Ocean Sci. Discuss., 3, S864–S875, 2007
www.ocean-sci-discuss.net/3/S864/2007/

Ocean Science Discussions

Interactive comment on "Effects of mesoscale eddies on global ocean distributions of CFC-11, CO_2 and $\triangle^{14}C$" *by* Z. Lachkar et al.

Z. Lachkar et al.

Received and published: 15 July 2007

1 General Comments:

Ref. #2 states that the interpretation is too restricted to the model results. He also says that more attention should be paid to transport patterns in the real ocean (specific comments #4-8).

Much of our analysis is focused on model-model comparison because our main goal was to quantify a potential systematic bias associated with simulated anthropogenic CO2 uptake estimates, which until now have come only from coarse-resolution models. That said, we agree that it is also important to check our results against real observations. To improve this aspect, as mentioned above, we now include additional

model-data comparison along two new vertical sections (see Figs 11 and 12 in the revised manuscript). Also, further diagnostics regarding the transport patterns have been added as well (Fig 21). Please also see our responses below to specific comments 4-8 and 10.

Ref. #2 says that the Southern Ocean model-data comparison is not the best.

Our focus is less on the Southern Ocean (all waters south of 60°S as defined by the IHO in 2000) and more on waters to the north where a much greater proportion of these three anthropogenic tracers are stored. To avoid 'confusion', we now use 'Southern Ocean' to refer only to the region south of 60°S; otherwise, we use terms such as 'southern extratropics'. This region, south of 20°S, is where ocean anthropogenic CO2 and CFC-11 uptake are largest and where OCMIP models disagree most.

Ref. #2 suggests that more discussion be devoted to describing why bomb C-14 behaves differently than the other two tracers.

As described in further detail in our responses to specific comments 5 and 6 (below), the discussion of the 'singular behaviour' of bomb C-14 has been expanded in the revised manuscript. In particular section 4.3 has been rewritten with this in mind .

Ref. #2 mentions that units on figures are sometimes missing or wrong.

Units on figures have now been included where they were missing and modified when wrong. See our responses to technical comments 4, 5, 6, 7, 9 and 12.

2 Specific Comments:

1)

Ref. #2 requests that we mention that the finer grid is only eddy-

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permitting in both Abstract and Introduction

Done. See Abstract [line 5] and Introduction [line 72].

Ref. #2 is surprised that the 4-fold increase in resolution is adequate to do nearly the 'full job', implying that higher resolution may not be needed and that this is only addressed in passing in the Summary section

Unfortunately, there is no way of knowing if a grid finer than that of our eddying model is actually needed, i.e., until one actually makes a rigorous sensitivity test at finer resolution (which we must leave for future work). In the revised manuscript, we raise this issue earlier (lines 111-115). Please see our more detailed response to Ref. #1's comment 4 on this same issue, where we mention that resolution might be sufficient to have reached a threshold, beyond which further increases in horizontal resolution will not substantially alter large-scale patterns of uptake and storage of tracers like CFC-11 and anthropogenic CO2.

Ref. #2 suggests comparing in detail our results to the Sasai et al (2004) model

We have made a limited comparison of our simulated CFC-11 inventories to those of Sasai et al. (2004) in Fig. 9 of the revised manuscript. However, our main goal here is to demonstrate the effect of improved resolution using a rigorous sensitivity test with different versions of the same model. Comparing our results in more detail to the higher resolution Sasai et al (2004) model would be interesting, but given that there are many other differences besides resolution (e.g., their lack of a sea-ice model and their restoring of temperature and salinity to observed values throughout the water column at the southern boundary), we could never be sure as to what actually causes the model-model differences.

2)

Ref. #2 suggests that we should bring up the GM simulations earlier in

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the paper.

Done. In the revised manuscript our GM simulations are first presented in the Methods section (lines 118-125) and then the GM results are compared to the non-GM simulations and the data at the beginning of the Model evaluation subsection of the Results (lines 329-340).

Ref. #2 mentions that we use the Laplacian formulation in the non-eddying model but the biharmonic formulation in the eddying model, thus favouring the latter.

Certainly the biharmonic formulation 'favours' the eddying model. But there is no advantage when using that formulation in a coarse resolution model. Please see our more detailed response to a similar remark from Ref. #1 (his specific comment 3) and lines 170-176 in the revised paper.

3)

Ref. #2 emphasizes that the tracer input parameterisation for CFC-11 adds perhaps $\pm$15% uncertainty, limiting the conclusion that the eddying model is 'so much superior'. He also mentions that tracer inventories for both model versions are low in the higher latitudes.

Unlike for anthropogenic CO2 and bomb C-14, CFC-11 is a purely an anthropogenic tracer and its air-sea exchange function depends much less on wind speed. Hence the associated uncertainty for CFC-11 could perhaps be less than the $\pm$15% mentioned . Differences can also be much more than 15% in some key regions such as between 60°S and 40°S. As for the very high southern latitudes, we have added (line 354-355) the statement 'Additionally formation of Antarctic Bottom Water (AABW) may be too weak' in order to make it clearer that the high-latitude inventories are problematic due to many model deficiencies, even though these are areas where little tracer is stored and are not the focus of this work.

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4)

Ref. #2 suggests that model agreement with the observations may be 'right for the wrong reasons' and that the revised version of the manuscript should include and discuss model-data comparison along a full Atlantic section, including deep waters.

We have addressed this concern in the revised manuscript by adding two vertical CFC-11 sections: one in the South Atlantic (Fig. 11) and another in the North Atlantic (Fig. 12); See corresponding text in lines 365-377.

Ref. #2 points out that there is no comparison for deep waters

We now better justify our focus on the upper ocean in the revised manuscript (section 3.2, lines 285-291). Following recommendations from Ref. #1, we prefer to focus this manuscript on decadal-scale changes associated with surface waters and ventilation of the intermediate waters in the Southern Hemisphere where air-sea tracer fluxes and storage are largest, and to avoid drawing conclusions about the deepest water masses which play a substantial role in tracer uptake and climate on centennial or longer time scales. Please also see our response to Ref. #1's General comment.

5)

Ref. #2 points out that the atmospheric history of bomb C-14 is also very different from that for CFC-11 and anthropogenic CO2, and that that (along with the corresponding different effective time scales in the atmosphere) may partly explain why bomb C-14 is less sensitive to increasing horizontal resolution.

Following this insightful comment, we have added a new figure (Fig. 22) to show the atmospheric history of each of the three tracers. Additionally, to discriminate between factors, we made another type of C-14 simulation, where we account for only the Suess effect, i.e., the reduction in atmospheric C-14/C-12 ratio due to the emission of fossil

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CO2 during the industrial era until just before the beginning of the bomb era (from 1839 to 1950) in both the eddying and the non-eddying models. The revised manuscript shows that the atmospheric history also matters, but the main reason why bomb C-14's response to increasing resolution is much weaker than for the other tracers is due to its much longer air-sea equilibration time (see lines 523-539).

Ref. #2 requests that we modify the statement 'These dissimilarities are due to the differences between the three tracers in terms of their in air-sea equilibration times and solubilities' (P. 1022, line 17)

This statement has now been modified to read 'These dissimilarities are due to the differences between the three tracers in terms of their air-sea exchange equilibration times and atmospheric histories (i.e., different forms and rates of change).' See lines 254-256 of the revised manuscript).

Ref. #2 states that the longer mixed layer residence times [in the eddying model] assist rather than penalize bomb C-14 uptake. Whereas CFC-11 uptake must diminish with longer equilibration time of the eddying model, the C-14 uptake just continues.

We agree. We have added text (section 4.3, lines 505-521) as well as a new figure (Fig. 21) to eliminate confusion about this issue See also our response to comment #6 (below). .Relative to C-14, surface concentrations of CFC-11 and anthropogenic CO2 are relatively much closer to equilibrium with the atmosphere. The increased mixed layer residence time associated with moving to higher resolution brings their air-sea fluxes closer to zero (equilibrium with that atmosphere). In contrast, surface-water bomb C-14 levels remain far from equilibrium with the atmosphere because much more time is needed for bomb C14 in the mixed layer to equilibrate with a C-14 perturbation in the atmosphere (isotopic equilibrium). Although the longer mixed-layer residence time in the eddying vs. non-eddying model, does allow for some increase in surface levels of bomb C14 (Fig. 21) that increase only reduces the air-sea difference by a small

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amount (e.g., a 10% reduction at 53°S, which is much less than the 50% reduction for CFC-11). Furthermore, this increase does not extend below the thermocline (Fig. 21 in the revised manuscript). Thus the overall inventory (storage) is hardly affected.

6)

Ref. #2 asks whether the differences between the three tracers are localized only in the mixed layer. He also requests to explain how the structure of the interior distribution of bomb C14 is affected by resolution change.

To address this concern in the revised manuscript, we added Fig. 21, showing vertical profiles of bomb C14, anthropogenic CO2, and CFC-11 at 60°S and 40°S in both models. Increasing resolution leads to higher bomb C-14 concentrations near the surface but reduces the vertical tracer penetration into the intermediate and deep ocean. The bomb C-14 inventory changes very little, but the bomb C-14 vertical gradient differs more. For CFC-11 and anthropogenic CO2, surface concentrations change less despite the increase of the mixed layer residence time, as explained in section 4.3 (lines 515-526).

7)

Ref. #2 states that invoking just mixed layer depth is insufficient to explain why the vertical penetration of tracers in the southern high latitudes is too weak

Certainly, the poor performance of the model in the southern high latitudes could result from other factors besides problems with the mixed layer. To make this clearer, we have modified the revised text (see section 3.4.2, lines 355-359).

8)

Ref. #2 points out that the use of the isopycnal transport stream function may be misleading for deep waters (especially for the separation between LCDW and NADW)

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We chose to use the sigma-0 coordinate because it is the best choice for characterizing near-surface water mass transformations in the southern high latitudes. Certainly though, it is a poor choice for deep waters, and Ref. #2 is correct to point out that the separation between NADW and LCDW may be misleading in this context. Therefore, in the revised manuscript we now regroup denser water masses (NADW, LCDW, and AABW) as a unique layer, i.e., the 27.6-28.1 density class, see section 4.2, lines 465-468).

Ref. #2 states that the presence of a small 'Deacon Cell' and the transfer from LCDW to AAIW seems unrealistic

Excessive upwelling of dense water near the Antarctic Divergence (and the associated Deacon Cell) in the non-eddying simulation is clearly unrealistic but this is not surprising given that this model has no eddies. As might be expected, lack of eddies in the coarse resolution model means that the meridional circulation in the upper Southern Ocean is too strong. This point is now better addressed in the revised manuscript (section 4.2, lines -469-490)

Ref. #2 thinks that the statement 'most of the formation of AAIW results from UCDW conversion from below the annual maximum of the mixed layer depth' and the rest of that paragraph sound a little strange

This sentence has been deleted in the revised manuscript. Further, we have rewritten the paragraph concerning the link between AAIW ventilation and the residual circulation. (see section 4.2, lines 469-487).

Ref. #2 asks which depth/density/water mass range is meant by 'upper ocean'?

By "upper ocean", we refer to waters above the permanent thermocline. This has been made clearer in the revised manuscript (section 4.2, line 454).

9)

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Ref. #2 suggests that the graph in Fig. 15 should stop at the equator

Done. See Fig. 16 in the revised manuscript.

10)

Ref. #2 asks how is the northward transport (Fig 7) divided between the upper waters, AAIW, and the still deeper waters?

We have done a separate analysis of how northward transport is divided between upper, intermediate, and deep waters, but results are already included in another manuscript focused on just that topic, i.e., depth variations in regards to northward transport of transient tracers and heat. Thus we are unable to add the same material here. However, it can be surmised from the revised manuscript (by comparing figures 7 and 17 and reading what we say about the importance of SAMW [lines 432-440]) that the surface waters dominate northward transport and that AAIW also plays a role. This is consistent with our separate analysis that will be published elsewhere..

11)

Ref. #2 asks if there is any explanation for the differences in the northward transport of the three tracers?

The northward eddy transport is not the same for the three tracers mainly because of their contrasting vertical distributions in the upper southern extratropics. For instance the north-south distributions of anthropogenic CO2 and CFC-11 have opposite gradients within the thermocline. This point is now mentioned in section 3.3 (lines 319-321) of the revised manuscript.

12)

Ref. #2 would like us to clarify the discussion of Table 1, p. 1024: 'Two processes appear responsible' (line 6)

This point is now better addressed in the revised manuscript (lines 294-304). We also

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deleted the last bothersome sentence referred to by Ref. #2.

13)

Ref. #2 asks how the present study could help the search for more reliable eddy parameterisation?

Our work highlights the need for improving the GM parameterisation within the surface ocean in order to also represent the diabatic effect of mesoscale eddies (see section 3.4.1, lines 334-340).

14)

Ref. #2 suggests a word of appreciation to the data providers in the Acknowledgements section

Done.

3 Technical Comments:

- p. 1013, line 1, the statement 'cannot be measured directly' is correct, but inadequate in the context

In the revised text, we have qualified this statement by adding a follow-up sentence that mentions that although data-based estimates of ocean anthropogenic carbon are available, the associated uncertainties are sometimes large (see lines 30-32).

- p. 1022, line 17: 'and solubilities' is incorrect in the context

This has been corrected in the revised text (see 2nd part of response to specific comment 5, above).

- Fig. 1: The related depth interval should be given in the caption. The

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non-eddying graph should be left out because it shows no structure at all.

Done. The EKE map was calculated at the surface of ocean. We have removed the EKE map for the non-eddying simulation.

- Fig. 3 and various others correspondingly (Figs. 5, 8, 12, 13, 15, 19): The ordinate is given in 'percent of global uptake' etc., but that misses references to the related meridional scale, probably 'per degree of latitude'.

Done.

- Fig. 4: units are fully missing, and model version should be stated in the caption.

The model version is now stated in the caption. There are no units because of normalization.

- Fig. 6: units are missing, and caption should give the model version. The colour bar is somewhat inadequate in that colours other than blue are found for CFC-11 in the western N. Atlantic. A suitable nonlinear colour scale would be preferable

The figure caption now mentions the model version and states that there are no units because of the normalization. The colour bar now uses a nonlinear colour scale.

- Fig. 8: Unify ordinate units.

Done

- Fig. 9: Figure does not cover the entire Southern Hemisphere, in that the caption is incorrect.

In the revised caption, we have now changed 'Southern Hemisphere' to 'southern extratropics'.

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- Fig. 10: Units are missing. Caption, to ease identification: 'The DASHED purple curves'

In the revised text, the units are now included and we have changed 'purple' to 'dashed purple'.

- Fig. 16, clarify the caption

The caption was fixed.

- Define the 'saturation index' in the caption

Done.

- Axis lettering is small

Axis labels are now much larger.

Interactive comment on Ocean Sci. Discuss., 3, 1011, 2006.