

Interactive comment on “Pre-operational short-term forecasts for the Mediterranean Sea biogeochemistry” by P. Lazzari et al.

Anonymous Referee #1

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In this paper, the authors present a simulation model for the ecosystem dynamics of the Mediterranean Sea (OPATM-BFM). BFM is an ERSEM-type ecosystem model that is applied in a pre-operational form with the support of the MFS OGCM and the OPA tracer model (OPATM). Emphasis in the paper is laid on the technical description of the operational implementation of the model, and on a fairly general description of how the model fits the observational data. Non parametric indicators are presented graphically for the comparison with the satellite observations. Some very limited interpretation of model results is given in terms of the ecosystem functioning. Describing model results obtained with such a large model that has been used in a limited number of runs is notoriously difficult. One almost needs a book to describe all the biological and chemical formulations used in the model, several pages of parameter values plus an extensive description of the sources of all parameter values, many figures illustrating the output

C386

of all variables plus the observational data, and only after all this one can start describing what the model can be used for. Framing all this into a single publication is physically impossible. One has to make choices and choose a clear focal point for the publication. It is my feeling that the authors of the present paper have not done this. They devote much space in the paper to a detailed description of the pre-operational system, while they provide a rather short part on the analysis and the validation of the model results without justifying their choice. They do not give values of the parameters at all, instead referring to data bases and publications. I guess that, since the values of the parameters have remained the same, also the formulations haven't changed. However, since the reader would probably like to access both the formulations and the parameter values, I propose that this material is made available as an electronic appendage to this paper. This would save quite some space in the paper, while at the same time making the description of the model more complete. Also, since BFM/ERSEM is a relatively well-known and well-documented model, the reader should be guided by detailing what version of the model was used, where formulations deviate from the source, where parameter values deviate from the default and how this can be justified. Such a description would place emphasis where the reader needs the information, and leave out redundant information that has already been published elsewhere. The problem of parameter settings is, in this respect, of the utmost importance. I also found the description of the validation very uninformative. The provided statistics of chlorophyll correlation and percentiles can tell everything and nothing. Without a detailed comparison of data and model for at least a subset of variables, there is no way the reader can get any feeling for the randomness of deviations between model and data, the occurrence of systematic bias in the model predictions, or structural shortcomings of the model. It remains too vague. Again, choices will have to be made. I recognize that the validation of a 3D ecosystem model is usually a difficult task due to limited observations. A solution I suggest, is the use of a cost function. It is a mathematical function enabling the comparison of model results with field measurements estimating a non-dimensional value which is indicative of how close or how

C387

distant two particular values are. Using the cost function you may identify regions /time periods where the model is performing well and regions/time periods it not. Using the Spearman correlation overall Mediterranean you significantly smooth the model and the satellite data missing important information. Please make sure that you demonstrate where the model behaves very well and where systematic shortcomings can be seen (if any, of course). The point of a publication like this is not to try and convince the world that your model is perfect, but to contribute to model development by critical appraisal of what works and what doesn't – so that it really becomes a trustable operational tool! In the model all the validation is based on the satellite observations and there is no reference about the part of the food web which has very limited profiles of in-situ data like bacteria, microzooplankton and mesozooplankton. It can be interesting to take some of this model output into consideration when trying to interpret the dynamics of phytoplankton (a group that can be validated, at least partly), but discussion should be restricted to no more than that.

Summarizing, I think this paper is based on interesting work that deserves to be published, but also deserves to be published much better than in the present manuscript. I think the authors should thoroughly rework their entire manuscript and resubmit. The guidelines for this reworking should be: focus on what is really different between this model and known versions of BFM/ERSEM; document all details of the model in an electronic appendage but keep the text to the main lines; discuss the validation of the model in a critical way, and show directly where the model works and where it fails in a spirit of trying to improve the modeling afterwards. And as a final guideline, I would add: use existing literature for the ecosystem of the Mediterranean much better!

The paper also needs a lot of editorial changes. I will not give a list of detailed comments, because I think the paper needs to be rewritten anyway. Just a few principles: Simplify the part three of the manuscript (The pre-operational system). It is very technical and I think that all these details will confuse the reader. Please read carefully and avoid any computer-specific statements (like LoadLeveler, LSF, etc). Finally make sure

C388

the English is checked for grammar and clarity before your resubmit.

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C389