Ocean Sci. Discuss., 9, C238–C245, 2012
www.ocean-sci-discuss.net/9/C238/2012/

Ocean Science Discussions

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

Anonymous Referee #2

Received and published: 1 May 2012

General comments.

The Discussion Paper "Assimilating GlobColour ocean colour data into a pre-operational physical biogeochemical model" presents the important work by Ford and colleagues on the development and test of a novel, global pre-operational data assimilation (DA) system for GlobColour chlorophyll product. The objectives of the work are: 1) the evaluation of the "operational applicability" of the DA system, and 2) the assessment of the DA impact in the model's representation of chlorophyll, carbon cycle and other model variables. The DA system uses a physical-biogeochemical model (FOAM-HadOCC) and an Optimal Interpolation DA scheme that is combined with the

nitrogen balancing procedure by Hemmings and colleagues. The system was tested in a one year long assimilation of daily products in 2008. The DA output was evaluated with respect to the control simulation (i.e. without biogeochemical DA), using a huge set of skill statistics and the datasets of assimilated and (semi)independent chlorophyll data, as well as nitrogen, phyto and zooplankton, DIC, Alkalinity, and pCO2 observations. The paper is well written and clear in most of its parts. The methods are clearly presented, despite some improvements are needed. The DA system and the results are surely of great interest to the DA community, showing that the 1-day forecast of chlorophyll was improved at regional and global scales. In addition, the hindcast of other variables was not deteriorated and in some cases it was improved, which is not an obvious result in biogeochemical DA. The conclusions are sustained by the results and discussion, despite more objective remarks on the limits of the work are preferable.

However, I think that the power of the skill assessment presented in the paper is affected by the low number of independent field data used by the authors. This is relevant, because of the objective #2 of the paper. More data should be exploited. For example, Nitrate data were considered at just one area and two months (SeaBASS data measured in North-Atlantic, in April and May), which is a rather small dataset to assess the skill of a global model. For example, why AMT data of nitrate were not used (but the authors used AMT data of Chlorophyll only)? A larger comparison with nitrate in situ data is relevant because the comparison with the nitrate climatology (Fig. 9) does not show noticeable impact of DA on the model estimates of the annual mean of surface nitrate (and DIC and alkalinity, as acknowledged by the authors). Moreover, the authors should discuss what DA taught them on the deficiencies of the biogeochemical model and tell the reader which are the possible future improvements of the model structure and/or parameterization (e.g. introducing plankton functional types, modelling variable carbon:nutrient ratios?). This discussion would help future progresses in biogeochemical DA.

The authors should address the following specific and technical comments in the paper.

Specific comments

Section 4.1 The problem of Gaussianity in biogeochemical DA is well known and the log-transformation is a common, pragmatic procedure. However, the log-transformation does not guarantee Gaussian distributions of the assimilated chlorophyll data (that is different from what stated by the authors at p 688, l16). To this regard "anamorphic functions" or similar approaches would be preferable (see e.g. Simon and Bertino, Ocean Science Discussions, 2009, Lenartz et al., Journal of Marine Systems, 2007; Brankart et al., Ocean Sci. Discuss, 2011). Please comment the possible implications of using non-Gaussian distributions in the assimilation scheme. Moreover, log-normal assumption and transformation are typically used in chlorophyll DA (e.g. Bertino et al., International Statistical Review, 2003; Torres et al. , Journal of Marine Systems, 2006; Nerger and Gregg, Journal of Marine Systems, 2007; Gregg, Journal of Marine Systems, 2008; Ciavatta et al., J. Geophy. Res., 2011). Why do the authors use log10?

Section 4.2 I do recognize that the "nitrogen balancing scheme" is extensively described by Hemmings et al. (2008). However, some more lines on the method would help the reader, e.g. to understand the relevance of the growth rate and loss rate that the authors mention in Fig.8.

Section 5.1 If I am not wrong, the authors: 1) apply FOAM-HadOCC (with physical DA) in a 2-year run and they found that the Nitrogen field is relatively poor, 2) thus they replace it with a climatological Nitrogen field and 3) they run a one-year spin-up. What is the utility of the 2-year run if the authors replace the nitrogen field? Does this replacement introduce inconsistency with respect to the other model variables? Is it sufficient a 1-year long spin-up to solve this inconsistency?

Section 5.2 I think that Section 5.2 is rather unclear. The observational and background error are highly relevant in an assimilation scheme. Thus, the authors should clarify several points in this section. Firstly. In section 3.2, the authors had provided a detailed description of the accuracy of the GlobColour products. Are those ones the errors

applied in the assimilation run, or they use observational errors computed in Section 5.2? Secondly. The authors compute initial errors from the GlobColour data on a 1° grid for the period 1998-2007. This error is then assigned "in equal proportions" (what does it mean precisely?) to the assimilated variables as well as to the background. I do not see the point of defining the background (model) error in relation to the error of the GlobColour data. Could the authors please explain this point? Finally. The authors use the same correlation length for chlorophyll and SST (100 Km for the mesoscale and 400 Km for the synoptic scales). However, phytoplankton patchiness leads often to relevant changes in chlorophyll on a much shorter scale. Please comment the choice of the chlorophyll correlation length.

P703, l17-23. The authors should consider to re-rewrite this part more clearly (e.g. l18: "verifying" what?). The National Meteorological Centre (please specify the acronym "NMC" in the text) and the Hollingsworth-Lonnberg method provide error estimates of the background error, of the observational error, of both of them? At the end of the day, which is the range of the background and chlorophyll errors applied in the assimilation run?

Figure 4. Please provide a quantitative comparison between reference vs satellite and assimilated versus satellite data, to help the reader in appreciating the improvements. For example, maps of mean percentage differences between outputs and satellite could be helpful.

Figure 5. The mean global bias (MGB) of the outputs and the mean absolute error (MAE) of the observations are not directly comparable (as acknowledged by the authors). MGB could be lower than the MAE because positive and negative errors compensate when averaging: is this the case? I think that the authors should plot MAE for the model outputs as well, to facilitate the skill assessment. The error statistics are computed using the data at latitudes > 60°? (Probably these data could be excluded from the computation, since they were not assimilated. This should lead to a further improvement of the DA error statistics).

Validation of the DA results vs. SeaBASS chlorophyll data. Are the SeaBASS chlorophyll data (or part of them) used to calibrate the assimilated GlobColour product (p 707, l29)? In this case, the DA output cannot be considered fully independent from the SeaBASS data used in the DA skill assessment. The dependency could be low, but the authors should address this point.

Table 1. The authors should clearly state that the GlobColour product does a better job than assimilation in estimating the SeaBASS observations of log10 (chlorophyll) at the surface. It is true that the normalised standard deviation of assim (0.868) is closer to 1 than climatology (0.597) in Table 1 (but note that control does a better job: 1.065). However, all the other statistics of climatology are noticeably better than assim. Please mention that assim leads to just a "slight" improvement in bias of the chlorophyll beneath the surface, with respect to the control (i.e. -1.3%). I think that the above statement on the better skill of climatology and mentioning "slight" improvements do not diminish the value of the work. Improvements are not obvious at all in biogeochemical DA. However, the authors should mention which are the further improvements in the structure of the biogeochemical model that could lead to better result.

Tab 2-6. Why the authors do not show the value of MPE, as they did in Tab 1.

Figure 8. I recommend the authors to replace the figure with vertical profiles where field data are available for skill assessment. It is not relevant if the data does not cover the whole set of model variables. The AMT transects of chlorophyll and nitrate, or at least punctual profiles (HOT?, BATS?) could be helpful. As it is, Fig. 8 is a general discussion on the features of the "nitrogen balancing scheme" by Hemmings et al. (2008). However, the short description of the method given in this paper makes rather difficult the understanding of the discussion. This discussion seems not necessary with respect to the objectives of the paper. Moreover, the Figure has some problems with the headings, the variable units are not specified, and I do not see the point of showing the increments. These are computed as a difference between the background and the analysis (eq. 1). Thus they are not very helpful when comparing analysis and control.

Finally, in Fig. 8 is quite evident that control and assim are quite similar in nutrients, DIC and alkalinity (if I do interpret correctly the shift in the headings). This low impact of chlorophyll DA on the model variables is reflected in the negligible DA changes in the global annual mean fields shown in Figure 9. What is the authors' comment on this low impact of chlorophyll DA on the model variables? How does "the low impact" reconcile with the DA improvement of the nitrate estimates shown in Figure 10?

Tab 4. The authors should clearly state that climatology does a better job (not just "slightly") than assimilation with respect to all the skill statistics (e.g. correlation +0.3).

Tab 6. The authors should point out that the normalized standard deviation of assim is worse than the control one.

Technical corrections (please consider "please" as implicit)

p 688, l 7 : delete "significantly": statistical tests to assess the statistical significance were not presented

p 688, l 10: not in every ocean basin (e.g. not in the Arctic Ocean)

P693, l11: specify that the FOAM system you're using is non-operational but it still assimilates physics.

P696, l8: can the authors use the data of the "day after" in an operational system?

p 697, 10: Just a curiosity: Why Seawifs error = 35.77%, i.e. it is higher than 25.96 % (p 697, l 1)? Operational errors are higher than non-operational errors?

p 697 l. 28 – p 698 l 7: the "Discussion" is a better place for these comments

p 698 , l 20 : "error" greater than 50?

p 698 , l 23 (and subsequent): could the authors use a word different than "background", to avoid any ambiguity with the assimilation background?

p 700, eq. 1 : replace x with y to indicate the observation vector

P701, l10: briefly summarize what “incremental analysis update” does.

p 701, l 18-19 versus l 24-25 : is sea ice concentration assimilated in the two-year hindcast?

p 703, l 16 define the acronym NMC (National Meteorological Center ?)

p 704, l 15: “dotted” or “dashed”?

p 706, l 23 : is the “single day” the first analysis (1st January)?

p 707, l 3: it is not “curios”, but relevant. It indicates that the skill statistics are influenced by irregularities in the globcolour data.

p 707, l 18 : “in general” (or similar) is better than “universally”

p 710, l 5-6: “all aspects of the model” would include the model structure and parameterization. Replace with “the other variables”. Anyway, not all the variables are improved: see fig 9.

Figure 8 (please consider replacing the figure: see the above “specific comments”). It is not useful to comment the discussion of this figure since I am not sure about the figure headings.

p 714 , l 24: mention that climatology does a better job than assim in hindcasting the in situ chlorophyll.

p 714, l 26: “consistent” is slightly ambiguous here. It is true that the assimilation scheme changed the other variables consistently with the changes in chlorophyll. But it is also true that these changes were rather small (in magnitude): compare control and assim in fig 8 and 9.

p 714, l 27 please mention that few data were used for the DA skill assessment (e.g. nitrate at one area, at 2 months).

p716, l10 : “considerably” holds strictly just for chlorophyll, not for the other model

variables

p716, l17 : “considerably” holds strictly just for chlorophyll, not for the other model variables

p 716, l 17-19: which are the possible improvements in the biogeochemical model?

p716, l28 : what does it mean that the “consistency” was improved?

p 717, l1 : “physical assimilation” was not integrated with the chlorophyll assimilation in this work?

Tables 2 & 3 : merge the tables in a single one

Tables 4-6: merge the tables in a single one

Fig 3: it is log10 (Chl) (not “log(Chl)”)

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