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Ocean Science Discussions

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Interactive Comment

Interactive comment on "Controllability of mixing errors in a coupled physical biogeochemical model of the North Atlantic: a nonlinear study using anamorphosis" *by* D. Béal et al.

Anonymous Referee #2

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Controllability of mixing errors in a coupled physical biogeochemical model of the North Atlantic: a nonlinear study using anamorphosis.

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In this paper, the authors investigate the ability of ensemble methods to control mixing errors induced by wind when observing biogeochemical variables and/or temperature.

The first part of this study focus on the response of the two components of the coupled model to wind forcing perturbations. Ensemble simulations have been realized during the spring bloom period and highlighted complex relationships between variables in several points of the North Atlantic.

The second part of the study focus on the introduction of anamorphosis functions in the Kalman filtering methods, in order to deal with the non-Gaussianity of some variables of the coupled model: a nonlinear transformation is applied to each variable to perform the analysis with transformed variables that are Gaussian. The algorithm suggested in the present paper corresponds to the EnKF with Gaussian anamorphosis suggested by Bertino et al (2003). The benefits of the this approach are evaluated into the framework of twin experiments with perfect observations. The results that are shown correspond to one analysis performed without taking account of the horizontal correlation of the errors. A significant reduction of the variance is observed when comparing with the plain Kalman filter.

This study can be seen as the preliminary results required when setting up complex operational multivariate data assimilation systems in a coupled physical biogeochemical model. The manuscript is well structured and the exposition of the ideas is globally clear (except for several comments below). Nevertheless, a better justification of the strategy and motivations of the authors would lead to a significant improvement of the manuscript (see comments). For such reasons, I would recommend its publication in Ocean Science after some revisions relative to the comments listed below.

Major comments

A It seems to be difficult to get a general conclusion in term of controllability of mixing error from your study which appears very localized in time as well as in space. The focused period corresponds to one month of the spring bloom when

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I guess the nonlinearities of the biogeochemical model are the most important. Furthermore, the relations between variables are really investigated on only two stations, the third one (INDIA) being dropped without justification when looking at the temporal evolution of the ensemble response (§3.2). You should better justify this spatio-temporal localization in introduction and explain why you do not validate your conceptual transfer during the other periods of the year and why you are focusing only on the BATS and GS station.

Concerning the assimilation experiments, it would have been interesting to see how the introduction of anamorphosis functions impacts the behavior of the ensemble after several cycles of forecasts and analysis. I am wondering if the "substantial reduction of error variance" that you noted in your unique analysis is a result that can be repeatably observed? Furthermore did you try to restart your systems from the analyzed ensembles? What was the behavior of the error during this forecast step for both ensembles? It would have been also interesting to show results of assimilation experiments performed in winter when the behavior of the biogeochemical model is strongly different of the one during the bloom period. Did you realize experiments at that time?

B I'm wondering why did you work with perfect observations. Real observations of surface chlorophyll present large errors. So the conclusion of your study may drastically change with the introduction of the observation error compulsory when dealing with real observations. Could you better justify this choice?

C Anamorphosis functions.

Your construction of the anamorphosis function raises important issues that may be problematic in a realistic framework (assimilation of real observations).

- Bias model.
  At each horizontal grid point, the data set use to build the anamorphosis function is made of the values of the n members of your ensemble. It means

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that the transformation will be strongly affected by the model bias. The way to define dynamically the interval of the original variable x (p. 1307, l.5 - 18, the bounds being defined by the forecast ensemble) may lead to an exclusion of relevant values for the observational update. For example, how do you deal with observations that will not be in the range $[x_1, x_p]$? This local adjustment of the anamorphosis functions using the ensemble statistics (p.1307, l.11-13) requires a model without or with a low bias. Well the coupled physical biogeochemical models, particularly the biogeochemical component, can present strong biases, that may damage the observational update. How will you take into account the problem of bias in future realistic configuration?

- Truncation of the analysis.

  It seems that you do not define tails to the anamorphosis functions. The minimum and maximum values of the transformed random variable y are defined by the percentile $r_1 = 1/2n$ and $r_p = (2n-1)/2n$. With n=200, it leads to $y_1 = -2.807$ and $y_p = 2.807$ (p. 1307, l.5 - 18). How do you process values out of $[-2.807, 2.807]$ that may appear during the analysis steps in the transformed space? Do you truncate them to x_1 and x_p when pulling back in the original space (as written in the manuscript)? In that case the functions would not be bijective. Furthermore you may favor arbitrary values (x_1 and x_p) due to the truncation, that are strongly influenced by the bias model. Would not it be better to consider to extend towards infinity the range of the transformed values to prevent such truncation?

- Spatial independence of the variables.

  As written p.1307, l.11-13, one anamorphosis function per variable is built at each grid point. It implicitly assumes that the variables are spatially independent. It results to a spatially monovariate observational update in the transformed space (as done in §4.3). I am wondering how does this loss of spatial correlation in the update affect the performances of the assimilation? As you do not assimilate observations localized in the vicinity of the grid point, the update may be very sensitive to the error (and the relevance of its estimation specified in the filter) of the unique observation that is used. Furthermore, what is your strategy with grid points that are not cover by an observation? We can also imagine that it may lead to spatial discontinuities for the transformed variables y, the anamorphosis functions being different from a grid point to another one, and then to spatial discontinuities in the analyzed original variables x^a. Did you note such phenomenon in your experiments?

- Stability of the anamorphosis functions to the Monte Carlo approach.

  The anamorphosis functions are built on the dynamical ensemble statistics (p.1307, l.11-13). It means that their shape may be greatly affected by the random draws generating the perturbations if the size of the ensemble is too small. Did you check the stability of your anamorphosis functions to the random process present in your Monte Carlo approach? Have you an estimation of the minimum size of the ensemble required to avoid such phenomenon?

- Observation error.

  I am not sure to understand your suggestion to deal with the observation error in the transformed space. If I am not mistaken, the observation error standard deviation in the transformed space is obtained by transforming the one from the original space with the "local slope" of the anamorphosis function. When you say "local slope", does it correspond to the slope associated with the percentile of the observation error standard deviation or the one associated with the value of the observation? Furthermore, I am wondering if this sort of "linearized" approximation can lead to the introduction of bias in the estimation of the observation error in the transformed space (overestimation or underestimation)? For the case of chlorophyll observation for which the error can be assumed to be lognormal, would not it be easier to

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directly define the transformed observation error standard deviation by the percentage of error assumed for the original observation?

D §4.3 Application of the non linear update over the North Atlantic

In your discussion, the impact of the introduction of anamorphosis function is only evaluated by the reduction of the spread of the ensemble. The framework of twin experiments allows you to access to the true state, that's why I am wondering why are you not looking at the RMS error of the solution? This diagnostic could be useful to check the relevance of the estimation of the error evaluated from your ensemble (P^f and P^a). So p.1312, l.12, you say: "Fortunately, they mostly corresponds to regions where the forecast ensemble error is small" when talking about areas where both assimilation methods are not efficient. First, it seems to be reasonable to get low corrections in areas where the filter diagnoses low errors (low forecast ensemble error). Secondly, it does not mean that the RMS error is low as the ensemble may underestimate the error. Unfortunately, these areas of low forecast ensemble error may also be areas of significant error (areas of strong model bias for example). You should add this diagnostic and also remove "Fortunately".

E I am wondering what are the differences between the Monte Carlo method used in your experiments and an Ensemble Kalman filter? It seems to be the same methods but I was not able to find the expression EnKF in the manuscript:

- p.1291, l.20: reference to Evensen (1994). You could add also Evensen (2003) and/or Evensen (2006).
- p.1292, l.7: "the Monte Carlo method".
- p.1304, l.10-15: it seems that you use an EnKF with perfect observations (Evensen, 1994).

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If the method used to assimilate data is an EnKF, please write it explicitly in the manuscript.

Minor comments

- p.1290, l.22: "a simple nonlinear change of variables". You could introduce the word "anamorphosis". It will be easier to understand the title.
- p.1291, l.23: "One then postulates a prior probability distribution for these errors". You could be more explicit and talk about Gaussian distribution as you perform Kalman filter analysis in the manuscript.
- p.1291, l.28-p.1292.l1: "if robust relationships exist between model and observation errors, if these relationships are linear". No sense: the theory of Kalman filtering assumes that the model and observation errors are independant.
- p.1293, l.14: "A possible approach to nonlinear estimation problems is the use of anamorphosis transformations (Bertino et al, 2003)". The introduction of anamorphosis function is more dedicated to deal with the non-Gaussianity of the variables rather than performing nonlinear estimations. The EnKF with Gaussian anamorphosis suggested by Bertino et al can be interpreted as a linear estimation method (the analysis scheme being linear in the transformed space). Please rewrite the sentence.
- p.1293, l.19-20: " The simplified solution that we propose in this paper is to perform the change of variable separately for each state variable". The use of monovariate anamorphosis functions corresponds to what has been suggested by Bertino et al (2003). Please rewrite the sentence.

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- p.1293, l.24-25: "However, it is usually possible to diagnose [...] is not". If you are talking about the multi-Gaussianity of the state vector, you will have to perform χ_2-tests. Unfortunately, it is very expensive for large systems (as coupled physical biogeochemical model).
- p.1294, l.9: "central model simulation" $\rightarrow$ "control simulation" (?).
- p.1295, l.20 and l.24: "initialised" and "initialized". Please unify.
- p.1296, l.12: "interanual" $\rightarrow$ "interannual" (?).
- p.1296, l.19: "EOF". Please define the acronyms.
- §2.2 Wind ensemble perturbations

  The ensemble is generated by the introduction of wind perturbations. These perturbations are built from an EOF analysis of ERA40 winds. It raises the problem of the transfer of biases present in the ERA40 database to the perturbations. We can imagine that such process may favor particular structures of perturbations, leading to a less relevant modelling of the ocean error subspace. Did you note such problem? Furthermore, what are the expected benefits for this EOF approach comparing to a spectral method (Evensen, 2003) for example?
- p.1297, l.10: "perturbated" $\rightarrow$ "perturbed".
- p.1298, l.10-l.28: you should consider to remove the paragraph dealing with the rank correlation. This diagnostic appears only in the table 1 and the results are not qualitatively exploited in the discussion.
- p.1300, l.1-6: I am not sure to understand the explanations given for the scatterplot at INDIA station. How can you explain this shape of the scatterplots? Is it due to the layers under 400m that are too thick to "feel" and transfer the wind

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perturbations? Furthermore, why does the increase of the wind stress lead to a slight decrease of the mixed layer depth?

- p.1301, l.6-7: "The INDIA station still shows a complex response which is difficult to interpret by simple mechanisms". Do you have a precise idea of the processes involved in this area, even if they are not simple? Did you note equivalent scatterplots in other stations (not shown and discussed in the manuscript) or are they only localized in this area?

- p.1302, l.27 -p.1303, l.1: "In particular, relationships [...] PHY". Is it not too ambitious to try to generalize the forecast length of your data assimilation system from a study realized at a given datum on an unique point? For example the 4-day length seems to be relevant in april at BATS station but not at GS station. How can we deal with such localized (in time and space) information?

- §3.2 Temporal evolution of the ensemble response

  It would have been interesting to look at the evolution of the ensemble over 2 weeks at INDIA station. Your conclusions for short term forecast (1 day) in §3.1 is that the mixing being low (due to the stratification of the water column) in this area, other processes are enough important to significantly influence the dynamics of the biogeochemical variables. We can imagine that the increase of the spread of the ensemble, resulting from long forecasts, may lead to a destabilization of the structure of the water column (at least for several outliers), and then to an increase of the dominance of the mixing in the behavior of the system.

- §3.3 Observability of physical and biogeochemical variables using chlorophyll data

  The benefits for the manuscript of including this discussion are not obvious. First, the way to get the update ensemble (blue points on figure 8) is not clear. For example, we have to read §4 to understand why there is only one abscissa. Furthermore it does not bring more information than what was written previously in §3 and what is written in §4. You should consider to remove the paragraph.

- p.1303, l.22: "obsevational" → "observational".
- p.1304, l.7: "non-Gaussian behaviours". It is more or less the first occurrence of the non-Gaussianity of the variables in the manuscript. It would have been more relevant to diagnose the non-Gaussianity of the variables in the previous discussions.
- p.1305, l.16: "formula (6) rewrites" → "formula (4) reads" (?).
- p.1306, l.17: "the true regression line has a general positive curvature". The expression "true regression line" seems not to be suitable, even if the mean is understandable. Maybe "segmented regression" would be more relevant. Idem p.1308, l.24.
- p.1308, l.24-25: "these are two features [...] distribution". It is interesting to get this linear relation between the transformed variables, even if it is not guaranteed. Did you note similar results at other stations (Gulf Stream, INDIA) and/or during other periods of the year?
- p.1309, l.15-29: The expressions "regression line is linear" and " regression line is nonlinear" seem to be not suitable. Maybe you should talk about linear/nonlinear relations between the variables.
- p.1309, l.26: "always leads to a significant improvement". Is it a result from additional experiments or a conclusion extrapolated from the figures shown in the manuscript? You state that the linear relationship obtain between the transformed variables on figure 9 can not be guaranteed. Can you justify this generalization?

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- p.1310, l.6-9 : case (iv). Even if the anamorphosis functions do not significantly improve the correlations between the variables, you may get benefits from the improvement of the distribution of the transformed variables. It seems to be worth to transform the variables in that case also.
- p.1310, l.15-17: You should remove the brackets.

Interactive comment on Ocean Sci. Discuss., 6, 1289, 2009.