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Biogeosciences Discussions

Interactive comment on "Chlorophyll signatures and nutrient cycles in the Mediterranean Sea: a model sensitivity study to nitrogen and phosphorus atmospheric inputs" *by* M. Pacciaroni and G. Crispi

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Author comments to Referee #1 Specific comments regarding Biogeosciences Discussions, 4, 909-959, 2007: "Chlorophyll signatures and nutrient cycles in the Mediterranean Sea: a model sensitivity study to nitrogen and phosphorus atmospheric inputs" by M. Pacciaroni and G. Crispi. We report after all R1 the Referee's comments, and ours after AC; the revised text is reported between asterisks.

Specific comments

R1-P. 910, Lines 16-17: "cycling at low nutrient sill" this statement doesn't make sense.-

AC-The revised text is: *This numerical sensitivity analysis suggests that the Eastern Mediterranean, considered as an oligotrophic environment, is eventually pushed toward an higher nutrient depletion, when loading new nitrogen and phosphorus.*-

R1-P. 911, Lines 2-5: Sentence is very awkward and difficult to understand.-

AC-The text in the revised introduction is rephrased in the following way: *The inverse estuarine circulation that characterize the Mediterranean Sea explains the nutrient depletion in nitrogen and phosphorus of the deeper layers. That is not the case for the oligotrophic status in the euphotic zone.*-

R1-P. 911, Line 12: Should add citations for experimental studies.-

AC-Citations are now added for giving experimental results of the Mediterranean biochemistry in different regions of the basin.-

R1-P. 911, Line 27: "The aim of this paper is to clear in which way" replace the word "clear" with the word "clarify".-

AC-This revised part of the introduction is:*The aim of this work is to clarify in which way nutrient availability influences the structure and functioning of the euphotic food web and the export of matter.* It introduces briefly aim, data and methods of the work after having introduced recent other works and results in similar fields.-

R1-P. 911, Line 29: "The importance of external loads cannot easily be underestimated" This is an odd statement. Its not clear what the authors are trying to convey here-

AC-The revised text is: *The external load quantification represents obviously an important estimation because of the induced nutrient variability, the enhanced productivity and the impact on the nutrient recycling; on the same time it results very difficult to represent detailed regional data all over the Mediterranean.*-

R1-P. 912, Line 13: "Biogeochemical setups descriptions of different" this doesn't make sense.- AC-The revised text is: *Different biogeochemical descriptions have been proposed in terms of:*-

R1-P. 912, Line 21: Replace "raise" with "rise".- AC-Thanks for this and the other hints. New text is:*This choice gives rise to systems*-

R1-P. 912, Lines 26-29: Very awkward phrasing.- AC-The revised text now is: *ECHYM model is applied in virtue of its ability in resolving the conveyor belt, from one side, and its interplay among the different nutrient limitations and the top-down control, from the other.*-

R1-P. 913, Line 21: What are to ramifications of using the "rigid-lid" approximation?-

AC-Tides and other waves are filtered out with the rigid-lid approximation; likewise the zonal fluxes among basins results well represented and these are determining for the overall dynamics and the oligotrophy, as described in introduction. Our interpretation of the discrepancies in the Sicily Channel and in the Ionian Sea, as reported in the revised version, could be bound to underestimation of the evaporation effects in the Eastern Mediterranean. The physical sub-model tries to recover the correct situation imposing salinity surface relaxation and therefore salt fluxes at surface. We believe that this problem could be important for the eastern basin, and suggest, aside the high frequency forcing proposed by Korres, Pinardi and Lascaratos (2000), also releasing this surface constraint possibly with sensitivity experiments to different precipitation values.-

R1-P. 915, Lines 7 and 9: Need to define "MAW" and "LIW".-

AC-We have changed the text into: *a) the presence of a non-returning flow of the low-salinity Modified Atlantic Water (MAW) from Gibraltar to the eastern end of the Levantine in the upper 150-200 m; b) the formation and westward spreading of a kind of Levantine Intermediate Water (LIW) at intermediate depth (200-400 m) from the formation region in northwest Levantine and South Aegean Seas to the Gibraltar Strait,

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where it enters the Atlantic Ocean;*-

R1-P. 916, Lines 8 and 9: "Phytoplankton and zooplankton instabilities are treated via borrowing: All biological sources are set to zero and the calculation proceeds after appropriate excretion" Some additional clarification is needed here. I am not familiar with this procedure. Is it a method to damp biological instabilities in the model?-

AC- To control the instabilities due to negative phytoplankton and zooplankton concentrations, we redefine the borrowing with the following new text: *On the other hand, phytoplankton and zooplankton instabilities are treated via borrowing, forbidding negative biomass concentrations at every stage of the simulation. The phytoplankton internal phosphorus to carbon ratio, R_PC, is higher than the zooplankton one, r_PC, and thus part of the negative biomass, del(C), is assigned to the zooplankton compartment, Z, and the residual part becomes carbonaceous detritus, D_C, accordingly to the following expressions:

del(Z)=del(C)*(R_PC/r_PC)

del(D_C)=del(C)*(1-R_PC/r_PC)

At the same time, phytoplankton internal phosphorus to nitrogen ratio, R_PC/R_NC, is higher than the zooplankton one, r_PC/r_NC, and thus the residual part is converted into ammonia:

del(A)=del(C)*R_NC*(1-(R_PC*r_NC)/(R_NC*r_PC))

Finally all the biological sources are set to zero and the calculation proceeds, after transforming as well zooplankton instabilities into the three forms of detritus:

del(D_P)=del(Z)*r_PC

del(D_N)=del(Z)*r_NC

del(D_C)=del(Z) *-

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R1-P. 917, Lines 15-19: Using multiplicative terms for N and P limitation is, perhaps, not the most correct way to express the interaction between these limiting nutrients. Taking the minimum of one or the other is probably more biologically correct. Also, it looks like the ammonia inhibition formulation used here follows Wroblewski's early formulation (Wroblewski 1977) which has some odd characteristics. Alternative formulations have been put forward that are, perhaps, more biologically correct and realistic. See, for example, Frost and Franzen (1992).-

AC-The mutiplicative expression of the nutrient limitation is chosen according to Chen and Orlob (1975, Systems Analysis and Simulation in Ecology edited by B. C. Patten, Vol. 3, 475-588). We do not take the minimum between the nitrogen and phosphorus limitations because the internal N:P ratios are fixed inside both compartments of autotrophs, S and L; the minimum formulation of the growth requires the introduction of the cell nutrients as independent variables (Cloern, 1978, Ecological Modelling, 4, 133-149). The other point depends on the generic nitrogen limitation after Fasham, Duklow and McKelvie (1990). We understand that there are different ammonia-nitrate limitations; in our opinion the differences should result small because of the low values of the limiting factor ammonia; in every case it is argument of a sensitivity study using different formulations and we note that the chosen nitrogen limitation is used by other recent three-dimensional modelling studies in Pacific Ocean (Jiang, Chai, Dugdale, Wilkerson, Peng and Barber, 2003).-

R1-P. 923, Line 3: A chla:C ratio of .0073 is very low, perhaps unrealistically so. See ranges in (Parsons et al. 1984).-

AC-The measurements of the Chl:C ratio show high variability in different marine ecosystems. In the Eastern Mediterranean the cells experience both nutrient limitation, with the net effect of decreasing Chl:C, and availability of the light during longer period and at deeper depths, as well with a decreasing of the Chl:C ratio. The cooperation of these two factors in the nutrient impoverished Eastern Mediterranean gives ratios clearly lower than in the western basin. This environmental behaviour is confirmed by measurements in phytoplankton cultures (see Table 1 in: Falkowski, Dubinsky and Wyman, 1985, Limnology and Oceanography, 30, 311-321) with the lowest values approximately 0.003, when low nutrient and high PAR is present. Our value of 0.0067 (0.0073, value reported in the previous text, is a mistake; the used value is 0.0067, as in our Table 4) is representative of measures in the Aegean Sea (Vidussi, Claustre, Manca, Luchetta and Marty, 2001; Siokou-Frangou, Bianchi, Christaki, Christou, Giannakourou, Gotsis, Ignatiades, Pagou, Pitta, Psarra, Souvermezoglou, Van Wambeke and Zervakis, 2002) and it is intermediate between the higher Chl:C ratios in the Western Mediterranean and the very low ones present in more oligotrophic regions.-

R1-P. 923, Lines 24-26: Need to state parenthetically here that seasonal patterns are not shown. Better yet, if the correlations are good why not show some aspects of the seasonality and comparisons between the model and the observations?-

AC-We analysed the GARUN monthly chlorophyll data in the presentation at the final ADIOS meeting. The surficial chlorophyll study was preliminary, however it represented a first step toward seasonality; in order to present our final results, we decided for annual mean values. Future comparisons with in field data could be also considered from a seasonal point of view. The new text introduced in the revised version is: *Monthly surficial chlorophyll maps (not shown) reveals the highest signal during December and January in the western basin, while in the eastern one maxima appear with one month delay. In spring and summer, this signals pregressively disappear all over the Mediterranean, with exception of the Alboran Sea and some coastal areas in the Ligurin Basin. In the autumn months, the conditions for new biomass growth reestablish. The dynamic explanation of this seasonal evolution resides in the mixing processes that supply new nutrients to the upper layers during the end of autumn. This process fovours the phytoplankton growth in the early winter, observed as surficial chlorophyll maxima. After this, there is chlorophyll maximum deepening, because of the beginning stratification, and the start of the secondary production. Later, the mortality and lysis of the living matter produce net organic matter fluxes toward the intermediate depths. Remineral-

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ization of the organic matter into nutrients fertilizes these intermediate layers, giving the necessary preconditioning to the repeating cycle.*-

R1-P. 924, Lines 5-10: Some reference back to observed ratios is needed here. How well do these modeled ratios/maps agree with observations?-

AC-We cannot estimate from data surficial ratios for different species, because carbon phytoplankton measures are scarce, or in the presented papers in the western and in the Eastern Mediterranean they are vertically integrated (Nival, Nival and Thiriot, 1975; Vidussi, Claustre, Manca, Luchetta and Marty, 2001). These annual average of the Chl:C ratios, Fig. 3 and the averaged monthly ratios, remain at this moment independent outcomes of the model. An indirect confirmation of these results is bound to the validation of the surficial chlorophyll maps of Figg. 2 and 4, results determined mainly by the dominance of the netplankton (diatoms) in the western basin and by the ultraplankton in the eastern basin.-

R1-P. 924, Line 11 - onward, Figure 4: The chlorophyll patterns in Figure 4 are hard to see and it is difficult to compare the maps because they are small and the color scheme for contouring does not differentiate the low concentration regions very well. Perhaps plots could be made larger and a different color contour scheme employed.-

AC-The color bars in the figure 4 are now more readable, besides the figure is made larger. Areas with low chlorophyll concentration are differentiated from the higher adjacent ones, the end scale in the color bar is reduced from 1.2 to 1-

R1-P. 924, Line 19 - onward: Many of the regions that are referred to here (e.g., the Balearic basin) are not familiar locations. Perhaps some labels could be overlaid on these maps to orient the reader.-

AC- New labels have been inserted in figure 4c, the main regions named in the text can now be individuated in figure 4c together with the sampling stations.-

R1-P. 925, Lines 12-25: Given the sparseness of the observations in both time and space, perhaps it would be more meaningful to make this comparison on a point-to-point basis, i.e., pick comparable values in time and space from the modeled fields that correspond to the direct observations and compare them directly in an X-Y plot.-

AC-We agree, it is possible to realise a point-to-point comparison. However this approch can be followed in the season in which the data were acquired. We decided to have in this work a repeting-year approch, while a point-to-point comparison should be performed, in our opinion, after applying the specific forcings relative to the cruise year.-

R1-P. 926, Line 9, Table 5: Why not include some statistics in Table 5 (e.g., 95% confidence intervals) to give the reader some sense of the statistical differences in these average chlorophyll values.-

AC-A statistical analysis on the last 36 months of each run determine the averages and 95% confidence intervals of the new Table 5. The statistical intervals of the three sensitivity run do not overlap for the eastern averages and only a little for GARUN and AVRUN in the western basin.-

R1-P. 927, Lines 10-21: Figure 6d reveals glaring discrepancies between the modeled and observed vertical distributions on the western side of the transect. This is suggestive of some problems with the physics, i.e., perhaps the pycnocline depth is not properly represented across this region of the basin. But no physical fields are shown. The authors state that the data coverage is poor in this region as an explanation, suggesting that the model is actually more correct than the plot comparison implies. Perhaps then the observational data density should be overlaid on the VIMS section to give the reader a sense of the validity of the comparison.-

AC- Concerning the disagreement in the western side of the zonal transect reported in Fig. 7, a potential density comparison between model and data profiles, see new figure 9, has been carried out considering the five regions, that contain this eastern zonal transect, and comparing model's results with MEDAR MEDATLAS II estimates.

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The pycnocline positions are also shown calculating the density differences along the vertical coordinate. The model underestimates systematically, in this area, the experimental density; this happens also in temperature and salinity profiles, not reported in the revised text. However the trends as a whole are in good accord with the exception of the surficial zone where the model does not recognize the double layer of the data. Physical explanation of the biochemical response in the Ionian Sea could be that the model does not reproduce the correct vertical movements due to net effect of the evaporation, precipitation and river inputs in the Ionian Sea; consequently the nutrient downwelling and phytoplankton cycle result not properly developed in that area.-

R1-P. 928, Lines 16-19: Here to the comparison with observations reveals glaring discrepancies between the modeled and observed chlorophyll sections. Again, this implies that there is something wrong with the physical model, i.e., pycnocline (and nutricline) depth substantially too deep. The authors suggest that this is at least partly due to summer bias in the VIMS data. If this is the case, then why not make a more meaningful comparison by showing summertime fields from the model for comparison?-

AC-We considered also the averages of the model's chlorophyll in summer. Anyway considering a 2x2 degrees in the centre of the west meridional transect, the deepening of the DCM is not very large, reaching about 40 m. The point regarding the dynamics of the central part of the transect, aside Sardinia, remain in our opinion open to the analysis of the forcings and of the mesoscale dynamics.-

R1-P. 929, Lines 2-12: The differences between the modeled and observed integrated chlorophyll patterns revealed by Figure 9 are glaring. Why does the model have so much more spatial variability? Again, this suggests that the physical forcing in the model is very different than reality. Alternatively, is this related to the fact that the observations are biased toward summer?-

AC-The general trend with lower chlorophyll in the southern and central parts of the transect, respectively Algerian Sea and Algero-Provencal Basin, are confirmed by data averages in the two areas: about 35 mg Chl/mˆ2. Otherwise the model's higher vertical content in the northern area, in the Gulf of Lions, is confirmed from MEDAR raw data average, giving 45 mg Chl/mˆ2. It appears a tendency of the VIM, which is an interpolating model at one sixth of degree, to smooth the input chlorophyll data. The results regarding the smaller scale in our model remain to be confirmed; anyway this model is not eddy-permitting, thus the model should not in principle resolve mesoscale dynamics.-

R1-P. 930, Lines 2-26: Why aren't any direct comparisons between modeled and observed DIN and DIP fields shown? This is a first order comparison that can and should be made. Surely there must be nutrient data available for the Mediterranean. Are the modeled concentrations approximately correct at the surface? And at depth? Is the nutricline in he right place? If horizontal spatial maps cannot be constructed, then still, some comparisons with vertical sections would be very illuminating. Based upon the previous comparisons of vertical sections of chlorophyll concentration, it is probably a good bet that there are some substantial discrepancies between the observed and modeled nutricline depths.-

AC-The model to nutrient data comparison is really important and we decided to insert two new figures based on vertical profiles in correspondence of five regions in the eastern and three ones in the western basin; these regions obviously include the transects discussed in the paper. As described in the text some important aspects emerges from these new figures however the model and data are generally in good accord.-

R1-P. 931, Lines 1-2: Is it not possible, and perhaps more interesting, to generate regional plots of the seasonal cycle for different subregions? It is really only useful to average over the entire basin if the seasonal cycles are basically similar everywhere. I doubt this is the case.-

AC-Monthly chlorophyll maps and vertical sections were presented in the ADIOS ppt file, for giving the seasonal evolution of the chlorophyll. This final presentation is at the base of this work, in which we investigate mean subbasin effects of the atmospheric deposition.-

R1-P. 931, Lines 13-23: Some explanation of the seasonal cycle is needed here, i.e., why are the highest values generated in winter/spring? What are the dynamics of the seasonal cycle? Are they everywhere similar in the Mediterranean Sea? And are there no data available to validate these model-generated seasonal cycles?-

AC-The highest chlorophyll signals appear as a phytoplankton response to the new nutrients upward due to mixing and deep convection movements, these intense dynamics occur in the early winter period. For what concern the dynamic explanation of this seasonal evolution, a new text is reported in the revised version: *The dynamic explanation of this seasonal evolution resides in the mixing processes that supply new nutrients to the upper layers during the end of autumn. This process favours the phytoplankton growth in the early winter period, observed as surficial chlorophyll maxima. After this, there is chlorophyll maximum deepening, because of the beginning stratification, and the start of the secondary production. Later, mortality and lysis of the leaving matter produce net organic matter fluxes toward the intermediate depths. Remineralization of the organic matter into nutrients fertilizes these intermediate layers, giving the necessary preconditioning to the repeating cycle.* The seasonal variability at surface, summarized above, is in the ppt file, containing also the monthly variability in the western and eastern transects. The presentation is public, so everyone can have an idea of the seasonal variabilities. We say some words here, for not burdening the text. In the western basin there is beginning of the winter bloom toward the northern Gulf of Lions; after that while it is reaching the maximum, there is some chlorophyll development in the central part of the basin; and after that it follows the southern part of the transect, Algerian. At the end of the year there are new conditions for the production due to preconditioning of abundant nutrients from deeper layers. The situation of the eastern basin is that the Levantine is characterized by DCM nearly constant during the year with higher values during spring; this 'regenerated cycle' is contrasted by some-

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thing more similar to blooming in the western side, that is formed at about 75 m and after then it is displaced upward and after that broken. About validation there is need of more data, because they show higher variability, and moreover we think that only a model taking into account high frequency forcing of that specific season of the data could be a reasonable study of the situation.-

R1-P. 933, Lines 19-20: It is stated here that the modeled primary production in the upper 180 meters is in keeping with the field measurements and other biooptical estimates. But no observations of primary production or biooptical measurements are presented in this paper and compared with the model. This statement should be dropped.-

AC-In the revised text primary production comparisons with field data averages and with biooptical estimations are introduced in paragraph 3.5. There is a general accord with the model's results; with overestimation of the biooptical model or, seen in another way, with some underestimation of the 3D model. The secondary productions are spot and sometimes not given in terms of carbon. We prefer keep open this question till the acquisition of new data; also here the model's results give independent values to be controlled by future integrated estimates.-

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