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Interactive comment on "Assimilation of chlorophyll data into a stochastic ensemble simulation for the North Atlantic ocean" *by* Yeray Santana-Falcón et al.

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We thank the anonymous reviewer of the manuscript for its careful revision and thoughtful comments and suggestions. We have considered all suggestions and responded below to each individual comment. We hope that we have been able to solve the gaps and answer other queries in our revised text.

Reviewer 2: This paper describes the assimilation of chlorophyll into a model of the North Atlantic Ocean using the SEEK assimilation method. The method relies on an ensemble of 24 members. The results show that the models' chlorophyll, which is the variable that is assimilated is improved after assimilation, however not in all regions.

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In some regions the model variability does not cover the observations and there the assimilation does not improve the chlorophyll. The model results also show that the non-observed variables (nutrients) are not necessarily updated to a better state and in some regions it increases in the upper 100 meters. Finally, they propose a method for alleviating this problem by only applying assimilation to the model fluctuations, this method is demonstrated for one month only.

Overall, I find the paper well written with very interesting results that contribute to the development of data assimilation methods for biogeochemical models and is therefore relevant for OS. However, there are a few things that are unclear, so I propose some minor revisions to this manuscript before it is accepted for publication.

Main comments: 1) Method of generating the ensemble: here the paper simply refers to two publications and refer the reader to those. The method of generating the ensemble is very important and I think the reader deserves a short description of how this was done.

As suggested by the reviewer, a simplified explanation of the methodology employed in Garnier et al. (2016) to generate the ensemble is included in the text: "...whose uncertainties may have a direct impact on the estimation of primary production. Specifically, the parameters perturbed are the phytoplankton growth rate at 0° C, the initial P-I slope for both nanophytoplankton and diatoms, the phytoplankton temperature sensitive of growth, the zooplankton temperature sensitive of grazing and the growth dependency to the day length for both nanophytoplankton and diatoms. For the perturbations, the starting point is a first-order autoregressive process setting up with a standard deviation of 0.3 and a decorrelation time scale of 1 month, at which a random noise is drawn at each grid point and at each time step. After spatial filtering, Gaussian noises are transformed in Lognormal noises to guarantee positivity. Stochastic perturbations are then introduced by multiplying by these Lognormal noises. To preserve vertical consistency, all perturbations are set identical for the whole water column. In addition, as the effects of unresolved scales will have an impact on the large scale biogeochemical representation, we create a perturbation that simulates the unresolved fluctuation of the concentration of each parameter within every model grid box.".

2) Update the paper structure: The method of only assimilating the model fluctuations around the climatology, should be introduced in the method section and the results presented in the result section. Then reserve the discussion section for discussion of the results. I am also confused by the sentence that starts with "For the sake of.. " on line 416, so please clarify what you mean by the climatology in this case. Also specify which period is run. What happens to chlorophyll in this case, is the spread of the profiles increases or does it just appear that way in the figure 9?

We considered the structure proposed by the reviewer when first starting to write the manuscript. However, we finally decided to present first the results of the main two simulations (the free run and the assimilated) and discussed them, and then explain the sensitivity experiment developed in order to cope with the inconsistencies found in the system. We think this structure is the most appropriate as we first present the strengths and weaknesses of the system, and then try to solve them by only assimilating model fluctuations. Section 4.3. has been rewritten to provide a more detailed explanation of the method, and to enhance the interpretation of the results. We expect these issues are clearer after modifications.

3) Trying to understand the results in context of the physical model performance: It would be useful to have some information on the physical models' performance, I am thinking especially of the representation of the extent of the subtropical gyre, since that seems to be a problem area.

We agree with the reviewer in that physical model performance is of utmost importance to understand some general flaws of our system. The eddy-permitting physical model used in the present study was first documented in Barnier et al. (2006), in the context of numerical schemes tested in a global, $\frac{1}{4}^{\circ}$ configuration to reduce the known biases in the representation of western boundary currents and subtropical gyres, such as in the North Atlantic. In Ourmières et al. (2009), a detailed analysis was made on the mixed layer dynamics at mid-latitudes, which is known to have a decisive impact on primary production. Comparisons were made between a free simulation (as in the present study), experiments with assimilation of physical observations (SST, altimetry) and climatological data (T, S and nutrients), and a seasonal climatology of mixed layer depth. They observe that in March, the free solution exhibits a too deep mixed layer extended over an abnormally large area compared to the climatology, in the Gulf Stream region and its north eastern extension. In April, stratification takes place in the model, in agreement with the climatology, despite a remaining area of large MLD east of Cape Hatteras and an overestimated zone in the north-east Atlantic above 45°N. They also show that the combined assimilation of physical and nutrient data has a positive impact on the phytoplankton patterns by comparison with SeaWiFS ocean colour data. It is obvious that this biased representation of the dynamics has a significant impact on the results of our study, which is dedicated solely to the control of sources of uncertainty in the biological model. Therefore, in the revised manuscript, we refer more explicitly to the MLD analysis of Ourmières et al. (2009).

Other: In the title and abstract 'ocean' is spelled with a capital O.

This has been amended in the new version.

Abstract ". . . are assimilated daily into. . .!

Line 42: " . . .to what extent. . ."

Line 45: "To that end.."

Line 115: It should be "cost efficient"

Line 118: "We observed that the. . ."

Line 123 exchange "one-day" with "daily composites"

Line 130 "commented on below. . ."

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Line 136 delete “biomass” at the end of the sentence.

Line 215: suggest “minimize” instead on “diminish”

Line 393: “. . ..(1) it significantly reduces . . .”

Precedent suggestions have been taken into account and changed accordingly in the new version of the manuscript.

Line 40 and onwards: Please explain the statement: “However, none of the latter studies explicitly incorporates the uncertainties in the ocean biogeochemistry introduced by stochastic approaches.” For example Ciavatta et al 2011, generates an ensemble by perturbing the background light attenuation, that would also be considered a stochastic approach, no? Do you mean that in this case the perturbations are done on the model parameters and not on the forcing?

In this system, in contrast to similar works, the ensemble has been explicitly developed by introducing perturbation on model parameters. To clarify this point, we have added a short comment on the manuscript: “However, none of the latter studies explicitly incorporates the uncertainties in the ocean biogeochemistry introduced by stochastic approaches on the model formulation.”.

Line 65: I would characterize 1/4 degree resolution as eddie permitting rather than eddie resolving.

Following the suggestion of the reviewer, we have changed the term to “eddy permitting”.

Line 85: the model description mentions iron input from rivers, are any other nutrients supplied from rivers?

Yes, riverine inputs of other nutrients are also taken into account by PISCES. As a default, river supply of all elements but DIC and alkalinity is taken from GLOBAL-NEWS2 (Mayorga et al., 2010). We consider that mentioning iron inputs is not relevant for the sake of this manuscript, and it has been removed in this new version.

Line 106: Do you mean the “biogeochemical system”?

Seven biogeochemical parameters were perturbed to include uncertainties arising from the limitation of the simulation to describe the biogeochemical system. We have included explicitly in this version that we refer to the biogeochemical system by “..the simplification of the description of the biogeochemical system to a limited number of state variables and parameters.”.

Line 108: Specify which key biogeochemical parameters were perturbed?

The parameters that were perturbed in the system presented in Garnier et al. (2016), and that we use here to build the subsequent assimilation system are presented now in the text as: “...whose uncertainties may have a direct impact on the estimation of primary production. Specifically, the parameters perturbed are the phytoplankton growth rate at 0° C, the initial P-I slope for both nanophytoplankton and diatoms, the phytoplankton temperature sensitive of growth, the zooplankton temperature sensitive of grazing and the growth dependency to the day length for both nanophytoplankton and diatoms.”.

Line 224-225: suggest: “However, there is a too strong gradient between the oligotrophic conditions of the North Atlantic subtropical gyre and temperate waters to the north.”

The sentence has been changed as suggested.

For the discussion: What could be done to reduce the ‘stripes’ left by the satellite swaths on the DA analysis?

Satellite imprints are due to the small localization radius that is used. This radius needs to be small because the horizontal correlation length scale of the forecast uncertainties in the chlorophyll field is also small. Because of this local behaviour of the system, the impact of a given observation on the observational update must remain local as well, and it is difficult to avoid seeing the imprints of the border between the observed and non-observed regions on the updated fields. The fact that this imprint can still be seen in the forecast actually means that the increment is well retained by the model, and that the model keeps it local (over a few days) consistently with what is said above. However, these imprints should progressively disappear with time as more and more observations are assimilated, so that the error in the system and thus the magnitude of innovation decreases. In our experiment, this does not happen everywhere because the time lag between observations is quite large with respect to the typical time scale of the system. The model error is also substantial, so that innovation does not become small enough to avoid producing quite large increments with a visible imprint of the borders of the observed area. This behaviour of the system is now better explained in the paper: "These imprints are caused by using a small localization radius. This radius needs to be small due to the small correlation length scale of forecast uncertainties in the chlorophyll field. Thus, the impact of a given observation on the update remains local. They should disappear over time as the magnitude of the innovation decreases. In this experiment, however, the time lag between observations is quite large with respect (5 to 7 days) to the typical time scale of the system."

The very northward region would be ice-covered during part of the season, is that included in the model?

Yes, the physical model code corresponds to NEMO version 3.4. This model benefits from the LIM-2 sea ice parameterization presented in Fichefet and Maqueda (1997), which includes the most relevant thermodynamics (exchanges of heat) and dynamics (exchanges of mass and momentum) sea ice-related processes.

Please provide the name of the Longhurst provinces by names in addition to numbers in the figures, there is room for that and it will make the reading of the paper easier.

Longhurst provinces are now named both in Figure 1 and its label, and into the text. Label of Figure 1 now includes: "Provinces indicated by acronyms NADR (North Atlantic Drift), NATR (North Atlantic tropical gyre), NASTE (Northeast Atlantic subtropical gyre), and NASTW (Northwest Atlantic subtropical gyre) are used throughout the text.". In the text we have included: "The provinces used are NADR (North Atlantic Drift), NATR (North Atlantic tropical gyre), NASTE (Northeast Atlantic subtropical gyre), and NASTW (Northwest Atlantic subtropical gyre).". Then, acronyms substitute province numbers throughout the manuscript.

Figure 6 and 9: it is very difficult to read the black text on the dark blue background, try white text?

Labels have been conveniently modified in the figures.

Figure 6: In province 6, the deeper nutrients also quite far off from the climatology also in the free run, why is that? Does it persist far down into the deeper layers?

The climatology used to compare with model data contains data up to 800 m deep. Through the water column between surface and this depth, nutrients are overestimated consistently in province 6. The shape is reproduced but values are far from climatological data. One possible explanation for this overestimation is that comparisons are made between daily snapshots extracted from our assimilated and non-assimilated simulations, and an observational-based climatology. On the other hand, the deterministic simulation already overestimates chlorophyll in province 6 (NASTW) as observed in Garnier et al., 2016. As commented above, Ourmières et al. (2009) carried out a detailed analysis on the mixed layer dynamics at mid-latitudes. They observed a band with too high concentrations of nitrate in the northern part of this region (35-40° N) that triggers a chlorophyll overshoot in the following months as we observed here.

Figure 7: Could you add a third column where you show the isolines of climatological nitrate?

Figure 7 was intended to show the effects of assimilation by comparing the water column before and after the procedure. The WOA nitrate fields correspond to monthly climatological data, and offer a coarse grid resolution of 1°. We consider that a section from this data is not appropriately comparable to the ensemble simulations presented here.

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