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Ocean Science Discussions

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 #3

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In the manuscript, the authors address the problem one can face when assimilating data into a coupled physical-biogeochemical model: interactions between components of the complex model and thier responds to model errors are non-linear and not always characterized by Gaussian statistics, moreover they are spatially variable. In particular, based on ensemble of model integrations performed in the North Atlantic for the period of phytoplankton spring bloom, the authors investigate the reaction of modeled upper mixed layer (UML) thickness, temperature (TEM), (salinity), phytoplankton (PHY), Nitrate (NO3) and zooplanktona (ZOO) on perturbations (imposing uncertainties) in

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wind stress (WND). To account for non-linear and non-Gaussian nature of the variable in space and time relationships between model components, non-linear anamorphosis functions are constructed for every grid point to project non-Gaussian variables to Gaussian space. Then any data assimilation approach based on Gaussian assumption and linear update could be used. Thus, in a so-called twin experiment, the authors implement ensemble Kalman filter to assimilate chlorophyll data (generated by the same model but with another forcing) and discuss benefits of using such anamorphosises for estimation of NO3, ZOO,UML...

The manuscript is well structured and clearly written. However I have got some comments and suggestions which the authors might want to consider. After the revision I would recommend the paper for publication in the special issue of Ocean Science.

Major comments:

Part. 3. Study of the ensemble forecast (p. 1297) "... at a dozen of locations in the North Atlantic..." The paper could be improved if the authors have discussed in some more details Table 1 (the spatial variability of the considered relationships between the physical/biogeochemical components) and then justified their choice of the 3 locations of interest. For instance, when discussing TEM/MLD relationships, correlation at the Labrador Sea, Norway, Newfoundland stations could be mentioned (with some explanation) as an exception in generally obtained negative correlation. The authors should try to interpret the WND/MLD (and MLD/NO3 and MLD/PHY) relations at the weather station INDIA. Convection is the main mixing mechanism at the latitudes. It could be that changes in wind stress may intensify or contrary make weaker the mixing (if less dense surface water is coming to the location due to wind circulation)... Here the question about WND perturbations arises. How did the authors perturb the wind stress: whether they considered u and v component independently or already combined amplitude?

p. 1301, when making a discussion on NO3/PHY relationship, it would be better to

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mention about surface nutrient (NO3) consumption while phytoplankton growing, than explaining the obtained negative correlation by "...inverse distribution of those two quantities over the water column." Such a distribution could be the consequence but not the reason;

Assessing the filter performance with and without the anamorphosis transformation, would it be possible to present RMS error relative to reference solution (which used to generate chlorophyll data for assimilation in the twin experiment). It would be also of interest to see results of a next data assimilation step (since reduced ensemble spread does not always guarantee better forecast and state update at the next analysis steps).

Minor comments:

p.1291, "Coupled physical–biogeochemical models..." it would be a better place/time to introduce your acronym CPBM;

p. 1292, "a Monte Carlo method" instead of "the Monte Carlo method";

p. 1295, "the December climatology (Conkright et al., 2002)" instead of "the December Levitus climatology 2001 (Conkright et al., 2002)" ?

p. 1297, WND, what kind of wind stress characteristic (u, v components or...) is considered?

p. 1297,1298, acronyms WND, MLD, TEM, would be better to define them, as well as BATS, INDIA NABE

p. 1303, "... the cascade of errors from WND to MLD, from MLD to TEM, and finally from TEM to PHY " What about direct "from MLD to PHY"? The authors consider the period of phytoplankton bloom, and the main mechanism of this event is shallowing MLD + increasing irradiance. Analyzing MLD/PHY correlation (p. 1301) the authors could also make an accent on this event.

p. 1304, Why is the results for INDIA station not presented? " K is the Kalman gain"

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instead of " K is the gain"; p. 1309, when distinguishing the "four kind of situations" of where and how well the state update with the anamorphosis transformation performs, would it be worth referring to the locations (BATS, GS, . . . INDIA stations) where the certain situation exists but not only to the figures (Fig 3, 4. . .5);

p. 1310, " . . .the method has been only applied to a state vector made of 2 variables. . ." please repeat which variables;

Fig. 7, 8, Would not it be possible to use the same scale for similar scatterplots.

Typos/Misprints:

p. 1295, "6 prognostics" instead of " 6 pronostics" ;

p. 1305, ". . . the gain (5) provides. . ." instead of ". . . the gain (4) provides. . ."; ". . . formula (4) rewrites. . ." instead of ". . .formula (6) rewrites. . .";

p. 1308, "The transformed scatterplot is shown in Fig 9 (middle panel)." Instead of "The transformed scatterplot is shown in Fig 10 (middle panel)."; please use r_k = {0.0025,. . .} and y_k = {. . .}. Fig. 8, upper panel, second and third plots from the left, figure quality differs from those of the others.

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