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Ocean Science
Discussions

Interactive comment on "Eastern Mediterranean biogeochemical flux model: simulations of the pelagic ecosystem" *by* G. Petihakis et al.

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1 General comment

This manuscript presents an application of a biogeochemical model to the Eastern Mediterranean, a region of great importance for the understanding of the whole Mediterranean ecosystem.

As correctly stated by the authors in the introduction, this region is characterized by an extreme oligotrophy and deep chlorophyll maxima generally composed of the smallest cells of the planktonic community. We therefore would claim the necessity to use a complex ecosystem model to capture the intricacy of carbon and nutrient flows within the microbial loop, in contrast with a more traditional one-nutrient, one-phytoplankton and one-grazer approach which would probably fail to describe these conditions. The BFM `http://www.bo.ingv.it/bfm` is a substantial "modern" rewriting of the original ERSEM parameterizations (Baretta et al., 1995), with some improvements and additions. The BFM is still strongly linked to the ERSEM original idea of interconnected material flows mediated by the actions of functional groups, where all the different biogeochemical cycles of elements participates to the physiological responses.

The genericity of the BFM should thus allow the simulation of both eutrophic and oligotrophic conditions, by permitting the coexistence of various kind of (fixed) food webs. It is a limited view of the complexity of marine ecosystems, firstly because it is reduced to the lower trophic levels, and secondly because only a portion of the plasticity of ecological and physiological processes can be currently implemented in deterministic models.

The major weakness of the manuscript is the absence of a critical analysis on the model behaviour in this region, which clearly indicate the assumptions, the limitations and possible suggestions for improvements. Indeed, as a general comment (and as one of the authors of the BFM), I have some problems in visualizing the overall aims of the manuscript in its present form.

- If it is *in primis* a description of the model, then I would have preferred a more mathematical "format", at least in accordance with the efforts made by the BFM team (see the documentation section in http://www.bo.ingv.it/bfm). One of the major weaknesses of ERSEM was the presentation of model parameterizations in the form of a meta-language resambling the code implementation. The attempt of the BFM is to clearly present the theoretical basis of the biogeochemical parameterizations and not the code of the model. The mathematical translation of observed physiological behaviour is in the form of the equations and only by spreading a common formalism we will achieve an adequate degree of consensus on the more suitable deterministic formulations.

- If, on the other hand, it is a calibration and validation of the BFM in this area (as I think it should be), then a more thorough comparison with the available data is needed, not just a verbal description and comparison of table values with maps.
- Finally, if it is a presentation of the potential capabilities of a predictive tool in the framework of the MFS objectives, then I would suggest that the validation part I mentioned above be addressed carefully and thoroughly before applying the tool itself to predictability problems.

I agree with the authors that this work is a first promising step towards successful uses of this tool in the Levantine basin, however, a considerable amount of work is, in my opinion, needed to bring this work to publishable standard. A substantial rewriting and a more robust validation exercise is needed, which implies that the manuscript could be acceptable only after a major revision that takes into account the comments below.

2 Specific comments

Beyond some specific comments given below, there are some general aspects of the paper that need to be revisited.

1. I would suggest to remove the word “ecosystem” from the title and use “biogeochemistry” because the model is capable to simulate only the lower trophic levels of the ecosystem.
2. The Introduction is interesting and well done, but the actual introduction on the paper topic is just limited to 4 lines at page 1353. This is rather unbalanced.

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3. The description of the equations is important, but should, in my opinion, be limited to the aspects of the BFM that are crucial for the Levantine Basin. For the remainder of the equations I would suggest to refer to the BFM equation description, which is available on line in the BFM web site. A partial description of the BFM equations in a global ocean context is also available on a peer-reviewed publication that is in press on the Journal of Marine Systems (Vichi et al., 2006, a preprint version can be downloaded at `http://hdl.handle.net/2122/1176`). In addition, units have to be checked throughout the text and the tables. The IS MKS abbreviation for milligram is mg and not mgr.

4. Section 3 is rather unsatisfactory. I already have expressed my doubts in the General Comments on the validation approach. The community knows well how little information we have on the biogeochemistry of the Mediterranean, because the available measurements are sparse, sporadic and focused on certain periods of the year.
However, this is what we have and on this set of information we have to challenge the assumptions of the model. In this respect, the comparison was done only in an approximate and qualitative way and needs to be improved. The continuous shifting back and forth from a table to a map trying to remember the value and the approximate location of the "S. Aegean" is frustrating. What are the boundaries of the S-Aegean in the model? What is the spatial representation of the measurements? I understand how difficult it is to extrapolate point information to a region, but providing even an approximate definition of the region both in the model and in the data is much better than nothing. Given the evident spatial patchiness of the biogeochemical variables in the region, it is very difficult for the reader to compare an average value to a map. The variability at certain data locations is even mentioned in the text (page 1371, line 20), but no other information is given. Data have to be shown together with model results!
Satellite data are affected by several problems (some of them clearly identified

by the authors), but since the authors themselves have used these data to test assimilation techniques in the framework of the MFSTEP project, I wonder why they are not showing any comparison with satellite-derived chlorophyll maps.
Concerning the calibration phase (page 1353, line 20), I could not find any reference to it in the text.

5. I feel the authors have missed an important occasion here to increase our understanding of the processes driving the mean state conditions in the Levantine basin. Previous studies (Crise et al., 1999) have shown that the zonal gradient in chlorophyll is consistent with the general anti-estuarine circulation of the basin, and that this feature can be captured by a simple NPZD model.
   I do hope that the BFM is capable to do the same, because the mean state of the physics is the major driver of this pattern. Being a climatological implementation, we cannot expect the model to capture the variability. But we can expect the BFM to provide some insights on the cycling of carbon within the microbial food web. Indeed the ranges of simulated biomass do compare well with the available observations. There are a lot of interesting results in Section 3, but they are unfortunately hidden by the unwieldiness of the presentation. The manuscript does not give any explanation on neither why the model does a fair job, or why the model fails.
   In addition, Figure 9 and 10 are never mentioned in the text though described, which is to me an indication of the preliminary stage of the manuscript.

6. The Discussion section is completely absent and the first part of the Conclusions is mostly another Introduction. I would expect to see a detailed discussion of the success (or unsuccess) in the model of predicting the depth and location of the DCM, particularly in relation to (and within the limits of) the physical evolution provided by climatological forcings. The authors give 4 very interesting conclusions in the Conclusions (Page 1375, lines 14-18), but none of them has been supported in the previous sections by model results.

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3 Technical comments

Several typos and grammatical errors. The authors should ensure a thorough spell-checking before submitting the manuscript.

P1350,abstract L5: Biogeochemical Flux Model (check also in other parts of the manuscript). L6: in regional areas

P1350,L20 This instead of thi

P1351 L2: Provide a literature reference for this water loss. L18: sentence unclear, all the verbs are in -ing!

P1354 L8-13: provide a reference for the runoff rates. L16: is this a range of variability? not clear! L25 The only possible, not the most accurate. Eq (1) partial derivatives instead of thetas in the first fraction.

P1355 L3: the concept of pelagic group has not been introduced yet, better use biogeochemical state variable. L5: surface or columns? not clear. L6: the use of "account" is improper. Data are provided to the ecosystem model and the model itself does not account for anything. L7: provide a reference for the annual nutrient inputs. L13: spell POM and DOM, Particulate Organic Matter can be confused with the Princeton Ocean Model, and units are not clear.
Note: ERSEM III actually was incorporated into the BFM project. The document to which the authors make reference was only for internal use to the BFM team and the revised version is now available in the document section of the BFM web site.
L26: Which models? please provide a reference.

P1360 Provide a reference to the original Geider formulation for chlorophyll synthesis.

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P1368 Provide a reference for the readers not familiar with the DYFAMED acronym, the same for the list of acronyms in the next page.

P1369 L21: none, not non. L26: “From the above”, is not clear.

P1370 L1: provide a reference to the work done during the MFSPP. L24: is it surface or average chlorophyll? L27: expected with respect to what observations? please provide references.

P1371 L1: “Basin scale...” is a totally unclear sentence, maybe something is missing.

P1372 L1: “the center of which”, better “whose center”.

P1373 L4: the distribution of bacterial biomass is first referred to as uniformly distributed but afterward as variable. Unclear. L13: why “limiting”? Is bacterial production limited by dissolved nutrients? L20: provide the same percentages also for the model as comparison.

4 References

The cited references are all available in the manuscript bibliography.

Interactive comment on Ocean Sci. Discuss., 3, 1349, 2006.