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Ocean Science
Discussions

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

G. Crispi et al.

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Authors' comments to Referee #1 for the manuscript "Reduced-order optimal interpolation for biomass data assimilation" by Crispi, Pacciaroni and Viezzoli for the Ocean Science, MFSTEP Special Issue. September 2006. The new title is: "Simulating biomass assimilation in a Mediterranean ecosystem model using SOFA: setup and identical twin experiments".

The manuscript is revised taking into account all the specific comments and technical details by the Referee #1. To show our points, we introduce the new Fig. 8 in the revised manuscript, plotting root mean squared differences of the free run with respect to the control (previous version reference) one. This phytoplankton rms Fig. 8a starts from a value not decreasing till the last ninth cycle. This result means that the average phytplankton Fig.6a (previous Fig. 7a) converges at a basin scale, because the winter bloom is occurring in both twin experiments but with different local values. The results given in Fig 6a combined with Fig. 8a demonstrate the non-convergence of phytoplankton along the free run evolution and the moderate effect of the assimilation in reducing the errors. The action of the SOFA is in the reported identical twin experiments figures based on initial phytoplankton condition perturbation. Anyway the initial condition perturbations are able to change the trajectories of all the ecosystem variables, Z, N, D (see Fig. 8 b, c, d). The trophic chains developed during MTP I and II, like NPZD, are respectful of oligotrophy, seasonal cycle and biochemical gradients in the Mediterranean Sea. References are given and not modified in the revised version. In the following our discussion of the specific comments are preceded by -C-; in the case of the technical details we give the revised text between asterisks.

Specific comments

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

-C- OSSEs as strategies are introduced citing Raicich (2006), as hypothetical data network in simulated system citing Vecchi and Harrison (2006). We cite in the revised introduction the work by Triantafyllou et al. (2005) on 1D and 3D applications of twin experiments in the Cretan Sea made with Kalman filter techniques.

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 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.

-C- The twin experiment methodology in the framework of the OSSE is now given in the introduction as follows: *The methodology of identical twin experiments (ITE) is chosen for understanding the surface biomass data impact on the ecosystem, taking into account the knowledge of the biomass coverage all over the basin. Synthetic “sea-truth” data are generated by a control run for assimilation in the ITE. The twin experiments consist in a free run with some modified conditions (initial conditions, parameters of the model, forcing functions, etc.) with respect to the control run and a comparison simulation, with the same modified conditions of the free, assimilating in addition the data extracted from the control run according to the network design.* The reference run is renamed control run in the revised version. The data network is designed with two values of the biomass at 5 and at 15 m , all points of the basin. This is understandable in analogy with the penetration optical length in the Mediterranean. The choice of the biomass as proxy of the chlorophyll should suggest that identical values are chosen at the all the top layers. We choose here, in our opinion, a more realistic procedure starting from the fact that the phytoplankton variable is dependent on the specific in situ depths. This maintains its validity when chlorophyll corrections are introduced in an ecosystem model instead of biomass, with one single average concentration at each position. In fact the tranformation of the chlorophyll data into biomass requires the car-

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bon:chlorophyll ratio. This parameter can be analytical, statistical or dynamical. It has the following dependences: limiting nutrient, temperature and photosynthetic available radiation (PAR). Therefore, it depends in general on the vertical dimension implicitly in the first two parameters, and explicitly in the last one, the PAR. In this ITE the dependence is "forced" when introducing the surface biomass in the system. For the other points we take the result of the Preliminary Phase of MFS, potential maps are composites or weekly averages of the chlorophyll concentrations. Taking the error of the measurement plus the C:Chl (in our case N:Chl) transformation as constant, and supposed unbiased is the first step made by MFSTEP. This simplification should be investigated, and it could be another work (it was in proposal, but was cut) considered in future.

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.

-C- The description of SOFA in terms of the approximate background error covariance matrix is introduced in the second revised paragraph as follows: *This reduced-order optimal interpolation system operates under the condition that the background analysis error matrix is calculated from the background error variances, calculated from the previous analysis error variances, and from the correlations, estimated on the observational data and taken fixed during the simulation. Thus SOFA evaluates directly the horizontal correlations at observation locations, assuming them vertically uncorrelated. The observational error covariance matrix is diagonal and the parameters for these identical twin experiments are given in Table 2.* The same paragraph contains now the definition of the 3D hydrodynamical model.

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?

-C- We did this applications in MFSTEP with homogeneous initial conditions of the inorganic nitrogen given in the third paragraph. These initial conditions are typical of the southern part of the Mediterranean giving a good start to the Eastern Mediterranean. The N of the Western Mediterranean as an integral value comes out as underestimated. P, Z and D are at the same time initialized with summer homogeneous profiles, but these variable have the time to adjust to reasonable beginning winter conditions in the preliminary dynamical adjustment stage. We have no parallel results introducing realistic gradients in the twin experiment. We expect that the evolution should be very similar in the Eastern Mediterranean; instead the results could probably be different in the western basin. In our opinion the anomalies in the total nitrogen should be enhanced, because they occur in the western part even under more oligotrophic conditions.

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. 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 performance. Paragraph starting at line 22. Since N is conserved (no external

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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.

-C- The Fig. 6 is cut in the revised version and relative information is given in Par. 3. The simulations span 9 weekly cycles of assimilation, without the beginning of the tenth one, in order to see the interactions among the variables and not only the phytoplankton change. In this sense the cycles span about three times the higher parameter time scale, that for phytoplankton mortality, which is 20.9 days. The non-convergence is now explained by the Fig. 8a, indicating that there is a residual P rms differences along the free run. This wrong evolution cannot recover by itself the “true” phytoplankton evolution; there is need for SOFA filter action for reducing the rms by about 40% of its free value. A limit in this OSSE is that total nitrogen, basin-averaged and vertically integrated, cannot get close to the control (previous reference) in one year. This is connected to the fact that biomass is corrected by assimilation only at surface layer and the dynamics can propagate the correction to the deeper layers only during winter mixing season, but not when the water column is stratified, in the summer periods. This is another motivation, like that of reducing correlated errors, to consider other multivariate schemes, also ensemble of simulation should be considered, in which correctly addressing at least bivariate assimilation of biomass and nutrients.

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 σ_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

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as a nudging filter, and not really merging 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?

-C- We maintain the three range: where data are assimilated (0-20 m), where not (20 m-bottom), entire basin, for maintaining the possibilities of cross-checking of the results. Entire basin is roughly equivalent to euphotic for phytoplankton and zooplankton: without vertical movements plankton remain mainly in the upper 120 m. The reference numbers of the relative errors are now rescaled to magnify the differences among the different ranges and western versus eastern basins. The parameters of the SOFA used for the assimilation of surface biomass are now introduced in Tab. 2. R1 phytoplankton increases for the assimilation. Then a greater uptake is in effect, and the inorganic nitrogen reaches lower values than the free run. The final effect is that the surface inorganic nitrogen is closer to that in the control (previous reference) run in Fig.. 11, with that relative improvement before day 35.

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.

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-C- The general motivation is that what we see with an average apparent convergence may hidden some relevant differences between the free and the assimilated runs. Now we define the deterioration of the relative errors, when they are greater than one, as follows: *In the upper layer the relative errors decrease at the beginning, but, after four weeks, R1 exhibits increased relative errors and after six assimilation cycles they become greater than one. Thus forecasting deteriorates at last, i. e. rmsAC become greater than rmsFC.* We have evaluated these relative errors in different regions and we see that the effects appear greater going toward the western areas (i. e. Fig 16 a in western subdomains, western, Algerian, northwestern), where greater differences in total nitrogen contents. Thus for our objectives this indicator is robust. The nonperformances of the filter (previous deteriorating effects) happen when the relative error is greater than one. This is the threshold after which the free root mean squared differences is lower than the assimilated one. These anomalies are present in Fig. 13 after forty days. Our conclusion is that western Mediterranean is critical area for this univariate methodology. We are saying this: the detritus is greater in the western basin than in the eastern basin for the specific activity of the zooplankton, which is higher; this produces higher nitrogen export, changing the average values in assimilation (lower) and free (higher), see bottom right box Fig. 15.

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.

-C- The quotas of zooplankton with respect to phytoplankton, as the detritus in the subasins, are reported at the end of the results and not shown in the text. This is writ-

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ten in the revised text. Sloppy feeding and mortality have different time scales. Both generate sinking detritus. See also point 9. for the detritus role in the western basin, which is higher in the assimilated run than in the free run, because of higher phytoplankton biomass. In our opinion results with food web design of the Mediterranean Sea should be obtained using a similar twin experiments approach and compared with this simple NPZD trophic chain outcomes, chosen as OSSE biochemical preliminary candidate inside MFSTEP. We have changed in the revised fourth paragraph this part: *The Eastern Mediterranean can instead be defined as an oligotrophic ecosystem, with scarce energy in the higher trophic levels.*

9. The conclusions do not reflect what shown in the text. There is no clear demonstration 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 sAR/FR increases in both subbasins according to Fig. 16.

-C- Any reference in the revised text to euphotic zone is dropped, because the work considers the effects in a global scale, where assimilation operates, and where the evolution is free (respectively R3, R1, R2). There is practical coincidence between the euphotic zone and R3 for the plankton components, but in this work we do not follow this approximation. Also the influence of the turbulent processes has been cut, because it must be used in another context because the hydrodynamics of the three run is exactly the same. The rms about phytoplankton, Fig. 8a, give us the advantages of this assimilation methodology for reducing the errors. Thus relative errors are maintained as indicators of the overall interpretation. The points in the conclusions regarding total nitrogen are revised in the following form, considering the limits of the method: *The limits of the methodology consist in: inorganic nitrogen error reduced mainly at surface by the surface biomass assimilation and poorly at an overall scale; slight corrections of the basin total nitrogen; anomalies, i. e. nonperformances, in the surface total nitrogen. Even if root mean squared differences are generally smaller in the assimilation run than in the free run, for all the four biochemical variables, inorganic nitrogen exhibits clear improvement only at surface, while in the interior the relative error remains about 0.95. The total nitrogen represents the weak point of this univariate methodology. The basin average shows poor convergence toward the richer "sea-truth" state of the ecosystem. Some limited recovery is attained only after the initial assimilation cycles. The consequence is that in this framework one winter assimilation season cannot recover the deeper layer total nitrogen concentrations.* We rescale the Fig. 16 in the revised version. The western Mediterranean relative errors reach values well higher than one, i. e. root mean squared differences are higher in the assimilated run than in the free one for the upper 20 m total nitrogen. Such behaviour is due to the Western Mediterranean trophic cycling, exporting more detritus from the surface layers (not shown in the text). On the other hand the eastern basin relative errors are for all the simulation under one, indicating that the present univariate approach aids in improving total nitrogen forecasting in such oligotrophic ecosystem. The point is that the R1 detritus in the western basin increases in the assimilated run, with higher organic nitrogen export. Now we rescaled Fig. 16 for clear comparison of the eastern and western relative errors. We also controlled carefully the subbasins results (not shown in the revised text) for all the NPZD variables and then maintain the interpretation of the anomalies.

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

-C- *Southern Balearic (DS4), northern Ionian (DJ7) and Cretan Passage (DH3) areas are selected and averaged, in correspondence to the stations in which phytoplankton data were acquired.*

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

-C- *Atlantic Ocean and marginal seas, Adriatic and Aegean, influences upon the pelagic Mediterranean Sea are treated using historical data (Figure 2) at which these three buffer boxes are relaxed during all the simulations.*

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).

-C- This phrase is cut (see also point P516,l6). The fluxes are in keeping with known estimates of nitrogen fluxes now reported in MPSTEP report. The data are of similar period of the Coste et al. (1988) evaluations of nutrients at Gibraltar.

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

-C- *All over the basin, detritus is set from surface to 100 m depth to the 0.5 μmol N dm-3 initial condition; the value is null beneath. This accords with Coste et al. (1988) particulate matter, as measured in the inflowing Atlantic water.*

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

-C- The control (previous reference) run and the twin experiments are introduced at the end of paragraph 3.

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

-C- *The along-shore currents intensify in these two months in the Gulf of Lions and Tyrrhenian coast; in the same late summer period cyclonic gyres develop or intensify.*

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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-C- All the informations are now in the paragraph 2, commenting Fig. 1, previous Fig. 5.

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

-C- *The FR evolves in a completely different way: starting from low biomass conditions, due to the summer biomass conditions prescribed at the beginning of the run, it shows a quick rise toward higher values, but of the same order, than the CR ones.*

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

-C- It is introduced as reported in the fourth point.

p516,l1 “Apport” has a different meaning.

-C- We have cut it because of the next point.

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.

-C- We have cut these nitrogen flux discussion in the text. We reported these estimates in the MFSTEP Final Report.

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.

-C- *According with Raicich and Rampazzo (2003), it is possible to evaluate in an OSSE experiment, the performance of the assimilation filter with respect to the free run by means of relative errors.*

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.

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-C- *The better performance occurs in the 0-20 m evolution.*

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

-C- *Zooplankton relative errors remain the same in all the three chosen spatial partitions, approximately 0.7 at the end of the integration period.*

p518,l8 Sentence not clear.Please reformulate.

-C- *The total nitrogen performance worsens in the case of this few weeks biomass univariate assimilation, i. e. the forecasting is not improved and it deteriorates at last.*

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.

-C- This expression is cut in the discussion about the western basin relative errors.

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