Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-38-AC3, 2018

Ocean Science
Discussions
Open Access

Interactive comment on "Dense CTD survey versus glider fleet sampling: comparison of the performance for regional ocean prediction West of Sardinia" *by* Jaime Hernández-Lasheras and Baptiste Mourre

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Received and published: 7 August 2018

Reviewer:

General Comments:

In this work, the authors present results from different simulations assimilating data of several observational arrays in synergy with glider data. The analysis includes a high-resolution regional model for the Western Mediterranean, with a focus in the coastal area West of Sardinia. The system incorporates a Local Multi-Model Ensemble Optimal

Interpolation scheme to ingest satellite and dense in-situ data. The concluding remark is that an optimized sampling strategy of a gliders fleet in the future can significantly increase the Data Assimilation (DA) performance of an operational application.

The manuscript is clear, concise and well written. The study shows some interesting results and supports well the argument of designing glider missions in synergy with other observational platforms to increase DA performance. However, I have one main concern regarding the performance of the stand-alone ocean model without DA, which in turns raises some questions for the DA post-analysis correction. Overall, I find the manuscript worthy of publication, after a major revision. Please find below a list of comments that I would like the authors to address. My first two specific comments are the most important ones.

Response:

We acknowledge the reviewer for her/his constructive comments which have helped us to improve the manuscript.

Reviewer:

Specific comments:

1) My main concern is the performance of the regional ocean model WMOP without DA. The authors provide a schematic of the main circulation features for the whole WMOP domain (i.e. Fig. 2 top panel), but model performance and outputs are only presented for the coastal area REP14-MED (i.e. Figs. 2 bottom panels, 4, 8, 9, 12). This is not a good practice, especially when the free run appears not to represent adequately the coastal dynamics of the REP14-MED domain (there are large biases, e.g. page 12 lines 2-3, and completely different circulation patterns before and after DA). The authors should at least provide a validation section of the regional model over the whole WMOP domain without DA. In my view, it is acceptable to have a well-

tuned regional model (e.g. like WMOP), even if it fails in some coastal areas (e.g. in REP14-MED domain).

Response:

This is linked to our answer to reviewer#1. We have added a new section aiming at providing elements of validation of the model over the whole domain, in particular illustrating the absence of model bias at the basin-scale. A more comprehensive validation of the model is out of the scope of the present paper. It is the focus of a specific work already presented in scientific congresses, and presently in the process of peer-reviewed publication.

Reviewer:

2) Following my first comment, the implications of having a biased model coupled with a DA system can be significant. For instance, a DA platform usually incorporates a convex scheme, and therefore it will always return an analysis correction. The main question is if this correction actually has a physical meaning (even if the RMSD error is reduced after DA, as it is the case in this study). Especially, in ensemble-based DA systems one should show that model and data pdfs overlap (at least partially). I would like the authors to illustrate that the model ensemble spread has joint probabilities with the assimilated observations, taking under account their errors mentioned in the text (e.g. page 6 line 32). This could be done providing innovation/misfit statistics in data space for some variables (e.g. at least one from SST, SLA, T, S), over a period and an area of the authors preference (e.g. an area covered from satellite observations and/or glider/Argo profiles).

Response:

We totally agree with the concern of the reviewer. To better clarify background model errors, we have added a figure showing the innovations in terms of SST, SLA, T and

S for the first analysis of the spinup period (figure 4 in the article). This shows that 1) there is no significant bias over the whole domain, and 2) the magnitude of the innovations is in agreement with the prescribed observation errors and ensemble spread. Thus, we are confident that the system is properly calibrated at the scale of the model domain over which the analysis is performed. The apparent bias in the REP14 area is due to local (in space and time) differences associated with the regional dynamics. This is precisely what we expect the data assimilation system to be able to correct, as it applies over a larger domain where these errors compensate.

Reviewer:

3) The title should reflect the fact that the study focuses on data assimilation, e.g. "... comparison of data assimilation performance..." or something like that.

Response:

We have changed the title.

Reviewer:

4) The "section 2.2." is clear, but quite compact when discussing the DA scheme. In a DA paper, it is always useful to present one or two equations (not more) of the analysis kernel, since there are several sub-optimal variants of the EnKF (e.g. SEEK, LETKF, EnOI, SEIK etc.).

Response:

The description of the model has been separated in a new section “2.3 – Data Assimilation system” and some equations have been introduced to clarify it.

Reviewer:

5) In "section 2.2." the initial state and ocean boundary conditions of the WMOP are discussed (page 4 lines 12-14). The use of the CMEMS-MED reanalysis is an appropriate option to provide initial/boundary conditions for the seven-year long free run hindcast simulation (used later on to calculate BECs). However, for the seven sensitivity DA experiments, spanning the short period 1-24 June 2014, the analysis CMEMS-MED perhaps would have been a better option (perhaps also the biases would have been smaller). I would like the authors to justify their choices in terms of initial/boundary conditions for the DA short simulations.

Response:

Some hindcast sensitivity tests have been performed using both analysis and reanalysis fields for initial and boundary conditions. A major difference is the consideration of atmospheric pressure forcing in CMEMS-MED analysis, and not in the re-analysis. This introduced some unrealistic high-frequency signal in terms of SLA from the open boundaries of the WMOP domain when using the analysis. This is the reason why we worked in this study with CMEMS-MED reanalysis fields.

Reviewer:

6) Page 6 line 1 "80-member ensemble". Calculating BECs by sampling long simulations it's a nice not expensive alternative compared to stochastic flow-dependent ensembles, but the degrees of freedom are eventually lesser than actually having an ensemble of 80-members (like in an EnKF system for instance). I think the most appropriate terminology in this case is "modes" or "realizations" instead of "members".

Response:

The word “members” has been substituted by “realizations”

Reviewer:

7) Page 6 line 26 "original 1-km resolution data is smoothed and interpolated onto 10km-resolution grid to limit the number of observations". This a common strategy in most DA systems to reduce the computational cost in each assimilation cycle. The most common options are "super-obbing" i.e. averaging/smoothing data like in this study or "thinning", i.e. just sub-sampling the data. Can the authors justify why they choose the one over the other option?

Response:

We generally consider super-obbing as a more appropriate approach since it theoretically allows to reduce the uncorrelated observation errors before assimilation. This is why we applied this approach in this study. However, we didn′t perform any sensitivity test to this particular aspect.

Reviewer:

8) Page 10 line 1 "does not negatively affect the overall performance of the system". The authors are correct with the word "not negative", since in both cases (with/without T, S assimilation) the RMSD compared to the free run is reduced for all variables. But, DA performance for SLA clearly reduces when T ,S are assimilated (see Figs 6 and 7 for SLA). Perhaps, this is not something surprising (covariances can be contaminated for SLA when T, S are injected), but I would like the authors to discuss this effect.

Response:

Page 12 l.15-17: “Notice that the relatively larger SLA RMSD found during the period 10-23 June compared to the spinup period also affects the GNR simulation. Therefore, it is not due to the incorporation of CTD observations, but rather related to the natural evolution of SLA errors”

The relatively lower relative reduction of SLA RMSD when assimilating CTD profiles from 10 to 23 June compared to the reduction obtained during the spinup from 1

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to 9 June is somehow misleading. Indeed, the same behavior is obtained when assimilating the same source of data (SST, SLA and Argo TS: GNR simulation from 10 to 23 June). This indicates that this difference is related to the evolution of the fields rather than the inclusion of high-resolution CTD profiles.

Reviewer:

9) Page 11 lines 7-8 "GNR simulation redistributes these water masses over the domain". In my view, the NO_ASSIM and GNR circulation patterns and water masses are completely different in the REP14-MED domain. I don't see it as a redistribution of these specific water masses. Is this a local coastal effect over the REP14-MED or perhaps a remote effect over the whole WMOP region? Please clarify in the text.

Response:

Altough, the apparent bias in temperature, which has already been discussed, can lead to a misunderstanding, in our opinion, the redistribution of two different water masses can de observed in the potential density fields from figures 10 and 11. Also in the salinity ones from figure 10 which, in this case, have more influence in the density structures than the temperature. This specification has been included in the text. Page 13 l.4 “As illustrated by the potential density maps. . .”

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