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Ocean Science Discussions

Interactive comment on "Optimal adjustment of the atmospheric forcing parameters of ocean models using sea surface temperature data assimilation" *by* M. Meinvielle et al.

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We would like to thank the anonymous referee number 1 for his positive appreciation of the work done in this paper and for his constructive comments that helps us improving the quality of the manuscript. The two main comments are answered below, and a revised version of the paper including the corrections is attached.

General comments

Yes the reviewer is right, the impact of the model used here on the correction computed has not been investigated in this study. However, regarding the model dependency of the forcing corrections, we would like to insist on the fact that many efforts have been made in this study to minimize it, by using strongly constrained experiments to isolate the input parameters error. We have been able thus to identify the forcing error from the whole model error. **Quantifying properly the remaining part of the error which is directly due to a given model implies to run exactly the same experiments but with another model. We could then identify the part of the correction which is dependent to the spatial resolution for example, or to a given formulation used in the model. Unfortunately, this kind of diagnostic would be numerically very expensive, and does not guaranty to capture properly the part of the correction corresponding to the model in all cases but locally in space and time.** To get closer to such an evaluation, we could though envisage to estimate the corrections model dependency by identifying several months which would characterize the main conditions of the given period. For instance, for the 1989-2007 period we could chose the months of January, April, July, and October of 2004 which is a typical year without any significant oscillation event like el Nino. We can then try to identify a seasonal pattern for the model dependency of the forcing corrections, but it still would be subject to important approximations. This is a complex question that could not be addressed by the time of this study, but it would be interesting to go further while the numerical means are improving.

To highlight this point in the paper, the bold sentences in the previous comment have been added in the 4th paragraph of the conclusion (p 2518, line 3).

We agree with the reviewer about the relevance of a diagnostic of the ocean model interior dynamics changes with our corrected forcing. However, the ERAinterim forcing

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was already known to ensure a good interannual variability of the long term dynamics of the model, and we showed in Fig. 10 that our corrections do not modify significantly this variability. **We have not evaluated the consistency of the interior dynamics by comparing our results to observations**, but mostly by the evaluation of the simulation with respect to another one involving another forcing (DFS4). Furthermore, **since the consistency of the corrections is mainly assessed for the intertropical band, it would be first useful to construct a comprehensive forcing dataset including these corrections without introducing any discontinuity in the forcing field before looking at the behavior of the global ocean circulation. However we diagnosed the equatorial undercurrent in the simulation forced by ERAcor and it turned out that the corrected forcing leads to a better representation (in terms of intensity) of this important equatorial circulation feature than ERAinterim with respect to TAO observations.**

We have added the bold part of the previous answer in the second paragraph of the conclusion (p 2517 line 9) to justify this point.

Remarks

p 2494,line 2: done.

p 2495, lines 15-20: We have modified the text as follows to include the fact that the atmospheric parameters from reanalysis have "seen" the SST: "Sea Surface Temperature (SST) is more accurately observed from space than most near surface atmospheric variables or air-sea fluxes assimilated in atmospheric models to construct the reanalyses. Even if the SST is used as boundary conditions in these atmospheric models, large errors remain in the produced surface atmospheric parameters due to bulk formulae and radiative transfer model uncertainties. And in the boundary condition of OGCMs, this observed variable, intrinsically linked to air-sea exchanges,

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is not involved at all except when explicitly assimilated."

p 2499, line 11: The impact of the use of relative wind instead of absolute wind has not been tested in our experiments. Although it is an interesting question, this assesment would imply to run again all the ensemble experiments, which is not possible at this time.

p 2500, lines 17-23: Fig. 1 has been modified by using another figure as bottom panel, showing the difference in the long-term means of turbulent fluxes computed with ERAinterim and DFS4.3 (Brodeau et al., 2010).

p 2502, Eq. 4: Yes, x is the ocean control vector, without any atmospheric parameters. We modified the text as follows: "with x the ocean state control vector containing the SST and the SSS, and p the parameters vector that we aim to correct, where t_2 is the $2\,m$ air temperature, q_2 the $2\,m$ air humidity, u_{10} the $10\,m$ zonal wind velocity, v_{10} the $10\,m$ meridional wind velocity, rad_{sw} the downward shortwave radiation, rad_{lw} the downward longwave radiation, and $precip$ the precipitation."

p 2503, line 13: The sentence has been modified as follows: "We do not know the true value of forcing parameters, neither the perfect initial condition..."

p 2503, line 23: We clarified this choice by modifying the text as follows: "First, to avoid spurious long range influence of the observations, the ensemble covariance matrix will be localized as described in Brankart (2011), using a horizontal cutting length scale of three grid points (i.e. about $600\,km$ in longitude along the equator). Although it is an arbitrary choice, the length scale of 600 km corresponds to the scale of the impact of a monthly perturbation of the forcing. Several possibilities have been tried

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before chosing this one, which is a good compromise between capturing the impact of a monthly forcing perturbation while not introducing artificial large scale correlations."

p 2504, lines 2 and 25: done.

p 2508, lines 10-11: Yes the reviewer is right. This point is already highlighted in the conclusions, but but we have also included the following explanation in the paper itself:"It is assumed that parameters uncertainties are comparable to their intra-seasonal and interannual variability in the ERAi reanalysis. This assumption turns out to be a fair way to identify parameters uncertainties, but one could envisage other options like for example quantifying the uncertainties by using the local differences between two or more forcing datasets."

p 2508 lines 22-23: We have added the bold sentence hereafter in the text to be more explicit: "The parameters prior probability distribution is defined as Gaussian (strictly speaking, a truncated Gaussian to avoid extreme parameter corrections, see section 2.2) and its covariance matrix is derived from the ERAi reanalysis intra-seasonal and interannual variability between 1989 and 2007. Perturbations are specific to a given month and constant over each monthly assimilation window. For example, to estimate a set of perturbations for April, we consider the reanalysis signal corresponding to all three month windows March-April-May of the reanalysis over the 1989-2007 period (60 states). **This Gaussian has a zero mean, and its covariance is defined by the covariance of the parameters around their mean over these three months periods.** This assumption is made to characterize the covariance of atmospheric parameters. The choice of intra-seasonal and interannual variability rather than the total variability is made to avoid in the perturbations of April for example, a variability that is characteristic of winter time. A random sample of 200 perturbations is then constructed for each month."

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p 2509, lines 5-9: We have added this point in the text: “The problem described here is not related to the ice extent variability in the ocean model, since the ocean model is not involved in the computation of the parameters perturbations. The monthly variability of atmospheric variables is much larger at high latitudes, particularly for the temperature and radiative fluxes, mostly because of the position of the sun influence.”

p 2509, line 23: This has been rephrased as follows: “The first important step of the method validation is to look at the results over a specific month (January 2004) which corresponds to one assimilation window.”

p 2510, line 5-8, Figure 3: Since we consider assimilation windows of one month, the actual increment is produced monthly. We have modified the text as follows to be more explicit and avoid confusions. “Figure 3 compares the mean SST increment resulting from the analysis step to the mean SST increment produced in a free run model forced by ERAcor atmospheric variables.”

p 2510, line 18: done.

p 2512, line 12: done.

p 2516, line 3: It is the MOC at 30°S. The text has been corrected. In showing only the 1995-2007 time-serie in the figure, we aim to avoid the spin-up period of the model. We thus consider only the results obtained when the model is stable.

p 2519, lines 7-9: We don’t make reference to any work in particular but more to

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the fact that both in situ and remote observations are experiencing some important developments.

p 2519, lines 15-18: “Results should be first subjected to further evaluation by comparison with available atmospheric or fluxes observations like TAO/TRITON, PIRATA (Pilot Research Moored Array in the Tropical Atlantic), RAMA, and reconstructions especially in the intertropical band like OAflux (Yu et al, 2007), TROPFlux (Praveen et al, 2011) or NOCS (Berry et al, 2009) databases.”

p 2519, lines 26-29: The idea here would remain in a partial coupling of an ocean model with the atmospheric model used to produce reanalyses. This has been clarified in the paper by adding the sentence in bold hereafter. “Conversely, our methodology could also be valuable to provide improved ocean boundary information for the implementation of atmospheric reanalyses, since our approach is not only dedicated to the simulation of the ocean state but can also be viewed as an objective way of controlling the air-sea interactions. **More precisely, one could envisage to improve boundary conditions in atmospheric models by not using anymore the SST directly but the atmospheric parameters corrections produced by the assimilation of the SST in an ocean model.**”

Figures

Fig.1 modification: done.
Fig.4 supplement: done.
Fig.4 and 8: done.
Fig.7: Contours from -2 to 2 $m.s^{-1}$ by 1 (added in the caption).
Fig.10: done.

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Please also note the supplement to this comment:
http://www.ocean-sci-discuss.net/9/C1745/2013/osd-9-C1745-2013-supplement.pdf

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Interactive comment on Ocean Sci. Discuss., 9, 2493, 2012.