Ocean Sci. Discuss., 9, C786–C795, 2012
www.ocean-sci-discuss.net/9/C786/2012/

Ocean Science
Discussions

Interactive comment on "Toward a multivariate reanalysis of the North Atlantic ocean biogeochemistry during 1998–2006 based on the assimilation of SeaWiFS chlorophyll data" *by* C. Fontana et al.

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Received and published: 27 July 2012

General comment: In particular, one of the stated aims is to "identify the best possible implementation of a multivariate, ocean color assimilative system based on state-of-the-art methods", but this aim is not really addressed.

The sentence was changed (see also answer to reviewer #1)

The conclusion is reached that the use of anamorphic transformations is better than the "linear" implementation of the SEEK filter. However there is no discussion of how

upgrading the simplified SEEK filter implementation may improve results, or the advantages/disadvantages of this approach compared to other multivariate approaches. Some discussion of this should be added to the "Conclusions and perspectives" section.

A paragraph discussing this point was added by the end of "Conclusion and perspectives" section.

This can be put in the context of limitations of the method implemented here, such as the use of the free run variability to specify the error covariances. As acknowledged, this means that only very small increments are applied in regions where there is little model variability, even if the model-data mis-match is high.

Yes, it is true.

Another point is the data sets used for validation. No independent chlorophyll data are compared against, only the assimilated data (although comparison is made to forecasts as well as analyses).

We did not use in situ chlorophyll data as satellite-estimated concentrations present satisfying temporal and spatial coverage (even though it is linked to the assimilation process). Also, improvement of chlorophyll description by assimilation of satellite chlorophyll data was already demonstrate in the literature.

The only other variable considered is nitrate, with comparison made to WOA data (about which there may be quality control issues, and perhaps some dependency between model and data, see below). It is demonstrated that the assimilation, with the use of anamorphic transformations, improves model skill compared to these data sets, which is an important result, especially given that nutrients are rarely shown to be improved by biogeochemical data assimilation. However much more validation is required before confidence can be placed in a "data-driven climatology" produced using this method. Given the aims of the paper, and the availability of data, this validation

is not required here, but the point should be noted when discussing the future of this approach.

It is now discussed in the Conclusion section.

Specific comments:

p1891 l2&6: Please add a brief definition (or reference) of sequential and variational in this context.

A good definition is given in Gregg et al., 2009 already cited in the sentence "A comprehensive review of biological data assimilation experiments, both sequential and variational, can be found in Gregg et al. (2009). " p1891 l5

p1893 l16: Please expand on the implementation of the “buffer zones”. Furthermore, please state how the boundary conditions are implemented for LOBSTER.

In "buffer zone", the physical model is relaxed toward a climatology. The implementation of a buffer zone depends on a relaxation time and a width. Generally, the relaxation time is spatially variable: short close to the model grid boundary (some days) while it is longer further away (some months). Reference was added to Tréguier et al. (2001).The LOBSTER model considers closed boundaries. Text added.

p1896 l25-29: I appreciate that anamorphic transformations are detailed elsewhere. However it would aid understanding to expand the description given here.

See answers to reviewer #1. A more detailed description of the anamorphic transformation was added.

p1897 l17-21: A two-year spin-up seems rather short given that nitrate is initialised from the same data set that much of the validation is performed against. Please comment on this.

It can seem short but the phytoplankton cycles show no important drift on an interranual time scale, so we guess that the model reached a stable state.

Does model skill compared to the WOA nitrate data remain steady throughout the nine-year reanalysis period, or does skill change later in the period, suggesting that the model nitrate is still spinning-up? If so, is the assimilation able to correct for this?

We did not study the model skill evolution with respect to time. Anyway, a major part of the nitrate data set concern the first years of simulation (see answers to reviewer #1)

p1897 l25: Which day of the eight-day binning period are the chlorophyll maps assimilated on?

The last one, in order to stay in an operational frame where future observations are not known. A sentence was added about this.

Are the maps at the model resolution, or higher resolution?

Satellite data are interpolated onto the model grid.

p1898 l27: Is any consideration made of representativity error (the fact that the model cannot resolve high resolution processes which affect the observations),

We did not compute any representativity error. It is assumed to be contained in the observation error (representativity error is intrinsically contained in satellite/in situ comparisons).

or the error introduced by using a modelled Chl/N ratio to convert chlorophyll to phytoplankton?

No

Underestimating the observation error could lead to giving too much weight to the observations.

Yes, it will actually overfit data, which may results in unrealistic response of the model. It seems not be a major issue with our simulations. This is discussed in: Oke, P., Sakov, P., 2007Âă: Representation Error of Oceanic Observations for Data Assimilation, J.

p1899 l18-21. Independent data are definitely required. Whilst I think it is perhaps sufficient for this paper, I don't think using a single data set for a single variable is enough to be "totally conclusive" about method efficiency. This is especially relevant given that the model was initialised from WOA data, and the error covariances are based on the model free run, and so there may be some dependency. Please comment on this.

Yes it is right. Unfortunately we do not dispose currently from sufficiently extended data sets for other variables to perform a valuable comparison. Nevertheless we agree that it can't be considered as "totally conclusive". There are dependencies between the model initialization and the validation data set. It could be an issue if we only compared the free run to nitrate data from which it was initialized. But, in the frame of this assimilation experiment, there are no dependencies between the assimilated data set and the validation data.

p1900 l17-19: I assume this specifically refers to the area just north of the elongated structure? Generally, chlorophyll is higher in the free run than the observations above 45° N in May/June.

Yes but in this sentence, we comment the March-April period. For the May-June period, the sentence is: "The available nutrients are rapidly consumed (Fig. 3, third and fourth rows, i.e. May– August) inducing a strong increase of the chlorophyll concentration. During the peak of the chlorophyll bloom in the free run, concentrations seem to be overestimated at high latitudes ", which is in agreement with the reviewer comment.

p1900 l25: It seems to me that the SeaWiFS data exhibit larger values beyond the summer season too.

Yes but ones must be aware of the logarithmic colorbar. The difference is not so important (blue/greem $\sim$ 0,2-0,3 mmol.m-3 while yellow/orange $\sim$ 0,5-0,1) by the end of

the year. "Until the summer season" was replaced by "until the summer season, and to a lower extent until the end of the year"

p1901 l1-6: Most of the main features are described, but the model does not exhibit a fall bloom or increased chlorophyll along the North American coast (I appreciate that good performance is not expected in shelf seas). This leads to these features not really being captured by the assimilation (as discussed later).

We do not have a precise explanation to this, the low variability of the model when compared to data (as discussed earlier in this review) might be the cause.

p1901 l7-24: The reliance on the free run variability seems to be a weakness which will need to be overcome if the aim of producing "data-driven climatologies" is to be realised. More discussion should be given to possible ways this could be overcome (in the "Conclusions and perspectives" section), including the improvements that might be expected from using the full version of the scheme.

Paragraph added to "Conclusion and perspectives".

p1902 l1-3: Please expand on the reasons for this.

As physics was not constrained by an assimilation process, and there is no nitrate in rivers, such a high concentration of nitrate can only be caused by the assimilation process.

p1903 l6-29: Overall the assimilation is doing a good job of correcting chlorophyll magnitudes. However in the free run there is sometimes a bias in the timing of the spring bloom, especially in regions #1 and #3 (seen also in Fig. 2), which the assimilation does not seem to be correcting. Please comment on this.

It seems again to be related to the running EOF basis, if features are not included in this basis (because the free run is biased), this bias remains in the assimilation runs.

Generally speaking the anamorphosis run follows a very similar temporal evolution to

the free run, even when this differs from that of the SeaWiFS data. Is this due to the way the errors are specified? Or to the data coverage?

The EOF variability is low by the end of the year mostly because the temporal variability of biogeochemical components (especially phytoplankton) can be almost null on a 2 months running windows. So the assimilation system is only able to perform small steps in the good direction, while in the bloom period, corrections are much stronger.

p1904 l1-7: In region #4, the free run sometimes overestimates the SeaWiFS data, whereas the assimilation runs underestimate it, which seems odd. Is this just down to the averaging and data coverage used in the comparison, or is it a result of how the assimilation works?

Well, it is not clear to us, most of the time concentrations are in good agreement for region #4. It might be due to the averaging or the biogeochemical response to assimilation process. Model can only equal data it if the observation error is null,.

p1904 l18: Statistical significance has not been presented, so please use a different word than “significantly”.

Word "significantly" removed.

p1905 l1: “The free run performs well” - it looks to me like the free run is performing fairly poorly, particularly compared to some of the other regions.

While absolute concentrations values are in the order of magnitude of data, it is true that the seasonal dynamics is not catched by the model. This seasonal variability seems mostly driven by the North-Western African coast upwelling, entering the region # 8, as discussed for the year 1998.

p1905 l12-18: State here that the non-linear run performs better than the linear run.

Text was added

p1905 l28: Are these exactly equivalent? If I understand correctly, the model forecast

is valid for an exact time, whereas the observations are averaged over an eight-day period. This probably doesn’t invalidate the conclusion, but may affect the results. Please comment on this.

No it is not exactly equivalent, Also the aim of this comparison is, as indicated in the first sentences of the paragraph, to assess Âńhow the model is able to preserve after 8 days some benefit of the assimilation increments injected in initial conditionsÂż more than investigating the fiability of the short-term forecast.

p1906 l8: Statistical significance has not been presented, so please use a different word than “significantly”.

Removed from text

p1907 l6: Why have data shallower than 10 m been excluded?

Assimilation impact on deeper part of the water column is generally weak (as shown later in the paper). Data shallower than 10 m have been excluded in order to enhance the contrast between simulations, while following paragraph discusses results with respect to depth.

p1907 l10: How has the colocalization procedure been performed (nearest grid square, interpolation?)

Colocalization procedure was performed using the Sesam tool, it is a nearest grid square procedure.

p1907 l16: Expand “RMS”. Moreover, what is this the RMS of? The difference between modelled and observed nitrate? Or the (log) ratio?

The RMS is the classical root-mean square error. It is now expanded in the text. We compute it as: RMS=sqrt(1/N*Sum((log(Dn) - log(Mn))ˆ2) where: N is the total number of data entering the computation Dn is the nth data Mn is the nth equivalent colocalization in the model

p1907 l23-24: “Overestimations remain more or less unchanged ... whilst underestimations are significantly reduced”. Unless the model and observations agree exactly more often (which you don’t show), surely a reduction in underestimations must be balanced by an increase in overestimations?

Sentence changed toÂă: "to the free run while strong underestimations are reduced”

p1908 l4: Garcia et al. (2006) describes WOA05. Should the reference be Garcia et al. (2010)?

Changed

p1908 l1-11: It may well be the case that comparing to better quality controlled data will show the model to match the observations better. However this is not guaranteed, and calls into question the robustness of the conclusions based on the comparison that has been performed. Can you use the fully quality controlled data to compare against?

As discussed in Garcia et al. (2010) : “Individual data, and in some cases entire profiles or all profiles for individual cruises, have been flagged and not used because these data produced features that were subjectively judged to be non-representative or questionable.“ Which seems unclear to us, so we decided not to consider the quality flags.

p1908 l16: Please state at this point why it was chosen to exclude data lower than 1 mmol(NO3) m−3 from this comparison. At the moment, following on from the previous paragraph, it reads as if this is being done in order to address the quality control issues just discussed. This is not the case, it seems to be done because the model performs poorly at the lowest concentrations.

This is a pragmatic choice to show clear results. There is no a priori justification so we prefer explaining it just after this sentence in the paper.

Also, please say how many observations are excluded.

Added in the text

p1909 l19-20: “Underestimated and overestimated by (resp.) the free (a) and the linear run (b)” - is this the wrong way round? The linear run seems to underestimate in frame 2 (red dots).

The reviewer is right, thank you, words “underestimated” and “overestimated” were switched.

p1910 l5-6: Why was this section chosen? There appears (Fig. 6) to be nitrate data here. Is this for the same period? If so, can it be shown alongside? Can the equivalent nitrate climatology section be shown too?

This section was chosen arbitrary to illustrate the assimilation impact on the bloom region. No data are available there for this period.

p1911 l10-13: I’m not sure I fully understand the reasons for this conclusion, please explain more clearly.

The reason is what we could have expected intuitively. Sentence was changed.

p1912 l26: Statistical significance has not been presented, so please use a different word than “significantly”.

Changed

All "Technical corrections" were proceed, including figures corrections.

Interactive comment on Ocean Sci. Discuss., 9, 1887, 2012.