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Ocean Science Discussions

Interactive comment on "Application of the Gaussian anamorphosis to assimilation in a 3-D coupled physical-ecosystem model of the North Atlantic with the EnKF: a twin experiment" *by* E. Simon and L. Bertino

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This paper presents a new implementation of the Ensemble Kalman Filter (EnKF) developed for data assimilation into dynamical models characterized by strongly non-linear behaviors. The uncertainty arising from the application of such models is manifested by non-Gaussian distributions of the state variables and related errors, making traditional assimilation methods such as the Kalman filter sub-optimal or simply unfeasible without drastic modifications of the original scheme. Even the EnKF in its seminal formulation isn't fully suitable for non-linear data assimilation as only the forecast step

properly treats the non-linear evolution of the error statistics while the analysis step is linear in essence.

In this paper, the authors describe a modification of the Ensemble Kalman filter using the so-called anamorphosis method to transform the model variables into approximately Gaussian variables and to execute the analysis step using the transformed variables. The concept of anamorphosis itself is not original as it was previously introduced by Bertino et al. (2002) in the context of geostatistics. However, the proposed implementation has the distinguished feature to build the anamorphic functions consistently with the ensemble statistics of the filter.

Even if some refinements of the method are still to be explored, the application to coupled ocean circulation/ecosystem models is convincing and demonstrates good performances in the framework of twin experiments. In addition, this study suggests that the anamorphosis approach should be further developed in the perspective of realistic ocean colour data assimilation (in the framework of the EnKF or other Monte Carlo methods) which is a very relevant topic for the oceanographic community.

The manuscript is well structured, the exposition of the method and experimental protocol is mostly clear (except for some aspects mentioned below), and I would recommend the paper to be published into Ocean Science after some moderate revision. I suggest that the authors address the following questions and comments when revising their manuscript.

General comments and suggestions:

1) The review of Kalman-based assimilation studies into coupled physical-biological ocean models presented in Section 1 (p 620) is interesting although probably not exhaustive. Instead of just enumerating the applications published in the literature, the authors should provide a brief discussion about the main lessons learned from these previous studies, and how the proposed anamorphosis approach articulates with them (especially Natvik et al. who considered a very similar modelling framework). In ad-

dition, alternative methods such as particle filters (SIR, Losa et al., 2003) exist for non-linear data assimilation: how would they compare regarding, e.g., computational efficiency ?

2) The description of the procedure for constructing the anamorphosis functions (sections 2.2 and 3.3) remains a little obscure and incomplete (especially for step 1, page 625). Some more practical details should be given with a view to make the procedure reproducible in similar setups, and to facilitate the interpretation of results (especially the spectacular improvement obtained with the anamorphosis method in ANA experiment). One could wonder what are the key mechanisms responsible for this improvement, and the reader is left a bit hanging on that point. In ECO experiment, it might be interesting to quantify how frequently the statistical analysis generates negative (or unrealistically high) concentration values.

3) The perturbed atmospheric fields used to generate the initial ensemble are primarily affecting the physical state of the coupled system. The perturbations then cascade into the biological components. However the assimilation scheme does only correct the biological state of the system, leaving the circulation component unchanged. Why not simply consider a fully coupled physical/biological state vector, or alternatively perturb the biological model components without perturbing the physics? The strategy chosen in this study should be better justified.

4) The observation operator H is not precisely defined. Please explain how the surface chlorophyll concentration is diagnosed by the model, and clarify the relationship between observations and the two phytoplankton compartments. It might be interesting to compare the anamorphosis functions constructed for diatoms, flagellates and chlorophyll.

5) The word “limitations” is repeatedly used in the text, with apparently different meanings from place to place and sometimes misleading interpretations. For instance, p 621 “Such models present important practical and theoretical limitations for the application

of data assimilation methods . . .”: in this case, the “limitations” are probably more in the assimilation methods than in the models! Similarly, “physical bounds” (e.g., legend of Table 1, p 622 etc.) is misleading as long as biological quantities are concerned. I suggest to adopt less sloppy terminologies and check the language throughout the text.

6) The coupled model simulations are unrealistic in the equatorial Atlantic Ocean. Since the assimilation procedure is not activated along the southern boundary, the figures showing horizontal distributions (e.g. Figs. 9, 10, 11) could be redrawn to focus on the regions (North Atlantic and Arctic regions) where the assimilation results are significant.

Minor corrections

- p 619, l.2: “in term of” → “in terms of” - p 619, l.8: “ecosystem” →“ecosystems” - p 919, l.21: the error on ocean colour (30%) seems to be excessively optimistic w.r.t. estimates from the literature (e.g. Ballabrera et al., 2003); - p 619, l.27 and l.28 are unclear. Please rephrase. - p 620, l.10 : “on ecosystem model mainly this last decade” → “to ecosystem models mainly during this last decade” ? - p 621, l.18 : “develops ” → “developed” - p 622, l.7 : “conclusion” → “conclusions” - p 623, l.9 : “variables” → “variable” - p 623, eq.4 : the product should be defined - p 623, eq 5: it might be useful to specify which forecast error covariance matrix (in the transformed space ?) is used to compute the “transformed” Kalman gain - p 624, l.7 : “in general case” → “in general” - p 624, l.20 : “check” → “satisfy” ? - p 626, l.3 : what do you mean exactly by Âń continuous distributions Âż ? - p 627, l.18 : “diatom” → “diatoms” - p 628, l.5 : “all the component” → “all the components” - p 628, l.29 : “Eq. 8” → “Eq. 3” ? - p 632, l.17 : “longer” → “higher” ? - p 633, l.6 : “on one only point” → “on only one grid point” ? - p 633, l.17 : unclear sentence, please rephrase.

References:

Ballabrera-Poy, J., R.G. Murtugudde, J.R. Christian and A.J. Busalacchi (2003). Signalto-noise ratios of observed monthly tropical ocean color, Geophys. Res. Lett. 30(12), 1645, doi: 10.1029/2003GL016995.

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Interactive comment on Ocean Sci. Discuss., 6, 617, 2009.