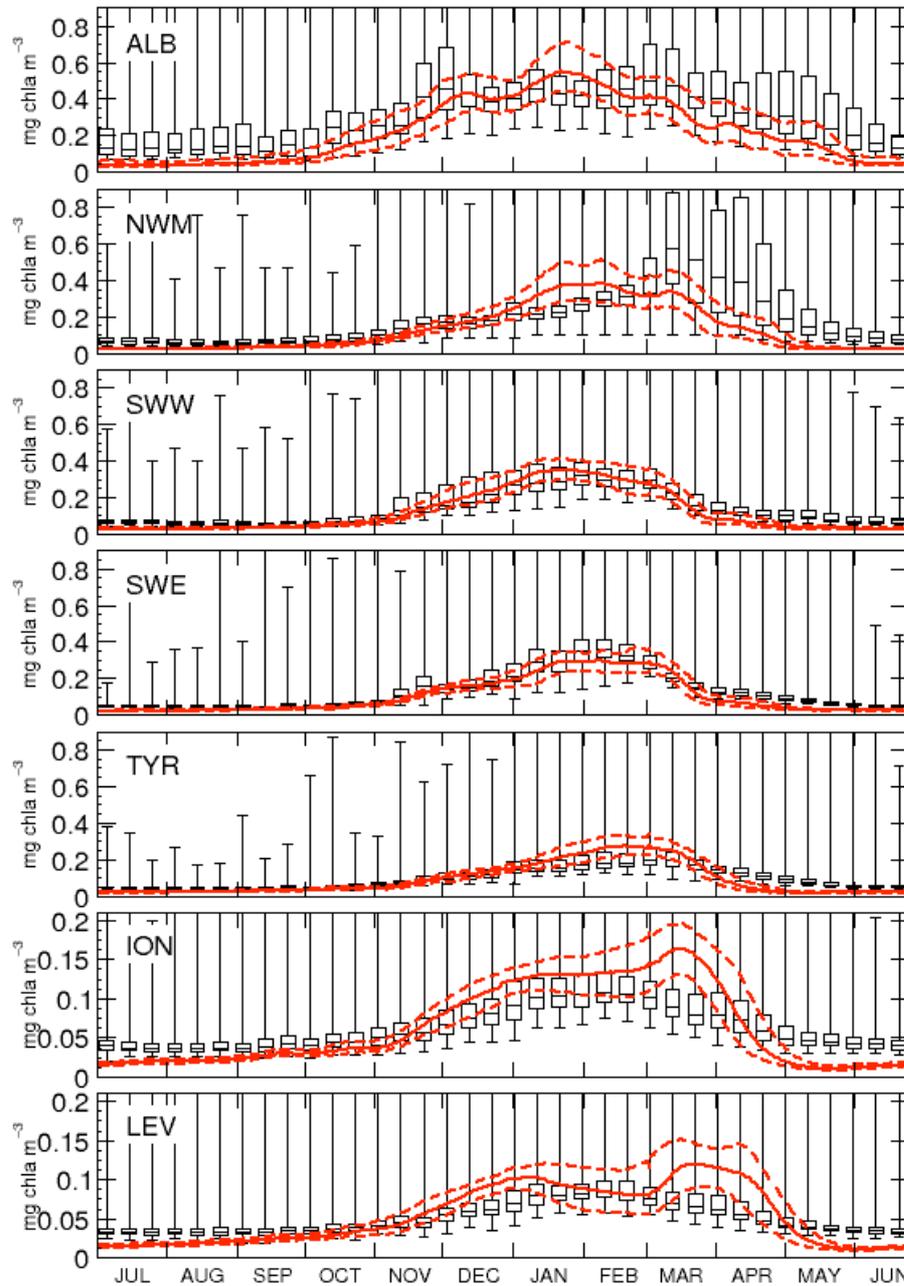


Fig. R8. Surface chlorophyll seasonal cycle (mg chla m^{-3}) for the period 1999–2004 simulated by OPATM-BFM model (solid red lines = median, dashed red lines = 25th and 75th percentile) compared with the satellite SeaWiFS data set (box-plot). Data are spatially aggregated on the regions using a spatial median. Model outputs are corrected with the algorithm presented by Vichi et al. (2007).

REPLY to Referee #2

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 Biogeochemical 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 sub-surface 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 over-looked 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.

[13] The time step of integration is 30 minutes. We will add this information in the revised manuscript.

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.

[14] Referee 2 asked additional tests on the photoacclimation response and its impact on PP of BFM using as a forcing function a diel cycle. We acknowledge the relevance of the different biological responses that vary from slow (genotypic) photoadaptation to faster (fenotypic) photoacclimation to quenching (photoprotective nonradiative energy dissipation) which takes place at scales not larger than seconds.

The Geider et al. (1997) model for photoacclimation is a simplification and a derivation of the Geider et al. (1996) seminal paper where the photosynthesis process was decomposed in three compartments: the Light Harvesting Apparatus (LHA, mimicking the Photosystem I and II of the Light-dependent cycle), the energy storage reserves that contain ATP and NADPH, end products of the LHA, and the biosynthetic apparatus (BiA) in which the whole Calvin cycle is lumped.

Geider et al. recognized the difficulties in finding the proper parametrization in this model and proposed, one year later, a simplified version of the model that was used our version of BFM. Geider et. al. (1997) explicitly recognized that all their formal derivation and experimental results were obtained using 'continuous light'. To accommodate the photoperiod, their proposed simplification was (according to Sakshaug et al., 1989) to determine the max growth rate as proportional to the (normalized) photoperiod. An additional (non-dimensional) term has been then introduced in the growth rate term. This is in line with the findings of Falkowsky and La Roche (1991), who concluded that phytoplankton does not acclimatate to shade at night. From the above considerations the Geider model has been tested and proposed only to respond to the day-by-day photoperiod-averaged light variability.

Moreover, the whole BFM model (which has its roots in the ERSEM model) was calibrated using constant light in the experiments, thus implicitly suggesting to maintain this approach further in order to obtain the best results available with the present model machinery.

As final consideration if we consider the Geider formulation as it is presented in Geider et al., 1997 - that is embedded in the OPATM-BFM implementation - we can evaluate the time scale of adaptation. The Geider model is essentially expressed by the following two ordinary differential equations regulating chlorophyll and carbon synthesis:

$$\begin{aligned}\frac{dChl}{dt} &= \rho_{Chl} P^C C - R^{Chl} Chl \\ \frac{dC}{dt} &= P^C C - RC\end{aligned}\quad (1)$$

where Chl and C are chlorophyll and carbon concentrations, P^C is the rate of carbon synthesis per unit of carbon, ρ_{Chl} is the rate of chlorophyll synthesis per unit of carbon synthesis.

While Geider paper focuses on the balanced growth conditions (derivative of C and Chl equal to 0), here we show that it is possible to express how dynamically the model reaches its equilibrium.

If we define θ as the chlorophyll to carbon ratio and impose $R^{Chl} = R$ (as it is done in the Geider's paper) we obtain the following relation for θ :

$$\frac{d\theta}{dt} = P^C (\rho_{Chl} - \theta) \quad (2)$$

This relation evidences that the system reaches, as found by Geider, the following equilibrium (if we consider $P^C \neq 0$):

$$\theta = \rho_{Chl} \quad (3)$$

Moreover Eq(2) expresses also the typical time scale of adjustment to reach the equilibrium that is P^C . The latter is of the order of d^{-1} , thus justifying our choice.

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.

[15] As suggested by Referee #2 we recomputed the chlorophyll as seen from satellite using the approach described in Vichi et al., 2007. We found that the main differences are in the region/period with higher biomass (see Fig.R9).

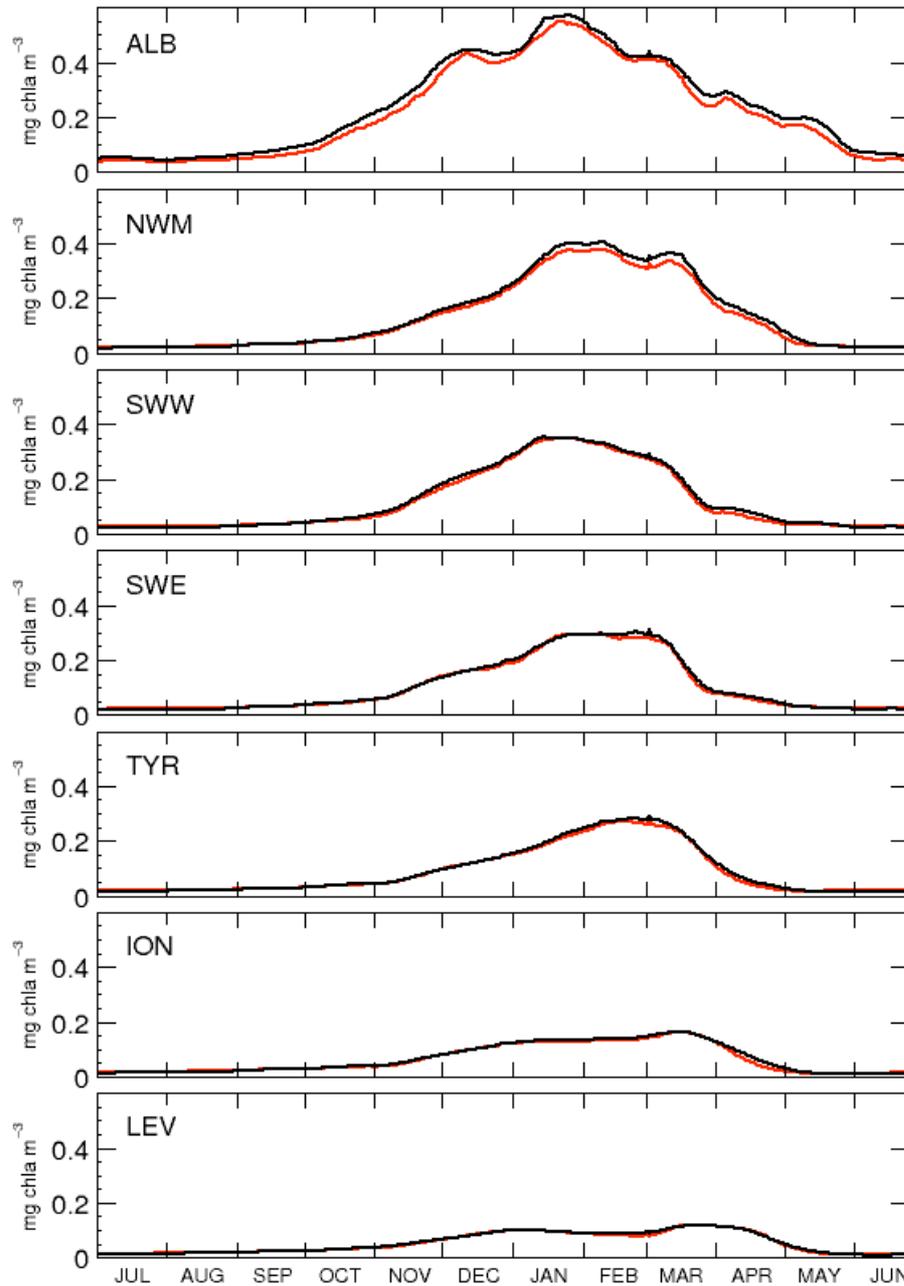


Fig.R9. Chlorophyll from the upper model cell (black line) and corrected (red line) to be compared with satellite observations, using the procedure reported in Vichi et al. (2007).

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 ?

[16] The presence of a higher variability can be explained in terms of the extended Longhurst diagrams considering also the vertical distributions of nutrients. In the western area the nutricline is shallower and the variability of the vertical fluxes can produce easily an entrainment of nutrient in the upper layers.

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?

[17] Longhurst diagrams indicate that during summer and autumn the productivity is controlled by grazing, indeed the initial start of the accumulation phase is triggered by a reduction in losses. The full bloom is fuelled

by up-lift of nutrients.

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.

[18] We described how the dynamics produced by the model behaves in this area in point [9] of reply to Referee #1

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?

[19] We are not sure we followed the line of reasoning of Referee #2, in any case we propose the following answer.

We considered particulate concentration at 200 m depth. The carbon component presents an evident west-east gradient (Fig.R10a), as a signature of different trophic regimes: mesotrophic in the western reaches and oligotrophic in the eastern reaches.

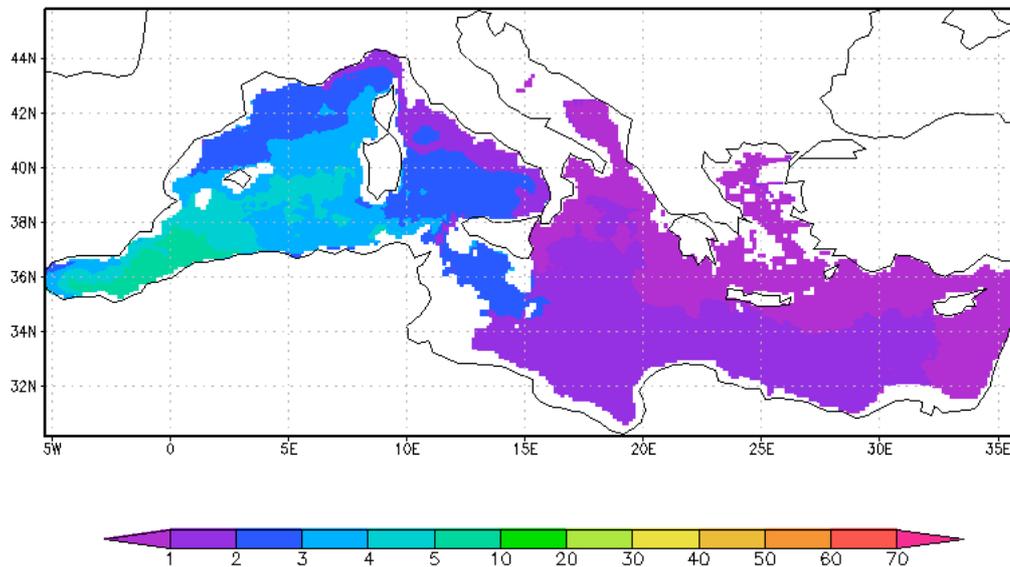


Fig. R10a. Averaged (period 1999-2004) particulate carbon content at 200 m depth (mgC m^{-3}).

The corresponding map for the N:P ratio in particulate (Fig.R10b), presents a west-east gradient as well. Western regions present a N:P lower or equal to the Redfield ratio, with the exception of the NWM area. Moving easterward the particulate presents N:P ratios higher than Redfieldian ones. If we consider the system with (Fig.R10b) and without ATI (Fig.R10c) the particulate presents a tendency to accumulate more nitrogen than phosphorus. This effect could be studied by performing long-term simulations (~100 years, Crispi et al. 2001) in order to establish which is the impact on nutrient distribution along the water column.

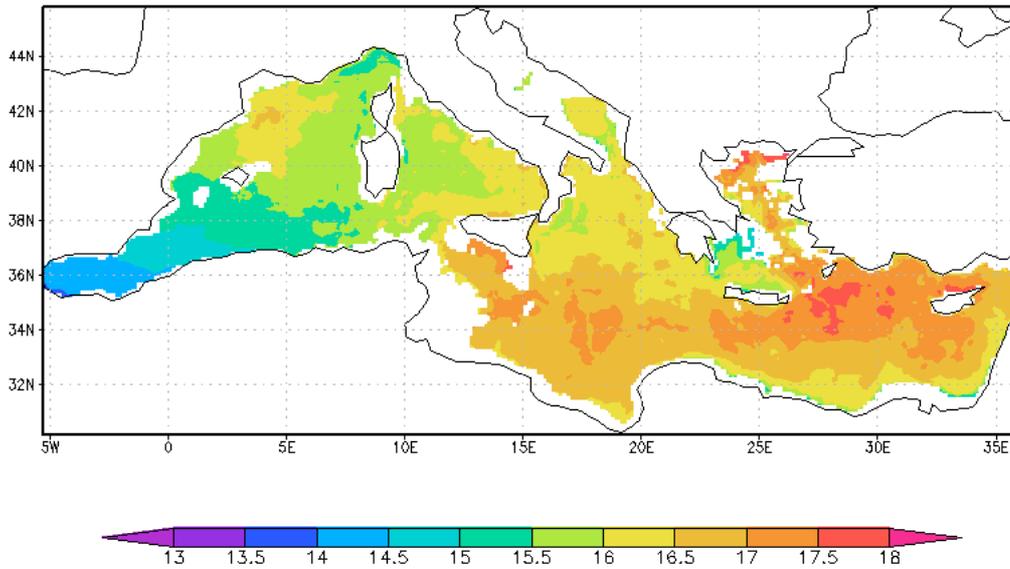


Fig. R10b. Average (period 1999-2004) particulate nitrogen/phosphorus ratio content at 200 m depth, mmol N/ mmol P.

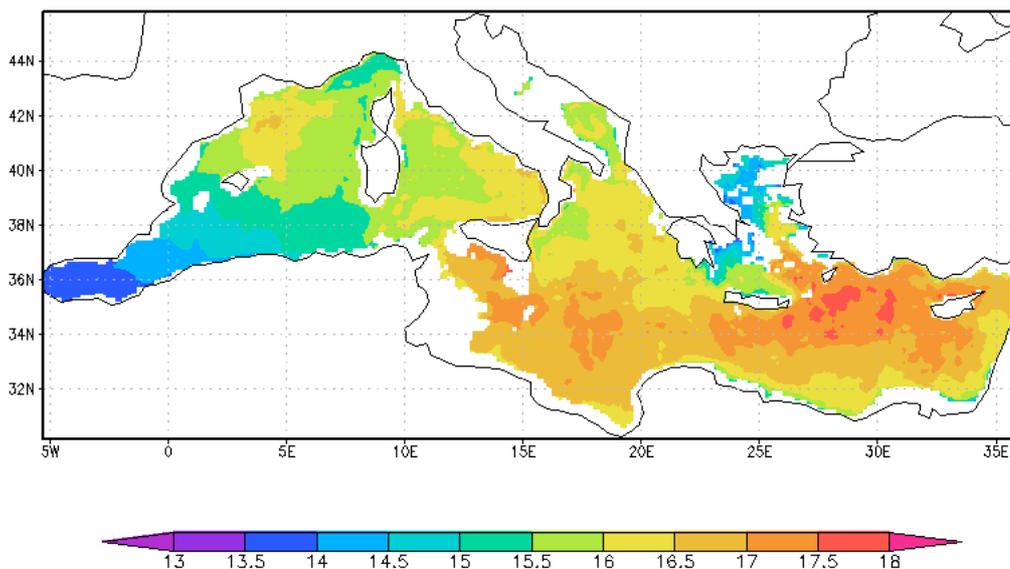


Fig.R10c. Average (period 1999-2004) particulate nitrogen/phosphorus ratio content at 200 m depth, mmol N/ mmol P. Simulation without ATI.

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.

[20] We already replied to Referee #1 about this issue (see point [9]). Our results are not in contradiction with what found by Behrenfeld who, referring to the North Atlantic, wrote in the conclusions (pag. 986):

“The current analysis also shows that the positive net growth phase in the North Atlantic begins prior to an increase in light and generally occurs while mixing depths are still increasing.”

Given that positive net growth phase coincides with accumulation phase, the above is congruent with what we found through the OPATM-BFM model and with what emerges from the in situ measurements considering our

analysis of DYFAMED station data set.

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.

[21] Following the suggestions of Referee #1 we modified the scatter plot considering each season separately. The new plot (Fig.R6) indicates that the annual cyclical behavior can be divided in four clusters, one for each season. The higher decoupling takes place during the summer period, when we observe that the surface variability is lower than the variability of the integrated NPP, for all the sub-regions considered.

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'? P.2 L.3 'role of external fluxes and light penetration' instead of 'the role of ecosystem boundary conditions'?

We agree to apply the Referee corrections.

P.3 L.23 'features' instead of 'consideration'? P.3 L.24 'system instead of 'picture'? P.6LL.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'?

We agree to apply the Referee corrections.

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?

We agree to apply the corrections suggested by Referee#2. We considered the value present outside Gibraltar from the climatolgy file belonging to the MEDAR-MEDATLAS data set.

P.8 L.18 'estimates' instead of 'estimations' ? P.11 LL.8-9 was the integral really computed down to sea floor? Why?

We agree to apply the Referee corrections; the integral is computed down to sea floor to account for the productivity along the whole water column. The results are the same computing the integral over the productive layer because beneath it the productivity decrease to negligible values. We integrated down to the bottom because it is simpler from a technical point of view.

P.14 L.15 I would rephrase as 'could be classified as a . . . system' It is a classification not a paradigm.

We agree to correct the text as suggested.

Supplement

The definitions of symbolism in the opening paragraphs are confusing. Eq. 1 formulates 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 par ts $R_{sup(2)}_{sub(C)}$ 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?

We agree to reformulate the supplementary material with a more concise but equally informative formulation. Traditionally the index of non-living organic compounds runs from 1 to 7, to allow the inclusion of a spectra of components with different size. In the current implementation of the model the indexes are 1-labile, 2-semilabile, 6-particulate, 7-refractory.

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