

Interactive comment on “Controlling atmospheric forcing parameters of global ocean models: sequential assimilation of sea surface Mercator-Ocean reanalysis data” by C. Skandrani et al.

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We would like to thank the reviewer for his/her careful reading of the manuscript and for his/her appreciation of the work done in this paper. His/her remarks have been taken into account as explained below, and we have done our best to read the paper again and simplify the text as much as possible.

1. These quantities have been added in the list of parameters included in the computation of the fluxes (except water velocity, because we are only referring to

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heat and fresh water fluxes). Yes, it is indeed important to evaluate the technique using something else than SST. In a sense, this is what we did with Fig. 7 giving RMS errors with respect to the reanalysis for temperature and salinity as a function of depth. We thought it was useful to give first the misfit with respect to the observed quantities SST and SSS (Fig. 4), before measuring the score for unobserved variables. But it is true indeed that this last kind of evaluation could have been more extensive.

2. Done. The definition has been added.
3. Done. “sun height” has been changed to “zenith angle”.
4. In Mercator reanalysis, the assimilated SST maps come from the Reynolds Olv2 weekly product (Reynolds et al. 2002). This information has been added in the paper, together with the reference: "Reynolds, R. W., N. A. Rayner, T. M. Smith, D. C. Stokes and W. Wang, 2002: An improved in situ and satellite SST analysis for climate. J. Climate, 15, 1609-1625."
5. A reanalysis is an estimate of the evolution of the state of the ocean that is obtained by combining optimally the model dynamics and all available ocean observations. It is thus our current best estimate of the evolution of the system. Hopefully, the assimilation system worked correctly and moved the model trajectory in the right direction towards the “real world”. It is in this sense that we say that “differences of model simulations with respect to the reanalysis are similar in nature to differences with respect to the real world”. In order to improve clarity, we have added the following explanation: “It is indeed expected that the assimilation of the real observations to build the reanalysis moved the model trajectory towards a more realistic description of the ocean.”
6. The mask is fixed in time, but is different for temperature and salinity. It is defined as the 10% of the ocean with the largest free run misfit (in SST and SSS), which

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is shown in Fig. 2. This was indeed not clearly stated in the original version of the manuscript. We added the following explanation to clarify the definition of the mask: “The mask is thus different for SST and SSS, and can be visualized in Fig. 2, as the region with the largest misfit (i.e. essentially the Western boundary currents and a part of the ACC).”

7. What needs to be controlled is the dominant source of model error. From a statistical point of view, they can be all mixed in the control vector whatever their physical nature (initial condition, forcing data, parameters). Our assumption was that turbulent exchange coefficients (like C_E and C_H) are much less accurate than other quantities like air density or latent heat of vaporization. The reason for which these last quantities are not included in the control vector is not because they are different by nature; the associated uncertainty is just much smaller (so small that it is also certainly impossible to identify from our observation datasets).

On the other hand, it is true that parameterizing C_E and C_H errors in the same way as for the other variables (i.e. using free model variability) is certainly oversimplistic and that much could be gained from a more realistic parameterization of their uncertainty. A word of caution has been added in the paper to mitigate the validity of our assumptions about C_E and C_H uncertainty: “Using time variability as a uniform way of parameterizing parameter uncertainties is obviously rather crude, especially if a better information is potentially available (as for C_E and C_H), but this is a useful simplification for the definition of statistics and the interpretation of the results. With this method, it is also easy to obtain directly a reduced rank parameterization of the covariance matrix.”

8. Yes, it is true that our explanation was probably insufficient to justify the decision not to include the wind in the control vector. We just wanted to proceed step by step to avoid introducing unpredictable difficulties in the estimation problem (see also answer to reviewer #1, with the text that we have added to clarify this point).

On the other hand, since wind speed is indeed an important source of error in the computation of heat and fresh water fluxes, ignoring the wind certainly produces additional compensation problems in the estimation of the control parameters (in the same way as error in the ocean dynamics are sometimes compensated by unrealistic parameter corrections). We added the following text in the discussion of the results to include the possibility that part of the parameter corrections are applied to compensate for wind errors: “In this, we must also include compensations for wind errors, which have not been included in the control vectors of the assimilation scheme (see section 3.2).”

9. Units of years and months have been added in Fig.1.

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