Ocean Sci. Discuss., 9, C501–C511, 2012
www.ocean-sci-discuss.net/9/C501/2012/

Ocean Science
Discussions

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

D. A. Ford et al.

david.ford@metoffice.gov.uk

Received and published: 8 June 2012

Thank you very much for your comments and for contributing to the open discussion. We will address your points in turn, quoting your comments in italics.

Regarding your suggestions for further validation, these are all very useful and interesting things to investigate, and we will comment on each one below. However you acknowledge that we *"show a big (and uncommon) effort for the valuation of the results"*, and as such the paper is already quite long. We are therefore reluctant to expand the amount of validation presented in the paper, which currently gives a general but thorough overview of the results. Instead, we plan to write another paper, which will look in detail at the performance of the scheme in specific locations, and its impact on different processes, including exactly the sort of validation you suggest. We will use your comments to inform the writing of this.

"Validation

"1. Validation temporal and spatial scales.

"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."

We agree that this would be very interesting, although we believe that the current validation is sufficient to demonstrate that the assimilation has a desirable and useful impact. We have performed some validation at finer scales, and found the conclusions to be generally similar to those presented here. As said, we plan to address this in more detail in a future publication.

"2. Assimilation impact on Biogeochemical dynamics.

"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)."

We agree that relevant biogeochemical patterns and dynamics should be preserved, and this is one of the aims of the nitrogen balancing scheme. In the validation that has been performed this is generally seen, at least in the phytoplankton, zooplankton and detritus fields. Figure 8 is intended to help demonstrate this, and the annual mean surface zooplankton fields we show in the response to the first anonymous reviewer (and which we plan to include in the paper) help demonstrate this further. A z,t diagram would definitely be informative, and we hope to investigate this in the future. Comparing this to observations is hindered by the lack of data available to us for 2008, so we may choose to assess different years in future. The ALOHA observations used in Table 2 are only once a month (approximately), so are unfortunately not particularly suited to this purpose.

“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).”

Agreed, and we hope to extend the work to do this in the future.

“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.”

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

We agree that this is a very interesting issue to address, but could be a whole new study, requiring the validation of considerably more model output. As such, we believe this to be outside the scope of the present paper, with analysis skill needing to be established before it is worthwhile considering forecasts.

“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).”

The description we have given is slightly simplified, and we will be more exact. The method used is described in Sect. 4.2 of Ingleby and Huddleston (2007), specifically their Eq. 1:

$$P(G|O) = \kappa P(G)/(\kappa P(G) + (2\pi V)^{-0.5} exp(-(o-b)^2/2V)(1-P(G)))$$

This calculates a probability of gross error (PGE) given the background value. κ is the density of the probability distribution of gross error due to instrument error, and is set here to be 0.1. P(G) is the PGE due to instrument error, and is set here to be 0.04. V is the sum of the background and observation error variances, o is the observation value and b is the background value. If P(G|O) is greater than a specified threshold, set here to be 0.5, then the observation is rejected.

“Further, it is not clear to me why, in the OPS procedure, the background error

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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).”

It was chosen to use the GlobColour error values in the quality control in order to benefit from the exact error information for each individual observation. However we accept that there are also reasons for using the assimilation observation error variances (consistency, inclusion of representativeness error), and this is something that could be investigated in the future. We will discuss this point in the revised manuscript.

“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.”

The parameterization used here is identical to that used in Hemmings et al. (2008). We will state this in the paper, and also expand our description of the method.

“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.”

The MLD is used in exactly the same way as in Hemmings et al. (2008). The MLD field has been validated in the same manner as for Table 1 of Storkey et al. (2010), and the errors found to be broadly similar. In this study the global RMS error was 34.6 m, and the mean (observation minus background) error was 0.7 m.

“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).”

We agree that the description of the method given is too brief, and will expand the text accordingly. The NMC method provides estimates of the two (mesoscale and synoptic scale) components of the background error covariances. Estimates are calculated at every grid point, as is required. The Hollingsworth-Lönnberg method provides error estimates of the observation error covariances, as well as the two components of the background error covariances. However these can only be calculated at grid points where observations are available, so complete fields cannot be obtained. Therefore the NMC estimates are used as a basis for the error covariances, giving the spatial patterns in the variances. To give a complete field, and to ensure consistency with the background errors, the mesoscale error variances from the NMC method are used as a basis for the observation error variances. Each of the three (mesoscale, synoptic scale, observation) sets of error variances are then scaled so that their global mean is identical to the global mean of the corresponding Hollingsworth-Lönnberg estimates. In short, the NMC method is used to provide the spatial patterns, whilst the Hollingsworth-Lönnberg method is used to provide the magnitudes.

“Further, authors could discuss and comment the impact on the assimilation results due to the new combined method.”

The global RMS (observation minus background) error for $\log_{10}$(chlorophyll), as stated in the text, was 0.586 $\log_{10}$(mg m^{-3}) for Control and 0.314 $\log_{10}$(mg m^{-3}) for Assim. For the hindcast used to calculate the error covariances, which used the variance of the GlobColour products for error covariances, the RMS error was 0.439 $\log_{10}$(mg m^{-3}). There is a clear improvement given by using error covariances designed specifically for use with the FOAM-HadOCC system, and we will comment on this in the text. Future improvements could potentially be achieved through further refinement of the error covariance estimates.

“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.”

Estimates for the length scales are given as part of the NMC error covariance calculations. These were found to be broadly similar to those for SST for the synoptic scale, and a bit shorter for the mesoscale. However given the relatively coarse (1°) resolution of the model, it is undesirable to set the mesoscale length scale to be much shorter than 100 km (noting that “mesoscale” refers here to small-scale processes resolved by the model, rather than to the actual ocean mesoscale). Therefore the length scales were set to be equal to those for SST, in part due to these results, and in part for consistency and convenience. No other length scales have been tested, but future improvements could potentially be achieved through tuning of these. We will note this in the manuscript.

"Minor points in the text

"1. In Equation 1 correct y instead of x for observations."

Thank you for pointing this out, we will change it accordingly.

"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."

Mean absolute error (MAE) has not been provided for the model, as this adds little information to the RMS error and mean error (bias). The reason MAE is shown for the observations is because the bias cannot be calculated from the information available. MAE is therefore given instead, as this is the maximum possible value of the bias, and can therefore be considered analogous to an error bar. However we accept that this presentation is inconsistent and could be confusing. One solution is to also provide the MAE for the model, and this plot is shown in Fig. AC2 below.

However we feel that this does not add much information about the results, and makes the plot overly cluttered. Therefore we propose to instead remove the observation MAE from the plot, so that only the RMS error is shown for the observations, as shown in Fig. AC3 below.

"3. The unit length of the Fig. 8 colourbar must be specified. Moreover, it seems that titles are not correctly aligned with plots."

We agree that this figure needs tidying up, and will change it accordingly.

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

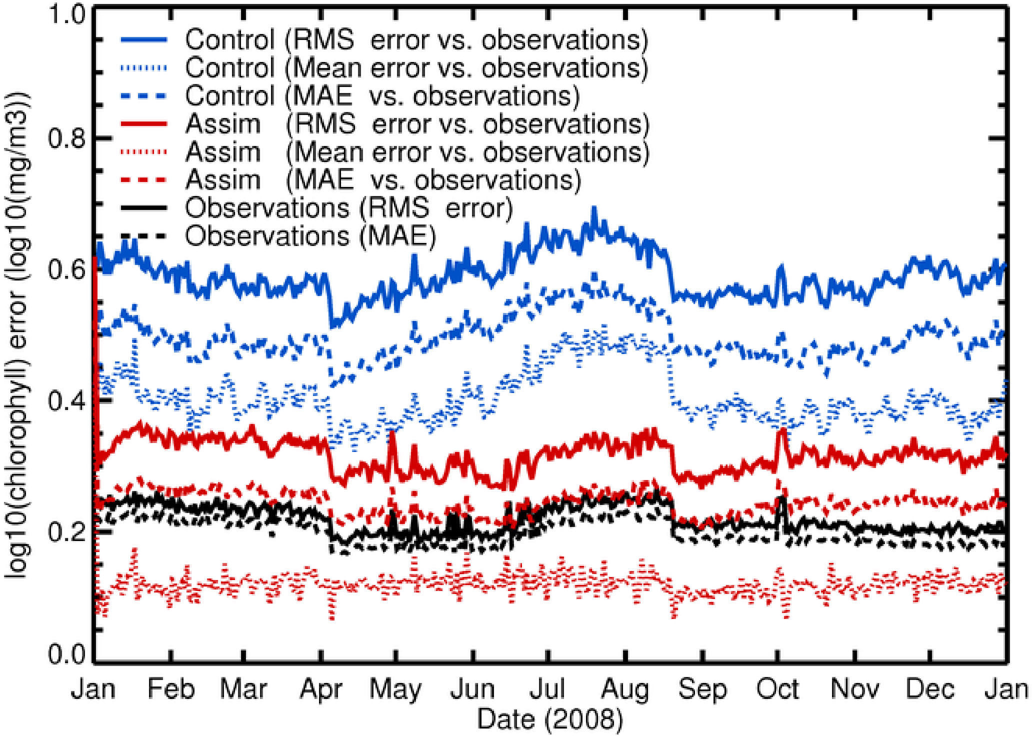

Fig. AC. 2. As Fig. 5

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

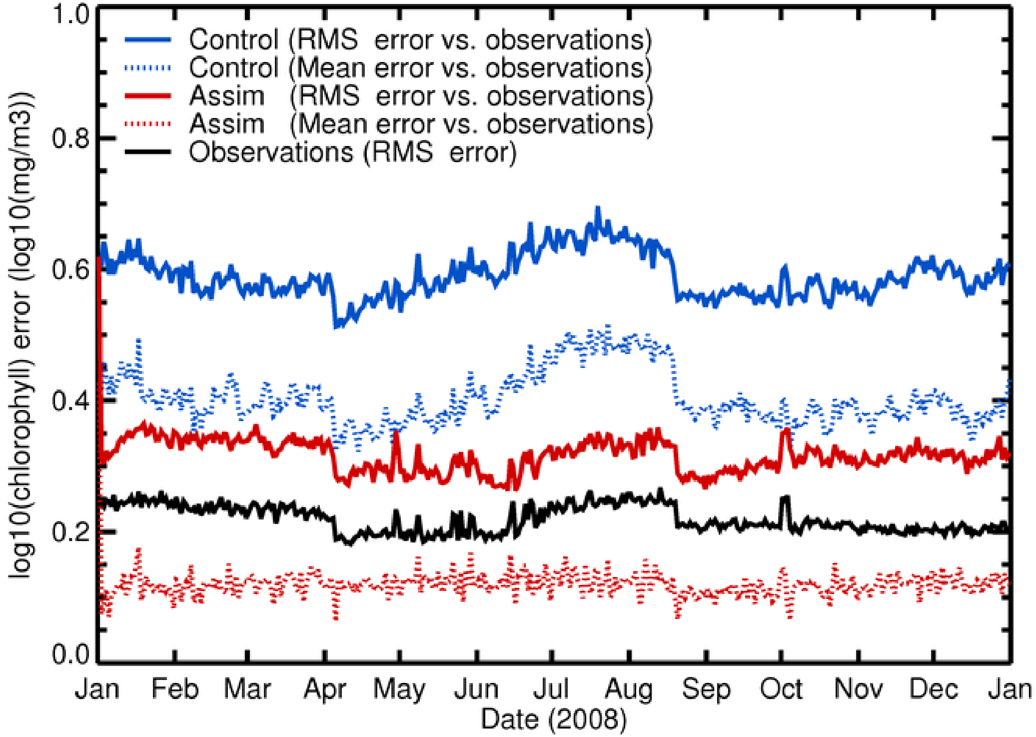

Fig. AC. 3. As Fig. 5

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper