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Interactive Comment

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

Anonymous Referee #2

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General Comments The paper presents modeling results for CFC-11, anthropogenic carbon, and bomb carbon-14, comparing simulations by means of a state-of-the-art global OGCM for different horizontal resolution, with emphasis on the southern ocean/southern hemisphere. Comparing to observations, it is found that the coarser model, like found in previous model intercomparisons (e.g., Dutay et al., 2002), overestimates the tracer input, while the finer model (resolution fourfold the coarse one) gives quite realistic results. It is also found that the difference in ocean input between the models is the highest for CFC-11 and the lowest for anthropogenic C-14, which is ascribed to characteristic air-mixed layer exchange times strongly increasing from CFC-11 up to bomb carbon-14. The different model performance is attributed to eddies being absent in the coarse model version, but present in the finer one, which in

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turn induces a strengthening of the upper-ocean stratification.

The subject of the paper is highly relevant in the context of quantifying the ocean sink of anthropogenic carbon, especially in the development of models that allow a realistic prediction. The paper represents a thorough piece of work involving novel concepts and ideas in an admirable effort. It is among the first modeling studies that compare results of a non- eddying and an eddying version of one and the same model for the real, global ocean. The implied extreme demand on computer power is resolved in favorable way (perturbation approach, offline tracer simulation). The simulation results are presented and discussed in detail. For this and for the basic subject, the paper is suitable for publication in Ocean Science. Abstract and Introduction are fine and the references to me look fully adequate. However, I judge presentation and interpretation in part as unsatisfactory, making a revision necessary. Interpretation is too much restricted to the mere simulation results, and to the topics mixed-layer depth, air-water exchange time of the tracers, and presence/absence of eddies. In my view more attention should be placed on the transport patterns in the real ocean, (see my specific comments #4 - 8 below). The authors claim emphasis on the Southern Ocean (p. 1032, line 6), but just there the comparison with data is not the best. I would like to see more discussion why bomb C-14 behaves that different from the other two tracers. A special point is that units in the figures are sometimes missing or even wrong.

Specific comments 1. Even the finer model grid is only eddy-permitting, as the authors also state (p.1016, line 15, but that should be pointed out in the Abstract and Introduction already). It is therefore surprising that the four-fold resolution increase should do about the “full job”, so that a still finer grid (which of course would pose an even more severe computer-power problem) might not be needed. That item is addressed in passing in the Summary section. On the other hand, the paper makes reference to the CFC-11 model simulation of Sasai et al. (2004), which does have appreciably higher resolution, and appears to do better in the Southern Ocean (Fig. 10). I suggest comparing with the Sasai et al. results in a little more detail to possibly get some clue

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on the “full job” question.

2. The authors state that coarse-resolution models usually use sub-grid parameterization for mixing, but they deliberately omit that for their own coarse resolution version, “to better capture the effect of mesoscale eddies Ě” (p. 1016, line 11-13). This choice reduces unnecessarily the potential of their coarse model. The point is then addressed in Fig. 19, where runs with and without Gent-McWilliams parameterization (GM) are presented. Apparently, GM does not make too much difference for the coarse model (although it gives the best agreement with data at the critical high southern latitudes). The item, GM or not, should be addressed earlier in the paper, such as in Section 2.2 already. Moreover, Laplacian formulation is used for horizontal viscosity in the coarser model and biharmonic in the fine one, which also gives an extra push for the latter model.

3. Table 3 gives data uncertainty margins (only global integrals), according to which only the CFC-11 inventory is significantly better for fine than for coarse, whereas for the other two tracers such a distinction remains more or less undecided. One must furthermore consider that the tracer input parameterizations introduce uncertainty, perhaps +-15%?? This limits the conclusion that the fine version is “so much superior”, and by the way, the tracer inventories at high southern latitudes are low for both versions.

4. It follows that it cannot be ruled out that the agreement with the observations is “right for the wrong reason”. In that context I note that the comparison with data is overwhelmingly in integrated form (zonally integrated tracer inventories etc.; even that is not always very good, e.g., Fig. 19, ˜65°S, 35°N). The only detailed comparison is one for CFC-11 along a meridional section in the South Indian Ocean (Fig. 11), which does show a really good agreement between data and the finer-grid model result, and significant improvement relative to the coarser model. However, this comparison essentially deals with the Central Water and AAIW ranges only. There is thus no comparison for the still deeper waters, especially for NADW and AABW, which do add to the tracer inventories. NADW, for example, should have taken up something like 10 Mmol

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of CFC-11 or even more, not a negligible figure. I would expect that the model, which is of z-level type with 250 m vertical distance for the deepest layers, does not perform very well with respect to the deep and bottom waters. That may not be a real drawback for the purpose of the paper but the point should at least be addressed, for example by showing, and discussing, a full Atlantic CFC-11 section.

5. Much reference is made to the different air-water exchange time scales (beginning with last line of Abstract), but it should not be ignored that the input history of bomb-14 is not only quite different from that of the other two tracers, but also in itself different between the northern and southern hemispheres. An additional figure showing the various time histories might be indicated. Furthermore, the time scale of the atmospheric tracer transient (probably highest for anthropogenic carbon) also plays a role. An example of a statement that might better be modified is in p. 1022, line 17. Beyond this, bomb-C-14 input is largely by an imposed flux, i.e., hardly ever approaches an air-water equilibrium, whereas CFC-11 input does tend toward equilibrium, i.e., toward vanishing flux. This means that longer mixed-layer residence times (p. 1030, line 25 ff.) assist, rather than put a disadvantage for, bomb C-14 uptake, because in that case CFC-11 runs closer to equilibrium which reduces uptake, while bomb C-14 uptake just continues. Could that add to the fact that for bomb C-14 differences between coarse and fine resolution are small?

6. The conversion of the different air-water exchange times of the three tracers into differences in inventories/fluxes repeatedly refers to mixed-layer depths (e.g., p.1027, line 17). However, the uptake into the real ocean more acts as a number of R-C elements in series, each with its specific time constant, with the longest time scales dominating. Of course, air- sea exchange for bomb C-14 is slow, but there are additional elements further downstream into the ocean interior that have similar or even longer time scales, and these naturally do not discriminate between the tracers. Such long time scales probably hold for most of the transfer beyond the mixed layer. Elements of smaller time scales have little effect, and that may make bomb C-14 less dependent on model

resolution. But turning the argument around, short time scales are mostly present in the upper ocean, which might suggest that the higher resolution makes a difference primarily there?? If it were only for a lower saturation of the mixed layer, then interior bomb C-14 concentrations would just be lower by a certain factor, while the structure of the interior distribution would be about the same as for the other tracers. I would welcome a more elaborate explanation why bomb C-14 is so little dependent on model resolution (Fig. 4, etc.) for which I do not see any straightforward explanation.

7. A related item is mixed-layer depth in the Southern Ocean that is very low in the fine model (Fig. 9). But here the transfer from the surface downward is particularly complicated - ice cover in winter with upwelling of Warm Deep Water, and surface-layer stratification in summer due to freshening by sea ice melt. Downward tracer transport occurs either by subduction toward the north or by deep/bottom water formation at the southern continental margins. In other words, invoking just mixed layer depth is probably insufficient.

8. The isopycnal transport stream function (Fig. 16 and p.1029 to 1030) refers to potential density, which is misleading for the deep waters. For example, as Joe Reid has shown, NADW in potential density appears as denser than LCDW (such as it appears in the figure), which at depth is not true. It is perhaps for that reason that the figure still seems to contain some smaller “Deacon cells”. I am also wondering about the statement that the fine model has 15 Sv less LCDW-AAIW transfer than the coarse one. Such transfer in my view is virtually absent in the real ocean. The following sentence says that “most of the formation of AAIW Ě results from UCDW conversion from below the annual maximum of the mixed layer depth. Ě”. But then, how can AAIW obtain the high tracer load?? The rest of that paragraph sounds also a little strange. The high tracer load of AAIW even in the fine model must be due to much contact with the atmosphere. I would like to see an account of how the required ventilation is accomplished in the model. - p. 1029, line 24: Which depth/density/water mass range is meant by “upper ocean”?

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9. Fig. 15 gives the AAIW CFC-11 inventory versus latitude, defining AAIW by a certain range in potential density. It should be noted that in the North Atlantic AAIW is very low in CFC-11 (see e.g., Ocean Circulation and Climate, Fig. 5.8.12), so that the graph should stop at the equator.

10. Fig. 7 shows the northward tracer transport versus latitude, displaying a huge peak near 40°S. It would be good to know how that transport is divided between the upper waters, AAIW, and the still deeper waters.

11. Difference in southward tracer fluxes, p. 1025, line 3 ff.: How could that difference come about??

12. Discussion of Table 1, p. 1024: "Two processes appear responsible Ě" (line 6), leading on to "(ii) the decrease of eastward transport due the eddy activity Ě", and finally to the statement "one might also expect that (eddies) contribute to meridional transport Ě". The basis for these statements to me is obscure. The eastward transport argument is repeated on p. 1032, line 17-18.

13. Reliable eddy parameterization is mentioned in p. 1013, line 23, as a subject that must be studied, but I miss a word in the Summary section on how the present study could add to that task.

14. It is true that all observational data used in the study are "free to use", but a word of appreciation, anonymously, to the data providers in the Acknowledgements section might be in order.

Technical corrections 1. p. 1013, line 1, "Ě cannot be measured directly": Statement is correct, but inadequate in the context, since results of indirect determinations are used heavily throughout the paper.

2. p. 1022, line 17: "and solubilities" is incorrect in the context, since the specific solubilities for anthropogenic carbon and for bomb C-14 are already contained in the "equilibration times".

3. Fig. 1: The related depth interval (vertical mean, or whatever) should be given in the caption. The non-eddying graph should be left out because it shows no structure at all. To state that in wording is fully sufficient. A comparison to observations would be far more meaningful, but it must be made sure that the related depth interval is the same.

4. 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 reference to the related meridional scale, i.e., probably, “per degree of latitude”.

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

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

7. Fig. 8: Unify ordinate units.

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

9. Fig. 10: Units are missing. Caption, to ease identification: “The DASHED purple curvesĚ”

10. Fig. 16, caption, “mixed-layer depth (m)”. What is probably meant to say is something like position in density space, so no “(m)”. I take it that positive circulation means anticlockwise, but that should be clarified in the caption.

11. Fig. 17, ordinate and caption: “saturation index” is uncommon, or would at least have to be defined. The literature uses “relative saturation” or the like, which however should also be defined.

12. In the available .pdf version on the web, axis lettering is small, making reading difficult. It should be ensured that fonts are of adequate size.

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