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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

Anonymous Referee #1

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Pacciaroni and Crispi describe a coupled physical-biological modeling study of the Mediterranean Sea, focusing on impacts of atmospheric deposition of nitrogen and phosphorus. The physical model is a Mediterranean implementation of POM. The biological model is a relatively simple NPZD type of formulation, that includes large and small phytoplankton size classes and also N and P cycling and limitations. The authors argue that the model captures major aspects of the observed temporal and spatial chlorophyll variability, and therefore provides a means of assessing how atmospheric deposition likely impacts the system.

My overall impression of this study is that it is a potentially important contribution to our general understanding of the impact of atmospheric nutrient inputs on the Mediterranean Sea. The model reveals how these inputs influence chlorophyll concentrations in both the eastern and western regions of the Mediterranean and also how phytoplankton size structure (ultraplankton versus netplankton) might be differently impacted in oligotrophic versus more eutrophic waters. These demonstrated influences are probably robust. However, my confidence in the model results are significantly compromised by the fact the model fails to reproduce some major features of the observed chlorophyll variability. This failure is most apparent in the comparison between modeled and observed vertical sections (Figures 6 - 9), where the observed deep chlorophyll maximum variability departs substantially from the model. For example, in the E-W transect (Figure 6) along the western half of the section the model generates a strong shoaling of the DCM that is not apparent in the observations, and also in a N-S transect (Figure 8) in the western basin where the observed deep chlorophyll maximum is consistently ˜50 meters deeper than observed. In addition, there are disturbing differences between the modeled and observed variability (e.g., Figure 9), where the model appears to be generating much more spatial variability in the integrated chlorophyll than observed.

The authors provide some explanations for these discrepancies, related mostly to potential problems with the data (i.e., sparse data and seasonal bias), but as it stands these comparisons give the strong impression that there are some substantial errors in the physical solution (e.g., poor representation of the pycnocline depth and therefore nutricline depth and the deep chlorophyll maximum). But it is not possible to assess how good the physical solution is because no information is given on the physical solution, i.e., no plots of the temperature, salinity or density fields relative to observed patterns on these same sections, or on any other sections. So my first and probably most significant recommendation is that the authors need to go back and validate the physical solution at some level to make sure that it is not the route cause of these discrepancies, and report on this to some degree in this paper.

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I am also concerned about the lack of statistical analysis. The authors validate the model using a lot of spatially and temporally averaged quantities, and they discuss differences between the model runs and differences between the model results and the observations, but no confidence intervals are calculated. It is therefore impossible to assess the significance of these differences. Confidence intervals should be calculated for all of the tabular mean values reported (e.g., tables 4-9). I fear the authors may find that the variability is large and that the differences reported are not statistically significant.

Finally, another significant concern I have about this paper is the complete lack of validation data on nutrient distributions (i.e., DIN and DIP) and also primary and secondary production. There must be historical data and transects in the Mediterranean that can be used for comparison with the model. It is particularly crucial to assess whether or not the DIN and DIP concentrations in surface waters and at depth are correct and determine how well the model reproduces the vertical position of the nutricline. Validating these fields will very likely shed some light on the discrepancies in the chlorophyll fields described above. And at least some tabular comparisons of modeled versus observed primary production rate should be included.

Finally, the sentence structure in this paper is awkward in many places. I have pointed out some of these problems in my specific comments, but there are many other places in the paper where rewriting is needed. This manuscript should be given a very thorough editing to correct these kinds of problems before it is published.

See also my specific comments below.

Specific Comments:

P. 910, Lines 16-17: “Ěcycling at low nutrient sillĚ” this statement doesn’t make sense.

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

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

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Interactive Comment

P. 911, Line 27: “The aim of this paper is to clear in which wayĚ” replace the word “clear” with the word “clarify”.

P. 911, Line 29: “The importance of external loads cannot easily be underestimatedĚ” This is an odd statement. Its not clear what the authors are trying to convey hereĚ

P. 912, Line 13: “Biogeochemical setups descriptions of differentĚ” this doesn’t make sense.

P. 912, Line 21: Replace “raise” with “rise”.

P. 912, Lines 26-29: Very awkward phrasing.

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

P. 915, Lines 7 and 9: Need to define “MAW” and “LIW”.

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 excretionĚ” Some additional clarification is needed here. I am not familiar with this procedure. Is it a method to damp biological instabilities in the model?

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).

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

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

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seasonality and comparisons between the model and the observations?

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

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.

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.

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.

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.

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.

P. 928, Lines 16-19: Here to the comparison with observations reveals glaring discrep-

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ancies 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?

P. 929, Lines 2-12: The differences between the modeled and observed intergrated 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?

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.

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.

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?

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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.

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