Ocean Sci. Discuss., 9, C178–C182, 2012
www.ocean-sci-discuss.net/9/C178/2012/

Ocean Science
Discussions

Interactive comment on "Assimilating GlobColour ocean colour data into a pre-operational physical-biogeochemical model" *by* D. A. Ford et al.

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Received and published: 17 April 2012

General comment

I read with interest the paper by Ford et al. They provide valuable advances in implementing an operational biogeochemical forecast system using a promising biogeochemical data assimilation scheme and they show a big (and uncommon) effort for the valuation of the results. I would like to contribute to the discussion on the manuscript with general comments on the validation and some minor comments on the method description. Few minor corrections are listed at the end.

Validation

1. Validation temporal and spatial scales.

   The authors very well define the guidelines applied for the assimilation results validation. The assimilation needs to be validated from three points of view: the comparison with assimilated data, the comparison with independent chlorophyll data, and the evaluation of the effect on the other biogeochemical variables. The assimilation should increase the model performance on chlorophyll forecast and at least not degrade the other variables.

   The validation method is well-founded and consistent with the aims of the work (and generally speaking with the aims of the biogeochemical assimilation). I wonder whether the proposed approach can be extend to the validation at finer temporal and spatial scales, in order to highlight the assimilation effects at scales comparable with those of relevant oceanic biogeochemical processes and also to the typical short term scales of the operational forecast framework. For instance, Fig. 4 provides an annual mean of surface chlorophyll concentration, showing promising results for the Assim run. This "annual mean information" could be enriched providing the results at a smaller temporal scales, at least at the seasonal scale (in the text the improvement of the seasonal signal in the Assim run is deduced from statistics but not further investigated, p.706) and for regions of particular interest (for example some of the basins cited in Fig. 6 or in the Brazil-Malvinas confluence, cited in the text as a region where the Control run show particular drawbacks, p.705). The addition of results at a smaller spatial scales could be useful also for Fig. 5. The time series of the statistics can be shown here also for regions of particular interests. Statistics on too much aggregate data might hide important information on how assimilation has improved Control run.

2. Assimilation impact on Biogeochemical dynamics.

Evaluation of the impact of assimilation on the variables other than chlorophyll is a difficult task, also because of the scarcity of the available data. As stated by the authors, the assimilation should not degrade the results, however I would suggest that relevant biogeochemical dynamics and patterns should be preserved, too. For instance, Fig. 8 shows the model variables at a fixed time, while it could be interesting to have a time varying view of the model variables evolution in the Control and in the Assim run, showing for example a z,t diagram for a relevant location (where data are available or biogeochemical dynamics are already documented, for example at the ALOHA site used for Table 2 statistics or other suitable mooring sites, http://www.oceansites.org). Further, it would be very interesting to discuss the assimilation results comparing them also with relevant ocean biogeochemical dynamics and processes (for example, primary production in different regions).

3. Evaluation of the forecast.

   In Fig. 5 one-day forecast statistics are shown. Since the work presented by the authors has been realized with the aim to analyze the feasibility of an operational forecasting system, it could be relevant to show the same statistics also for forecast at a longer scale. For example, the statistics could be evaluated for the two-, three-, four-, five-, and six-days forecasts (being the last the forecast length used in the FOAM system, p.693), and the difference with respects to the one-day forecast results could be discussed.

Method

1. OPS quality control (Sect. 4.1).

   It is not specified how much the difference between the observations and the background should be larger than the sum of the background and observation

   error variance in order to reject the observation (the “too large” words may be specified). Further, it is not clear to me why, in the OPS procedure, the background error covariances are those obtained as described in Sect. 5.2, while for the observations the results of the Sect. 5.2 method are not used here (and instead the GlobColour value is used).

2. 3D increments for the model variables from the surface chlorophyll increment (Sect. 4.2).

   The method is shortly presented (referring to the work of Hemmings et al. 2008). Even if the parameterization used is largely discussed in Hemmings et al. (2008), it could be briefly summarized in the text, or it could be said that it is the same used in the cited work. Moreover, it could be interesting to know how the MLD (which is a key factor in the method proposed by Hemmings et al. 2008) is evaluated in the present implementation.

3. Definition of the error covariance matrixes (Sect. 5.2).

   Two methods are used and combined: the Hollingsworth-Lonnberg and the NMC methods. Is it correct that the first one provides the evaluation of the error covariance matrix both for model and observations? If yes, it should be useful to state this issue it in the text. Furthermore, more details on the two methods combination can be provided (or a reference can be cited, if the approach has been already applied). Further, authors could discuss and comment the impact on the assimilation results due to the new combined method.

4. Correlation length scales (Sect. 5.2).

   The authors could discuss why the correlation length scales for chlorophyll are the same of the SST assimilation, and also if other length scales have been tested.

Minor points in the text

1. In Equation 1 correct y instead of x for observations.
2. In Fig. 5 why the same statistic provided for observations (mean absolute error) has not been evaluated for the model runs (mean absolute error vs observations instead of RMS vs. observations)? It should be helpful for the plot readiness to have uniform statistics.
3. The unit length of the Fig. 8 colourbar must be specified. Moreover, it seems that titles are not correctly aligned with plots.

Interactive comment on Ocean Sci. Discuss., 9, 687, 2012.