

Interactive
Comment

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

Anonymous Referee #1

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Review of “Optimal adjustment of the atmospheric forcing parameters of ocean models using sea surface temperature data assimilation” by Meinvielle, et al.

The authors present a method to optimally correct surface atmospheric parameters by means of sea surface temperature data assimilation. The objective of the paper to improve atmospheric forcing parameters is clearly presented and is an important issue for ocean modeling. The approach used is new and represents state-of-the art in that field. This paper is the continuation of 2 previous studies (Skachko et al., 2009 and Skandrani et al. 2009) performed by the same research team.

The methodology developed and implemented is very clearly described and the results

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are interesting and positive, showing that the method performs well. The authors show that the data assimilation is able to correct the low frequency variability of the surface parameter in a realistic way. The ocean surface net flux is improved, and the data assimilation residuals are reduced compared to the innovations (SST). It is also shown that a long (1989-2007) free run forced with the corrected atmospheric parameters is in better agreement with the observed SST than the control run forced with uncorrected surface parameters.

For the 2 following points, one would have liked having explicit answers instead of a discussion in the conclusions section:

- How much are the corrected surface atmospheric parameters model dependent ?
- Do these corrected surface atmospheric parameters data sets improve some aspects of the ocean model interior dynamics / thermodynamics ?

It would have been very valuable if these points could have been part of the paper.

In spite of these two points not being investigated by the study, and because the results are really new and original, I recommend this paper to be accepted subject to minor revisions.

REMARKS:

p 2494, line 2: the atmospheric → the atmospheric boundary condition

p 2495, l 15-20: Comment: The SST is used by NWP models as boundary conditions but not directly assimilated. However, it is a constraint for the NWP surface variables. The atmospheric parameters have in some sense “seen” the SST but the bulk formulae and radiative transfer model uncertainties produce large errors which are in the end present in the surface atmospheric parameters.

P2499, l 11: It seems that the wind stress calculation has been done by including the ocean surface current in the bulk formulation. Isn't it contradictory with the hypothesis

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made by NWP models which assume $U_{ocean}=0$? How much sensitive are the results to that hypothesis ? Any idea ?

P 2500, l 17-23: In Figure 1b, we have an estimate of the uncertainty related to the radiative fluxes (they are known to have very large errors), which are not turbulent fluxes. Would not it be more instructive to have the sensitivity of the turbulent flux to the surface parameters, i.e. to show a map of $Q_{turb}(ERAI)-Q_{turb}(ERA-40 \text{ or OPER or MERRA or ...})$?? It could be directly compared to Fig. 1a.

P 2502, Eq (4): It is not clearly stated what “x” is (I imagine that it is the ocean model control vector without the atm. parameters)

p2503, l 13 : ... and neither ... : not english

p2503 , l 23: Length scale=600km: Could you justify that choice. Why not using a larger scale ? How does this choice impact the results?

P 2504, l 2: monthly SST → monthly mean SST

p 2504, l 25: isolates → isolated

p2508, l 10-11: This is a subjective choice which turns out to be valid. It may be useful for the reader to point this out.

p2508, l 22-23: Please be more explicit. How is the scaling / normalization of the perturbations done ?

P2509, l 5-9: This is clearly related to the ice extent variability. It should be stated in the text.

p2509, l 23: “by assimilation window”: correct the syntax please.

P 2510 , l 5-8 and Figure 3: The phrasing is a bit confusing between the text and the figure caption / title. The authors talk about “increments” (i.e. a instantaneous fields) in the text and show in Fig. 3 SST mean differences. Could you please avoid this

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confusion and change the text accordingly.

P2510, l 18: for the temperature → for the 2m air temperature

p2512, l 2: strong advection regions → midlatitude strong advection regions

p2516, l 3: is it the MOC at 30°N or 30°S. From Fig 10 it is rather 30°S. In Fig. 10, why not showing the results for the whole period (1989-2007) ? Why not showing the MOC at 26.5°N ??

p2519, l 7-9: Do you have any reference ?

p2519, l 15-18: It could be also interesting to see if ERAcor is closer to in situ observations (TAO/TRITON, RAMA, PIRATA measure atmospheric surface parameters) than ERAint.

p2519, l 26-29: It is not very clear how this could be implemented. Could you be more explicit ?

FIGURES:

Figure 4: It would be useful to have also the zonally averaged surface parameter correction plots next to each map.

Fig. 4 and 8: for q_2 , the scaling factor (10^{-3} ??) should be placed in the figure title next to the unit (its size make it really very hard to read)

Fig. 7: could you please indicate the contour intervals ?

Fig. 10: 30°N or 30°S. It contradicts the text.

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

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