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Ocean Science
Discussions

Interactive comment on "Reduced-order optimal interpolation for biomass assimilation" *by* G. Crispi et al.

Anonymous Referee #1

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1 General comments

The manuscript presents a technical application of a well-established data assimilation method to a coupled physical-biogeochemical model of the Mediterranean Sea. This topic has been thoroughly discussed in many recent papers in the literature, from zero-dimensional applications to full 3-D implementations in several ocean regions. However, the authors do not give any background information on this topic, particularly on other published work in the Mediterranean (e.g. Triantafyllou et al., 2005 and references therein).

The implementation of an assimilation filter to describe the whole Mediterranean biogeochemistry is indeed novel. However, the manuscript does not meet the required standards of a Peer-reviewed internetational publication. The method and the scientific tools are not clearly presented in the introduction, and also the major aims of the work are rather confused. The concept of Observational System Simulation Experiments is weakly outlined and there is no reference in the text of the basic methodology of OSSES (see specific comments below). The use of ocean color satellite data is also referred to in the introduction, although the assimilation is carried out with “phytoplankton nitrogen”, which is indeed a state variable of the NPZD model, but is not for sure a data that is readily available with space-born sensors. This point is not even considered in the text.

The current form of the manuscript is rather difficult to read, partly for the awkward English and partly for the scattered presentation of important information. I am not a native English speaker, but I had quite some problems in interpreting the meaning of several sentences (see a list in technical comments).

The overall structure is confused, some details on the forcing functions are given in the ecological model setup, and some others are mentioned two sections ahead in the presentation of the twin-experiment results. Particularly, it is not clear what is the exact setup of the reference experiment and the design of the OSSE. A surface map of phytoplankton nitrogen evolution is given, but there is no discussion on how well the reference simulation reproduces the major biogeochemical features of the Mediterranean. The authors claim in the conclusions that the “surface biomass constraints succesfully drive phytoplankton concentration toward better forecasting”. This conclusion is not supported by the presented results, particularly because it was not clearly stated what the time-window goal forecast was. The assimilation procedure does not lead to any evident improvement of predictability as clearly demonstrated by the convergence of the reference, free and assimilated simulations at the end of the simulation period. This clearly shows that inadequate initial conditions can be recovered by internal adjustment as the seasonal cycle progresses. If the aim of the work was to provide

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a method for the recovery of proper initial conditions, then there are much more suitable tools in the oceanographic literature to be used (e.g. Auclair et al., 2005). If, on the other hand, the aim was the improvement of biogeochemistry model performances, then it is necessary to clearly define the predictability window. If the time window of predictability was 1-2 weeks, then the assimilation procedure is inadequate. If it is longer than 1 month, then there is no need to apply a filter because the convergence ois ensured by dynamical interaction with external forcing functions.

In summary, this work is a mere technical application of a mathematical method with apparently little emphasis on the problems related to the use of this method for “biomass data” assimilation. The manuscript cannot be accepted for publication in its current form. A substantial rewriting is needed with a clear presentation of the final aims of the exercise, together with an objective interpretation of the performance of the filter for the specific purposes.

2 Specific comments

1. There is little reference to more recent literature on the Mediterranean Sea biogeochemistry and also on the application of OSSEs.

2. An OSSE is a simulation of an observing network, and the outcome of the experiment is the evaluation of the skill of the chosen network to reproduce the observable system. A typical methodology is the use of twin-experiments, one representing a synthetic truth (the observable system) and one that is the representation of the system derived from a subset of information collected from the synthetic truth. None of this is described in the text.
   There are no details in the text on the chosen observational network. It looks like data are subsampled from the synthetic truth at 2 levels for each grid point, using

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weekly averages. This means that the observational system designed by the authors must be able to provide information on the phytoplankton nitrogen content every day and at 2 different depths. I wonder whether the authors have considered the feasibility of such a sampling strategy, particularly in the framework of a Mediterranean Forecasting System aiming at operational forecasts of the real state of the ecosystem. Satellites can only provide an integrated information on the concentration of photosynthetic pigments within an optical depth and therefore it is not possible to have two distinct levels. Moreover, ocean color satellites cannot provide weekly averages, but only weekly composites. Finally, satellites give information on water-leaving irradiance, which can be translated into chlorophyll information through empirical algorithms. To derive nitrogen content from this measure requires additional information. An operational sampling strategy at least needs to discuss this topic and consider the related uncertainties.

3. Page 510. Many technical details on the computer systems but little description of the assimilation method and none of the hydrodynamical model, which is just menthioned (and for the first time) at page 514.

4. Section 3, page 511.
   One single profile is used to initialise the whole Mediterranean. What are the consequences of this choice? How does the reference run differ if more than one profile is taken into account for the initialization?
   The authors should provide some details on the choice of the 120 days required for the internal adjustment. Why not using an entire annual cycle?

5. Section 4, page 513.
   Figure 6 does not depict an assimilation cycle, but simply an information flow. Many technical details are provided on the way SOFA reads data but no information on the choice of the data (see point 2 above).
   The authors should justify the choice of a 69 days simulation.

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The FR gives a completely different behavior because it starts from perturbed initial conditions. However, the time evolution is from a biological point of view comparable with the reference run, as also stated by the authors. Therefore, I do not understand how the authors can say that distinct higher values are obtained (without any objective measure but visual comparison). Given the uncertainties inherent to NPZD model formulations, my personal view is that all the experiments converge.The authors should justify their statements. Yes, the AR is slightly closer to the RR but this is by no means a demonstration of the filter capability to recover the synthetic truth conditions. I would suggest the authros to do an ensembles of simulations with different perturbed initial conditions, to see the statistical spread of the AR in all the cases. This would give a more robust assessment of the filter perfomance.
Paragraph starting at line 22. Since N is conserved (no external inputs) the filter will have to recover the initial conditions of the FR by artificially adding new nitrogen in surface phytoplankton. It is thus not possible that SOFA can spread this information in all the N compartment. This is an inherent limitations of biomass assimilation, that should have been discussed furthermore.

6. Section 4, page 517
I agree with the choice of the objective measures but not with the separation in three depth ranges. The third range R3 is always indistinguishable from the measure done in the interval 20-4000 m, which means that the correction is only capable to propagate in the first 20 meters. However, the conclusions report a different interpretation (p519,l23) which is not supported by the results shown in this section.
I would suggest the authors to report a reference number of $\sigma_{AR/FR}$ which is acceptable, and give details on the choice.
Figure 9 shows a clear 7-days cycle which can be related to the assimilation cycle. It looks like SOFA is simply acting as a nudging filter, and not really merging

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the data with the model. Particularly, the correction is reduced at the end because the 3 runs converge. The authors should provide some more details on the parameters they used to implement the filter.
How do the authors explain the relative improvement in DIN in the first 25 days observed in Fig. 11?

7. Fig. 13 and 14. It is not clear why the relative error indicates a poor performance of the filter while the total nitrogen AR performs “slightly” better than the FR. If there is a “better forecasting average” (whatever it means), than the relative error should show this. This hints at a low robustness of the chosen indicator. It might be that since the FR and AR are so similarto each other and to RR, the relative error ratio largely oscillates around 1, artificially magnifying the difference.
The authors should aso explain better what they mean with “deteriorating effects of the assimilation univariate scheme” in the western basin (p518,l17). Do they imply that the ecosystem model is not capable of adequately simulate the western basin, to the extent that the assimilation filter cannot recover the reference simulation? The authors should first give a quantitative measure of the deterioration, if any, and also investigate further the biological reasonsof this point finding.

8. Page 519, paragraph starting at line 3. It is not clear whether these speculations are derived from the results presented in the manuscript or from previous knowledge. I have no direct knowledge that sloppy feeding is a relevant pathway of organic detritus production. The authors should justify their speculations if inferred from their results. The eastern Mediterranean is not a phytoplankton-dominated ecosystem. It is an oligotrophuc ecosystem, characterized by picophytoplankton and their nanoplanktonic grazers. There is indeed little production of detritus, but this is because of the microbial loop dynamics that leads to the formation of dissolved organic substances.

9. The conclusions do not reflect what shown in the text. There is no clear demon-

stration of the success of the filter, and it is not shown that the analysis improves the results also in the euphotic zone, because only the layers R1 and R2 are shown. The euphotic layer extends down to 100 m in the eastern Mediterranean. The authors mention the influence of concentration in deeper layers via turbulent mixing processes. This inference is not supported by any evidence in the previous sections. The distinction between eastern and westerm Mediterraneann performances is not supported by the results. The relative error $\sigma_{AR/FR}$ increases in both subbasins according to Fig. 16.

3 Technical details

p511,l12 Readers are not necessarily familiar with these codes. Please, provide geographical regions.

p511,l16 Does this mean that these are the open boundary conditions? Please detail.

p511,l19 "... in keeping...". The meaning of this sentence is obscure. In addition, there are much more recent data that can be used (e.g. WOD01).

p512,l12 "Moreover,..." Is this detritus initialization applied to the whole Mediterranean or only to the Aegean? This is not clear from the text.

p513,l5 The reference run is first mentioned here, but explained in the next section. Very confusing.

p513,l14 "The along-shore... if permanent.". Sentence unclear.

p513,l19 Other information on the forcing functions. Is this different from the forcing used for the adjustment period? Please, collect all the information in one introductory section.

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p515,l1 Use “prescribed”, not “injected”, this is not a laboratory experiment with inoculation. The use of terminology is rather poor throughout the manuscript.

p515,l28-29 Atlantic water, and Levantine water.

p516,l1 “Apport” has a different meaning.

P516,l6 Which previous model results? This sentence is not clear if it refers to the estimates referenced above or to other model results. Also below, l12, please give reference to the source of the new estimates.

P516,l17 “How much assimilation is important”. Better say “the performance of the assimilation filter”, otherwise it looks like assimilation is just a nudging factor and not a dynamical melding of data and models.

p517,l16 “The better increase of the assimilation error versus the free error...” . The “better increase” of an error is a rather cryptic concept. Please, rephrase.

p517,l21 “As expected”. It is not obvious why it should be expected.

p518,l8 Sentence not clear.Please reformulate.

p518,l28 “the Western Mediterranean adversely acts in the forecasting”. Very imaginative, but meaningless. Probably the authors meant that the filter performance was poor in the western Mediterranean.

4 References

Triantafyllou G, Hoteit I, Korres G, Petihakis G, 2005 Ecosystem Modeling and Data Assimilation of Physical-Biogeochemical processes in Shelf and Regional Areas of the

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Mediterranean Sea Applied Numerical Analysis Computational Mathematics Volume 2, Issue 2 , Pages 262 - 280

Interactive comment on Ocean Sci. Discuss., 3, 503, 2006.