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Biogeosciences Discussions

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Interactive Comment

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 #4 in 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. The manuscript has been revised taking into account all the general and specific comments by Referee #4. Our comments follow, after AC, Referee #4 ones, beginning with R4. The comments, when quoted between asterisks, are introduced directly in the revised text.

General comments

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R4-This paper presents results from a high-resolution coupled bio-physical model of the Mediterranean Sea, where a range of atmospheric nutrient loadings of N and P have been applied. The authors then seek to distinguish how variation of these loadings impacts the modeled ecosystem. There are some very basic flaws with this report that are described below in the general comments that I have developed. Without addressing these basic needs, some or all of which may be solely a matter of including additional detail that has been omitted and improving how the information is presented, it is quite difficult to make an assessment of the results that are discussed. At this juncture, I cannot recommend publication of this manuscript. However, investigating how aeolian mineral deposition affects oceanic ecological variability is certainly of high topical interest, and I would urge the authors to work to improve this contribution as it has good potential to contribute to our evolving understanding of atmospheric-oceanic linkages.-

AC-The work is revised along three main routes. Some climatological papers are introduced in the revised version for addressing the long-term coupling between the physics and the biogeochemistry. The links between the atmospheric deposition sensitivity analysis and the general objectives of the work are made more precise; the methods are detailed for improving the information about the coupled sub-models. The impact of the physics on the chemical variables is introduced. The average values of the experimental nitrate and phosphate profiles are compared with the model's nutrients.-

Specific comments (ordering is not indicative of relative importance)

R4-Comment 1: There are several aspects of the model structure and application that are troubling and at least need to be clarified for readers benefit. 1) There appears to be no inclusion of air-sea O2 exchange in the configuration, based on Eq. 21 where none of the terms depend on wind speed. If this is really the case, this is a fundamental flaw. The reference provided as source for this aspect of the model (Gromiec, 1983) appears to be applicable to river applications and therefore may be a relatively poor choice as a basis for a marine application. However, it is also not clear why an oxygen compartment

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is needed since no mention of whether anoxic or hypoxic conditions regularly manifest in the Mediterranean is included in background information.-

AC-Analysis of the dissolved oxygen evolution requires simulations at least as long as the typical residence times in the Western Mediterranean, about 50 year, and in the Eastern Mediterranean, more than 100 years, well longer than the six years in the present atmospheric input runs. A specific sensitivity analysis to different air-sea oxygen exchanges - wind speed, sea state, thermohaline dependent - is required in order to obtain the maxima in the oxygen profiles at 800-1000 m. The present model configuration maximizes the importance of the positive flux due to photosynthesis and of the negative one to biochemical oxygen demand, for giving impact to the denitrification rate, without anoxia episodes revealed by the data in the pelagic areas studied by means of the model. The descriptions for eqs. (21) and (22) is changed as reported after comment 3, for introducing the oxygen behaviour in the euphotic zone, verified by us in some areas, and the future work to be done for comparing the effects in the intermediate and deeper layers of different modelled exchanges.-

R4-2) The configuration of the model's light field components also needs to be better explained and justification of the approach is a must. In particular, prescribing regional attenuation coefficients based on observations (section 2.3) is questionable and is a concern, as it may predispose the results. Especially since these K values are applied in the Chl transforms (Eqs. 23, 24). A much more satisfying approach would be to allow the model to determine attenuation freely and then have this available as an additional diagnostic for comparison to observed characteristics.-

AC-The light formulation in the ecological sub-model is in accord with the estimation of the heat budgets in the physical one. The PAR is dependent on the total radiation reaching the sea surface under clear sky conditions and on the clouds. The normalized day-length takes into account the declination and the latitude. We agree with the importance of introducing realistic values in k_z, the light extinction coefficient. The revised text is: *k_z (cm-1) is the light extinction coefficient, which varies zonally from 0.00034

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(far Eastern Mediterranean), 0.0005 (Ionian Sea), 0.0007 (Western Mediterranean) to 0.0011 (Alboran Sea). These values are in accord with Secchi disks measures (Rabitti et al., 1994; Martin and Bart, 1995).* A different formulation could be to reconstruct these differences by means of numerically simulated shading. This is connected with the introduction of the chlorophyll as independent variable, adapting Chl:C ratio. We do not choose this way of modelling in the present work, because it is difficult in our opinion to validate results due to present lack of carbon biomass data. In our modelling approach these light extinction parameters, regional measured, are fixed inside the model varying the light extinction with depth and they are consistently used in the diagnostic Chl:C ratios for the ultraplankton, S, and the netplankton, L. For this reasons we do not agree with Referee #4 that these parameters could be a concern for our modelling approach and could predispose the results: the validity of such formulation is confirmed by the validation of the surface chlorophyll maps.-

R4-3) Description of the numerical experiments is incomplete. The duration of the main run simulations is not prescribed in the methods (Section 2) and only mentioned in passing in the caption for Table 8 and within the section detailing the results. Also, there is a need for more detail in describing the forcing fields applied to the model, as opposed to the generic reference to forcing being based on NCEP reanalysis. Which fields are applied? Why is the 1980-1988 time frame chosen? Does this choice relate to some specific aspect of conditions over the Mediterranean region for this period?-

AC-The revised description of the numerical experiments in 2.6 is: *The simulations of this sensitivity numerical experiment start, after spin-up of the hydrodynamics spanning four years, from the realistic nitrogen and phosphorus initial conditions and imposing the Gibraltar, Adriatic and Aegean relaxations and the riverine loads; the analysed biogeochemical numerical evolutions cover six years. Three different atmospheric inputs of dissolved inorganic nitrogen (DIN) and dissolved inorganic phosphorus (DIP) drive, through the same physical conditions, three different ecosystem scenarios. In the no input run, NIRUN, atmospheric deposition is zero. The second one represents the

‘Continuous inputs after Guerzoni et al. (1999; see Tab. 16 of their work)’, referred to as GARUN hereafter. The third run follows intermediate loads of nutrients, the ‘Continuous inputs after Ribera et al. (2003; see Tab. 7)’, hereafter as AVRUN. These atmospheric inputs, see Tables 2 and 3, are uniformly distributed along the year, loading equal nutrient amounts for each 2400 s time step at the 10 m thick surface layer. For what regards the Mediterranean grid, zonal different loads are taken into account in the eastern (Markaki et al., 2003; from Tab. 3, the inputs comes from dry and wet sum), central and western basin (Bergametti et al., 1992, Loye-Pilot et al., 1990); the central area GARUN inputs in Tables 2 and 3 are directly estimated from the authors, while AVRUN ones are the eastern and western average values.*- The forcing fields are described in the revised text, as well as the restoration buffer zones. The motivation for choosing this period in the eighties is twofold: the evaluation of the water fluxes at Gibraltar and at Sicily Channel near to the measures; after realization that this is the main engine of the zonal oligotrophy of the Mediterranean, the physical submodel contains big interest in a climatological sense. The second point is that Eastern Mediterranean transient happened in the early nineties, as remembered in our interactive comments, increasing the deep water formation and determining the uplifting of the deeper and intermediate layers. This changed the situation in terms of controlling results with previous data. Also the model should be capable of introducing these convective processes in high frequency.-

R4-Comment 2: In general, the presentation of material is difficult to track since its ordering is rather disjointed. For example, in the introduction the motivations for the study are described on the bottom of p. 911. However, this is then followed by text that is more suitable as background information that should come prior to model motivations. Similarly, the last few paragraphs of the hydrodynamic model description (section 2.1) would be more appropriate to include as part of introductory material.-

AC-The background informations are extended in the revised version and precede the aim of the work. The descriptive points of the 2.1 hydrodynamical section are mod-

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ified and maintained there because they are results by the cited Korres, Pinardi and Lascaratos (2000) work about the physical sub-model.-

R4-Comment 3: The description of the model's two main components is quite detailed. In particular for the biochemical application being presented here, details on the physical model portion are quite extensive. Has the physical model been reported elsewhere and could that be cited as a reference for interested reader? If there is not such a resource, then it is really a necessity to report on how well the physical model performs. Indeed, with the 1/4 degree resolution employed for this application, it is disappointing that analysis of the model results is largely limited to assessing/describing annual mean fields and characterization of mechanistic interaction between physical environment and the biological components is not addressed.

AC-The related paper, see Comment 2, is cited in the text for detailing the physical submodel. GARUN monthly chlorophyll data, at surface and in the transects, were presented in our ppt file presented at the final ADIOS meeting with our seasonal considerations and distributed after that on the www. In order to summarise these sensitivity results in the present work, we decided to show the response of the system as impact on the biomasses and budgets, integrated in annual average values, last three years of the six years repeating-cycle simulations and in the two subbasins. Some general comparisons with field data are now considered in the revised text from a seasonal point of view.-

R4-Why is it necessary to provide conservation equations for both T and S. Since there is no inclusion of penetrative component of irradiance within the heat flux equation, the structure of these equations is identical. For the biotic model description (section 2.2), there are several cases where explanations need to be clarified or amplified.-

AC-The equations for the thermodynamics of the seawater are cut in the revised version. There are general descriptions of the air-sea heat fluxes and of the salinity relaxation at the sea surface. Papers discussing the total radiation reaching the sea surface

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and the heat budget calibration are now referenced.-

R4-1) What is meant by ‘detritus depend on evolution of dynamics’ (2nd paragraph)? This is in general true, so either it is trivially obvious or possibly there is some specific mechanism being referred to and there is need to make this understood by reader.-

AC-The phrase refers not to detritus dynamics but to nutrient ratios inside the detritus. The revised text is: *differently the nutrient ratios in the detritus are not fixed, depending on the different remineralization rates chosen for the phosphorus, nitrogen and carbon components contained in the detritus.*-

R4-2) It would be more accurate to state that the generic B equation is based on the tracer equations (T or S), with additional sources and sinks associated with biogeochemical processes, as opposed to linking B to the momentum equation set where there is the additional Coriolis term. Also, what is meant by ‘local derivative terms’? This needs clarification and should have been mentioned in presentation of Eqs. 1, 4, and 5 as well since it would appear to be originating there.-

AC-We agree. It is proper for the generic biochemical tracer B to refer here to the physical tracers, not to momentum equations; we changed the text as follows. In fact the transport terms are the same, only biochemistry diffuse differently with respect to T and S because of different constants. Another difference is the presence in the physical tracers of fluxes at surface, such as heat flux and salinity relaxation, while the biochemical variables are subject to insulating conditions, except surface relaxation for dissolved oxygen. Specific biogeochemical sources and sinks are introduced in the revised text: *The evolution equation for the generic biochemical compartment, B, which is resolved at the same grid of physical tracer equation, contains advection, diffusion terms, vertical sinking and biogeochemical sources and sinks: (Eq. for B) *-

R4-3) Need to provide explanation for the water column and biological instabilities that are noted on p. 916. Without additional context, it is left completely up to the reader to extrapolate the intended meaning.-

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AC-The biological instabilities are controlled by means of the following borrowing procedure: *On the other hand, phytoplankton and zooplankton instabilities are treated via borrowing, as explained in the following, 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) *-

R4-In general, the sections describing the biotic compartments of the model need to be better developed. For one thing, there is inconsistent inclusion of multiply signs (x) within the equations (particularly 15-17). As well, some of these sections include good textual descriptions of the processes associated with the various terms within the pertinent equation (for example Eq. 17) while in others the reader is left to ascertain these processes with Eq. 21 in particular needing further detail.-

AC- Now the equations are consistently edited. The revised description for the dissolved oxygen evolution is: *The equation for the dissolved oxygen, O, takes into account as sources, ultraplankton and netplankton growths, and as sinks, respiration terms, nitrification and carbonaceous biochemical oxygen demand: (Eq. 21) The last term in Eq. (21) is the surficial oxygen relaxation performed by means of the reaeration in fuction of temperature and salinity (Gromiec, 1983): (Eq. 22) This formulation responses in terms of maxima of the dissolved oxygen in correspondence with the phytoplankton growth and of minima at the base of the euphotic zone; it should be

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verified and possibly improved for describing long-term dissolved oxygen behaviour in the intermediate and deeper layers. *-

R4-Comment 4: The referencing is dated. Of the 14 cited sources that appear since 2000, 4 are technical reports and 1 is an abstract. Also, none of these is more recent than 2003. Furthermore, there appears to be only one citation that relates to testing sensitivity of marine ecosystem to atmospheric deposition of nutrients and this cited work is the aforementioned meeting abstract. There are a number of modeling studies appearing over the past 4 years that investigate sensitivity of marine systems to aeolian deposition and some comparison to, and acknowledgment of, these works is a necessity. Indeed, its lack of currency is a substantial shortcoming and could be an indicator that this manuscript has met with difficulty in past attempts at publication.-

AC-Our aim is to report in this work the ADIOS results with the atmospheric data and methods chosen at the beginning of the project for obtaining the best estimates in a seasonal/repeating year view; in this sense we do not want to load with different expectations this sensitivity numerical experiment. In any case, we have followed the way of reasoning of Referee #4 about the importance to overcome these difficulties that could date the results of this model, even in absence of different evaluations/interpretations. As we confirm after our interactive comments, it is not possible for us to introduce new data and methods for the reasons raised after every point discussed before, but there is a link in the revised introduction with other studies in this field. JGOFS works, about the seasonal biochemical cycles in different environment, eutrophic and also subject to permanent features, are cited in the previous introduction giving insight into the new methods and analysis of data. The principal applications, that these modelling activities are having in their climatological effects, are at the same time important. Now in the introduction the validity of these approaches, that are similar to ours, is considered for biogeochemical interactions in medium and longer time scales, with aeolian deposition and air-sea variabilities.-

Interactive comment on Biogeosciences Discuss., 4, 909, 2007.