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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.

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1 General Comments:

Ref. #3 suggests that the model-data comparison should have been done before comparing the both models

We start the Results section by comparing the non-eddying and eddying models because our main goal was to test if simulating eddies makes a difference in terms of large-scale transient tracer uptake and storage. That seems to us to be the most logical starting point given our goal.

Ref. #3 would like to see a discussion of the tracer uptake in lower water masses

Ref. #1 made a similar comment, but also suggested one option (involving only minor revisions) where we should focus on the AAIW and ventilation of the thermocline in the Southern Hemisphere (see our response to Ref. #1's General comment). This is certainly our approach in this manuscript where our main interest is studying the region where the majority of the three transient tracers are stored. In our eddying simulation, about 80% of anthropogenic CO2 that is taken up by the ocean is stored in the upper 1000 m, whereas for both CFC-11 and bomb C-14, near surface waters store somewhat more ($\sim$ 90%), due in part to the shorter atmospheric history. In the Southern Ocean, there is relatively little anthropogenic CO2 associated with the formation of AABW in both versions of the model because of a weak formation rate of this water masses in our model. Furthermore, our model's 46-level vertical resolution may be inadequate to properly represents AABW formation. In the revised manuscript, we now provide better justification for our interest in the upper ocean (see lines 285-291). The only place where substantial concentrations of anthropogenic CO2 and CFC-11 penetrate to mid and abyssal depths is in the North Atlantic owing to formation of NADW. We have added CFC-11 section in the North Atlantic (Fig. 13) to make comparison of the tracer penetration between the two versions of the model.

2 Specific Comments:

1) Ref. #3 points out that the statement 'increased eddy activity reduces' (P. 1012/8-10) is somehow misleading

In the revised manuscript we now say 'refining the horizontal resolution reduced'.

2) Ref. #3 suggest clarifying (P.1013/7-9) whether the disagreement is between different models or between model(s) and observations

Our focus is on disagreement between models.. The phrase has been changed to 'The

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OCMIP models disagree substantially...'

3) Ref. #3 suggests adding other references to publications outside model community papers dealing with ocean tracer uptake from a different approach (P. 1015/11-12)

Our revised manuscript now includes a reference to the work of Waugh 2003 (section 2.3.1) , which discusses the use of CFC-11 to characterize the timescale of ocean ventilation.

4) Ref. #3 asks to what extent 'full resolution' might change the findings presented here

This is impossible to know until one actually makes a rigorous sensitivity test with comparable simulations at higher resolution, which must be reserved for future work. Please see our response to similar remarks from Ref. #1, comment 4 and Ref. #2, comment 1.

5) Ref. #3 asks about the difference between online and offline performance for the eddying model

We have improved the discussion concerning of our offline approach at eddying resolution (lines 146-150). Please also see our response to Ref. #1's related comment #1.

6) Ref. #3 asks that we justify our use of different horizontal viscosity formulations in the two versions of the model

The use of different horizontal viscosity formulations in is now better justified in the revised manuscript (section lines 170-176). Please see our response to Ref. #2, comment 2.b.

7) Ref. #3 asks about the precision of the observations for CFC-11 as well as the other tracers. (mentioned in P1019/18) and for the other two tracers

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In the revised manuscript, we now state that the precision of the WOCE CFC-11 data is ∼1%, but that the error estimates for the other two transient tracers are substantially larger and difficult to quantify because they are only derived estimates based on other data (section 2.3.1, and section 1, lines 30-32).

8) P. 1021/7: '280 ppm' versus p. 1020/17: '278 ppm'. Did Siegenthaler and Joos (1992) used a different number or is this important?

Siegenthaler and Joos (1992) as well as Sarmiento et al (1992) used 280 ppm as their reference. In this study we used 278 ppm as the preindustrial reference (as did all the OCMIP models during the past 10 years). This is referred to in the main text and in the appendix although it is provided more for specialists that are interested in reproducing the same type of simulations with identical forcing.

9) Ref. #3 asks the reason why the general features (P. 1022-1025) are discussed only on the non-eddying model results?

We use the coarse-resolution class of models (the non-eddying model) as a reference because until now all global simulations of anthropogenic CO2 uptake have been made with such models. Then after establishing that there are general differences between tracers, which justifies our 3-tracer approach, we go on to establish how those general features change as we move to higher resolution.

10) Ref. #3 states that the statement (P 1022/17-18) as it is written might be misleading or could be interpreted incorrectly.

We have now clarified this statement by adding another sentence 'When combined with local differences in stratification and surface-layer residence times, these lead to different tracer uptake patterns.' (lines 256-257).

11) Ref. #3 points out that from Table 1 (P. 1023/6-7) only 14C seems uniform, but not CO2

In the revised manuscript, we have rewritten that sentence, making it clear that the

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discrepancy between basins is less for CO2 than for CFC-11 (lines 270-271).

12) Ref. #3 would like us to clarify what is meant in the sentence 'Ĕ is smaller.'(P. 1023/23-25) (smaller than what?)

Done (see lines 285-286). In short, we mean that contrast between basins declines with increasing air-sea equilibration time.

13) Ref. #3 points out that the statement 'there is a decrease of equatorward transport in the Northern Hemisphere between 35°N and 40°N' in (P. 1024/22) is not exact and needs to be clarified.

The statement (P. 1024/22) has been modified to '...there is a decrease of equatorward transport in the Northern Hemisphere between 0°N and 45°N, which is largest at 35°N' (lines 314-315). Additionally, we deleted the statement 'at the position of the central mid-latitude jet, lying at the boundary between the subtropical and subpolar gyre and characterized by high eddy activity' because the case does not now seem strong enough to link this reduction in transport to the mid-latitude jet eddy activity.

14) Ref. #3 states that the motivation for the hypothesis in (P. 1025/3-4) is not clear and that the rest of the paragraph is not clear.

To make it clearer, the text formerly between P. 1024/28 and P. 1025/11 has been completely rewritten (see lines 320-323). In the revised manuscript, we now state that differences between the three tracers can result from their contrasting vertical distributions and different horizontal gradients in the upper ocean.

15) Ref. #3 asks why we compare the inventories in (P. 1025/18/Figure 8) instead of normalized inventories as in Figure 3a

In Fig 3a, inventories are normalized in order to compare tracers with one another. That comparison concerns only the globally integrated quantities and only the non-eddying simulation. In Figure 8, we compare the two model versions and the data in each of the three ocean basins. For the latter figure, normalization is an unnecessary additional

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step, which can also create problems. Normalisation allows us to compare patterns but it hides the magnitude of actual differences between models and data.

16) Ref. #3 mentions that it is hard to see anything from the three subplots in Figure 9. He also suggests plotting the differences as model-minus-data

We have now changed the contour levels and the colour scale to better visualize differences, without resorting to difference maps, which we think can be confusing to non-specialists.

17) p. 1026/1027 Ref. #3 points out that some readers might conclude, mistakenly, that it is pointless to refine the resolution in ocean model studies focusing on anthropogenic CO2 and bomb C14 because both the eddying and non-eddying models fall within the uncertainty of the global inventory. This referee suggests we need a more careful discussion of this issue, particularly emphasizing the striking local differences between models.

In response to this insight about potential reader misperceptions, we have rewritten part of the Results section that describes the CFC-11 validation, and we have rewritten the subsection 3.4.3, which describes the anthropogenic CO2 and bomb C-14 validations (lines 379-402). We now emphasize the striking differences in the southern extratropics and the eddying model's much better local agreement there with the data reference for both CFC-11 and anthropogenic CO2. We also clarify that CFC-11 is the tracer of choice because it is based on direct measurements whereas anthropogenic DIC is estimated indirectly from other data and thus involves assumptions and potentially large systematic errors. (lines 395-397).

18) [p. 1030/6-10] Ref. #3 requests to clarify the paragraph about the AAIW ventilation (P 1030/6-10)

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Done. See the response to the comment #10 of the Ref. #2.

19) [p. 1031] Ref. #3 states that a discussion of tracer distributions in the deeper ocean is missing

See the response to the comment #4 of the Ref. #2

20) [p. 1033/26]: Not only 'heat' but 'heat and CO2'?

We mention only heat in this context because we are speaking about climate models, which do not have a carbon component.

3 Figures

Figures generally: many of the smaller figures have axis labels that are hard to read.

Axis labels on the smaller figures have been made larger.

Figures generally: Units on some axes are missing and in some cases are not formatted consistently

Correct units are included on all axes and are given in a consistent manner.

Figure 1: The EKE map for the non-eddy resolving model is somehow redundant

The revised manuscript omits the EKE map for the non-eddying model.

Figure 9: Once again: hard to see from the figures

The size of this figure has been increased. We have also changed the contour levels and the colour scale to better visualize differences between the models and the observations.

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Figure 10: No axis label on y-axis

Corrected.

Figure 16: The maximum mixed layer depth in the figure is plotted in sigma/latitude space, but in the figure caption it is defined as [m]

This error in the figure caption has been corrected.

4 Technical Comments

p. 1014/4: One 'space' before 'Vallis (2000)' too much?

This extra space has been removed.

p. 1014/24: In this line it should be 'Broecker and Peng (1974)' instead of '(Broecker and Peng 1974)'

Done

p. 1014/8: '6 month' instead of '6-month'?

Done

p. 1015/9: 'corresponding' instead of 'correspondng'

Done

p. 1017/6-7: Inserting the paragraph here is misleading. Just continue the line (similar as done on p. 1016/lines 14-26).

Done

p. 1023/5: If referring to Table 1 type '34%' in the text or '>33%' in Table 1.

We now refer to 34% in both in the text and Table1.

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p. 1025/15-16: Misleading here, since there actually are observations also for CO2 and 14C.

We have deleted this offending sentence here and we now explain more carefully that CFC-11 is more valuable as a tracer to validate ocean models because it can be measured directly and does not have to be estimated from other measurements, as do anthropogenic CO2 and bomb C-14 (in order to separate the anthropogenic component from large natural background component).

p. 1027/20-22: To avoid misunderstanding write: 'both mixed layer depths are deeper than in the observations, but deepest for the non-eddying model.'

We have clarified this ambiguity in the revised manuscript.

p. 1027/24: Delete the first of the double 'of wintertime convection'

Done.

p. 1028/23: What and where is the SAF (possibly the SAF was already mentioned above). Three lines below it is the same for the APFZ.

The SAF is Subantarctic Front (near 50°S). It is now defined when it is first mentioned in the (Results section 3.4.2, line 370). The SAF represents the northernmost current core of the ACC. The APFZ is the Antarctic Polar Front Zone located near the Antarctic Divergence at the southern flank of ACC; it is now defined more exactly in the revised manuscript.

p. 1030/3: What means 'termed the bowl'? If bowl is an expression then it should be marked adverted commas 'bowl'?

In the revised manuscript, we have rephrased the sentence. The 'bowl' is defined as that region which separates the subsurface ocean from the interior ocean (lines 476-478). We also provide a reference (Marshall and Nurser, 1992) for those interested in

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more details.

p. 1033/11: 'insensitive' better to be replaced by 'not affected'?

We now say that 'bomb C-14 air-sea fluxes are much less sensitive' (line 580).

Figure 4: Not clear from the figure caption if ORCA2 or ORCA05

The revised caption now makes it clear that these results are those simulated by the non-eddying model

Figure 6: Not clear from the figure caption if ORCA2 or ORCA05

The caption now includes the phrase 'simulated by the non-eddying model'

Figure 14: To help the reader, write something like: 'tracer inventories [mol] divided by its surface concentration [mol/m3]'

Following this advice, this part of the caption now reads 'The penetration depth of a tracer is defined as the tracer inventory [mol m-2] divided by its surface concentration [mol m^{-3}].'

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