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Geoscientific Model Development Discussions

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Interactive comment

Interactive comment on "Physical-biogeochemical regional ocean model uncertainties stemming from stochastic parameterizations and potential impact on data assimilation" *by* Vassilios D. Vervatis et al.

Anonymous Referee #1

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The general purpose of this paper is to produce ensemble simulations with a physical-biogeochemical model of the Bay of Biscay, to assess these simulations using sea surface temperature, sea level anomaly and ocean color observations, and to evaluate the potential impact of the new ensembles in a Kalman data assimilation framework. In terms of model development, the novelty is in the introduction of long-range horizontal correlations in the stochastic parameterization of the NEMO ocean model. In terms of application, many ensemble simulations are performed to evaluate the effect of a variety of stochastic perturbations. In my view, the general approach is meaningful,

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Discussion paper

even if the paper is clearly more directed towards practical application than model development.

However, for the reasons explained below, I struggled a lot to read the paper and to write a review, mainly because there are so many different things involved (as acknowledged in the introduction: “the scientific objectives cover a broad spectrum of interdisciplinary components”), and because the text is often unclear and makes it difficult to figure out what is effectively done (despite the many details provided). I also found that the methods used to simulate the large-scale perturbations and to evaluate the results are questionable, without sufficient explanations and justifications. Overall, I do not know if these problems can be solved or not, but this should certainly require a thorough reconsideration of the text to improve the clarity of the arguments (more simple, more factual), and to provide the missing explanations and justifications.

Main comments

1) I do not understand the method used to generate the large-scale perturbations. I do not know what an “elliptic Gaussian equation” is. I checked the expression in google, and I had only 7 replies, all of them (except one) from the author’s work. A classic approach to generate a long-range correlated noise from a white noise is to solve an elliptic partial differential equation like $E(x) = w$, where w is a white noise, x the resulting correlated noise, E an elliptic operator, like a Laplacian operator (times the square of a length scale). I checked the code provided with the manuscript and I found no solver for an elliptic partial differential equation. Anyway, whatever the method, we need to understand what it is, how it works, what are the benefits with respect to other methods, and what are the expected properties of the resulting noise in terms of correlation structure. If not, there is no real model development in this paper, and it is probably better to move to another journal.

2) I do not really understand the method that is used to compare the stochastic ensemble simulations to observations. This is at the core of the paper and would need

to be based on a solid ground. The results of the comparisons are displayed inf Figs. 6, 7 and 8, which mainly show averages or mixtures over the whole domain. On the one hand, what is done to compute these statistics is unclear to me, and it is difficult to have a clear idea of what stands behind the various curves that are shown. On the other hand, I do not understand how these global results can be used to deduce that the ensemble are more or less consistent with the observations. It is indeed repeatedly stated in the paper that the system is very heterogeneous, and I thus presume that the local consistency with observations is an important issue. In summary, we need to understand the logic supporting the validation procedure, and why it is applicable to this problem.

3) The paper is made of three different components (description and development of the method, evaluation of the ensemble simulations, and impact on data assimilation), which are presented and discussed almost independently. I understand that correlated stochastic perturbations were needed to produce the ensemble, and that good ensembles are likely to improve data assimilation. I think this is not sufficient to connect the components of the paper together. For instance, the effect of the new methodological development on the ensemble simulations is not specifically discussed, and the effect of data assimilation is mainly described in terms of realism and physical consistency than in relation to what is done in the rest of the paper.

Other comments

1) A previous paper by Vervatis et al. (2016) is cited throughout the paper. I think it would be necessary to better position the present paper with respect to the previous one.

2) The appropriate reference for the SPPT scheme is Buizza et al. (1999).

3) Many of the figure legends are confusing. For instance, in Fig. 4, it is necessary to read the whole legend to find out that the first 6 panels are for SST and the last 6 panels are for SSH.

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