Ocean Sci. Discuss., 3, S52–S56, 2006
www.ocean-sci-discuss.net/3/S52/2006/

Ocean Science
Discussions

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

Anonymous Referee #2

Received and published: 22 June 2006

Review of "Reduced-order optimal interpolation for biomass assimilation"
by G. Crispi, M. Pacciaroni and D. Viezzoli

A) General comments

This paper presents results of twin assimilation experiments, that have been performed with a 3D coupled circulation/ecosystem model of the Mediterranean Sea (1/8 degree horizontal resolution). Simulated surface biomass data are computed from weekly averages of a reference simulation between January and March 1998. These data are assimilated in a model run differing from the reference simulation by a perturbation in the initial condition for the phytoplankton concentration, for which a summer concentration is used instead of the true initial condition. The results show that, except for the total nitrogen concentration, the error is significantly lower with assimilation, than without assimilation of biomass data. Tentative explanations of the results for the total nitrogen concentrations are also given.

The problem of the controllability of ecosystem models by the available surface biomass observations is undoubtfully an important question which has received no satisfactory answer yet. Investigations on that subject are thus of valuable interest to the operational oceanographic community. (As stated by the author: “The aim of this work is to evaluate the feasibility, the efficiency and the limits of the assimilation of superficial biomass data in view of possible activities in operational oceanography.”)

However, the paper fails to provide a significant contribution to the subject mainly because key information is missing (see specific comments below for more details): (i) the link between the general objective stated above and the experiment that is performed is not explained in the paper; there is no explanation or justification for the experimental protocol that is used to investigate the question; (ii) there is no description of the assimilation scheme (except unimportant technical details); in particular, there is no information about the statistical parameterization of the assimilation scheme (which is stated to be reduced-order optimal interpolation only in the title of the paper and in the conclusions); (iii) the text is often unclear, confusing and not properly organized. Hence the paper requires considerable improvements before it can be accepted for publication.

B) Specific comments

1) The link between the general objective of the paper and the experiment that is performed is not explained. Why do the author apply a perturbation on the phytoplankton

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initial condition? Is it the dominant source of error in the system? Why controlling initial condition errors and not modelling or forcing errors? Why applying a perturbation on the phytoplankton and not on the other state variables? To what extent will the conclusions hold in realistic experiments, in the presence of other sources of error? Why is it an appropriate preliminary step? One can doubt that the answers to these questions will always support the choice of the experimental protocol. It is thus necessary that the authors explain their choice and state to what partial conclusions that kind of experiment can lead (and also on what matters no conclusions can be drawn). Without such explanations, the experimental protocol appears oversimplistic to investigate the general objectives stated in the introduction, and the conclusions of the paper remain almost meaningless.

2) Without a minimal description of the assimilation scheme, it is impossible for the reader to understand, to interpret, and even less to reproduce the results. Maybe adding a few lines (in addition to the reference that is given) would be sufficient to remind the general features of the assimilation method. But the statistical parameterization is specific to this work and must be fully documented in the paper. How is the observation error covariance parameterized? How is the background error covariance parameterized? I assume that the statement that the scheme is univariate means that surface biomass data are used to update only the phytoplancton variable, but over the whole water column. In order to do that you need to provide vertical error covariance for the phytoplankton? How is it parameterized? What is the phytoplankton background error horizontal correlation structure? On what grounds did you set the values of these parameters? In order to answer these questions, a new section describing the assimilation scheme is needed.

3) Only relative errors of the assimilation run with respect to the free run are presented. I think it would be important for the reader to have an idea of the amplitude of the error for each variable. It would also help a lot the interpretation of the results. For instance, since the perturbation is only on the initial condition, it may be that, for some variables,

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the system is relaxing by itself close to the true trajectory. (Fig 7a shows that such relaxation to the truth without assimilation occurs at least for the basin average of the phytoplankton concentration.) In such a situation, it may be not very useful to know that the relative errir is asymptotically becoming 30% lower in the assimilation run, if the amplitude of the error in the free run is already becoming very small. Generally speaking, I believe that the presentation and interpretation of the results should be improved and clarified.

C) Technical corrections

1) The title is not appropriate, because it leads the reader to think that there are new developments in the assimilation scheme.

2) The abstract should be clarified and made more informative of the content of the paper.

3) The introduction discusses of problems that are not useful to understand the paper, but fails to introduce the true subject of the paper, which is the setup and the interpretation of twin assimilation experiments.

4) I think that speaking of OSSE (Observing System Simulation Experiment) is not really appropriate here because one single observation system is being tested. Saying that “the OSSE proposed here provides the quantitative basis for a rational design of subsurface observing systems” is incorrect since only surface data (or near surface data) are assimilated.

5) The name of the ocean GCM is not given.

6) Use one letter, not two, to describe a mathematical quantity (BT for biological tracer is not appropriate), because it can be confused with multiplication.

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7) The paper provides technical details that are not useful here (computer trademark, number of nodes, memory size, CPU power, names of computation libraries, introduction of O2 optimization for the compilation).

8) Very often, new concepts or acronyms are mentionned before being defined or introduced. This gives the feeling that the paper has been built from pieces of text coming from elsewhere, that were put together without sufficient checking. For instance, what are areas DS4, DJ7, DH3 that are mentionned in the second paragraph of section 3. They are not defined, and not used anywhere else. In the same section, the concept of "preconditionning period" is used before defined. Since the description of this "preconditionning period" is the purpose of section 3, it is only at the end of the reading of this section that the reader knows what it is about. I insist that these are only a few examples. The text should be carefully checked and better organized.

9) In the result section there is a discussion on technical aspects of data preprocessing, which are not useful. It is even misleading because it mentions "profile data" several times, giving the impression that you are assimilating profile data (even if it is stated a few lines above that that biomass data characterizing the 2 upper levels are assimilated). I understand that it is only the technical way by which the data are processed but this makes the text very unclear. That discussion must be dropped.

10) I do not understand the sentence: "AR zooplankton behaviour is very close to the free one, even if only phytoplankton biomass is assimilated in our experiment."

11) There is a discussion beginning at the end of page 515, continuing on page 516, which is difficult to relate to the results that are presented. Please explain the relation or drop the discussion.

12) The conclusions should certainly be modified according to the first of the specific comments above.

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