Ocean Sci. Discuss., 11, C685–C689, 2014
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Ocean Science Discussions
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Interactive comment on "Argo data assimilation into HYCOM with an EnOI method in the Atlantic Ocean" *by* D. Mignac et al.

Anonymous Referee #1

Received and published: 8 August 2014

General comments

Overall the paper is quite well written and structured, although there are a few errors in usage and syntax that are inevitable when the authors do not have English as a native language. The model description is clear, although one or two details are omitted (see comments below). The technique of Ensemble Kalman Filter is adequately described, and the results from assimilation using this method using two different datasets (NCODA and ARGO) are clearly summarised.

The result that the assimilation did not significantly improve the circulation in the model is a notable negative result, and in my opinion needs more discussion. Indeed, the assimilation seems to make the equatorial circulation less realistic than in the control, both with NCODA and ARGO data. The authors should add a sentence or two of

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discussion of why this might be the case, and preferably relating it to changes (or lack of change) in the density structure close to the Equator. It may be that the model has insufficient resolution to simulate this circulation, or perhaps the equatorial flow features are more directly controlled by the wind stress and basic fluid continuity than by the geostrophic balance that holds over most of the ocean interior, so are less sensitive to adjustments in the temperature and salinity fields.

It seems to me that the large-scale circulation is a major deliverable of an operational model, and the lack of dynamic consistency in the EnKF methodology is a real shortcoming. If the advective flow in the model is incorrect, this will lead to it having an additional tendency towards an incorrect tracer distribution (beside the effect of any other biases in the model, for instance excessive vertical mixing or surface flux errors). The logical extension of this is that the actual choice of model (here HYCOM) becomes almost irrelevant.

Despite this, my feeling is that the paper makes a valuable contribution to the field and is merits publication with minor corrections.

Specific comments

2. HYCOM and its configuration

P1738.L26: Why stop at 50°N? The phrase "... almost all the Atlantic Ocean..." doesn't seem to be justified here! I think the authors should explain why they have excluded the climatically important subpolar region from this study.

A justification should also be included in this section for the decision to use surface pressure here as the reference for potential density. A pressure of 2,000 dbar (sigma-2) is the de facto standard these days in isopycnic models, for the chief reasons that it distinguishes AABW and NADW, as well as giving smaller pressure gradient errors in the ocean interior.

P1739,L10: Are you relaxing to monthly or annual mean lateral boundary fields? I

OSD

11, C685–C689, 2014

assume the former, but this need to be stated explicitly.

P1739,L27 and foll.: in the context of this paper some brief speculation might be appropriate as to the reason for the model discrepancies. Are these typical of comparable non-assimilating implementations of HYCOM? Might they be related to the forcing, to the resolution, or to a generic limitation of HYCOM itself?

3.1 Calculation of the innovation vector

Figure 2: I'm not convinced that the "light black dashed lines" to show the HYCOM target densities are a good idea: they don't appear correctly on my printed hardcopy of the PDF, and will probably be redundant anyway when the figure is reduced for the final typeset paper.

5.1 Comparison of mean states

P1749, L18 and foll.: Why is the absence of mass flux at the northern boundary relevant or interesting in this context?

P1749, L20: "It was already expected that the control run would have larger biases around the middle latitude band. . .". Are you referring to your comments on Figure 1 in Section 2, or is this an expectation based on previous knowledge? Why would you expect these biases?

P1751, L2: The raising of the layer interfaces is one possible consequence of the changing of density as a result of assimilation; convection resulting from a water column becoming statically unstable with respect to the underlying water masses is another. Can you confirm that the latter does not occur in extreme cases?

5.3 Adjustment of the altimetry and velocity fields

As the authors state, Figure 12 confirms that there is a general decrease in sea surface height. It would be helpful if a time series of the individual thermosteric and halosteric tendencies were added as additional panels to this figure to compare the relative im-

OSD
11, C685–C689, 2014

pacts of the two contributions.

Technical corrections

Abstract: The acronym “HYCOM” should be expanded. “RMSD” should also be expanded.

1. Introduction:

P1735, L21: insert “a” before “few”. This error occurs several times in the manuscript.

P1735, L24: replace “until” by “down to”

P1735, L25: replace “millennia” by “millennium”

P1736, L7: replace “taken from a previously done model run. . .” by “derived from an existing model run. . .”, or similar.

2. HYCOM and its configuration

P1740, L1: ”bellow” should be “below”. This spelling mistake is repeated several times in the manuscript.

3.1 Calculation of the innovation vector

Figure 2: I’m not convinced that the “light black dashed lines” to show the HYCOM target densities are a good idea: they don’t appear correctly on my printed hardcopy of the PDF, and will probably be redundant anyway when the figure is reduced for the final typeset paper.

3.3 Generation of a running ensemble

P1743, L13: “Many works show. . .” is a clumsy expression; perhaps replace with something like “Many authors have shown. . .”. Also, “sensitive” is more correct than “sensible” in English in this context.

5.1 Comparison of mean states

OSD

11, C685–C689, 2014

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P1750, L13: “the control run is not representing very well. . .”; not very idiomatic English: replace with “the control run does not represent well. . .”

The standard abbreviation for Antarctic Intermediate Water is “AAIW”, not “AIWM”.

P1752, L27: “on a daily basis”, rather than “in a daily basis”.

Conclusions and Discussion

P1762,L3: “evidence”, not “evidences”.

P1762, L13“The present work was a key-step towards two major directions..”; it is more like a “key step forward in two major directions”.

Interactive comment on Ocean Sci. Discuss., 11, 1733, 2014.

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