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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.

Anonymous Referee #3

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Oceanic uptake, storage and transport of (increasing atmospheric) CO2 is an important issue in understanding and predicting possible climate change scenarios from climate models. The paper of Lachkar, Orr, Dutay, and Delecluse deals with the impact of horizontal ocean model resolution to this issue and demonstrates the need of improved resolution to give reliable estimates of oceanic CO2 uptake, storage, and transport, which can not be reached by better model parameterization only.

General Comments:

By comparing a non-eddy resolving (2°) with a barely eddy resolving (0.5°) model of the same model type (OPA9) incorporating several tracers (CFC-11, CO2, 14C) with varying surface equilibration time scales and further comparison with observations the

authors find substantial differences, particularly for tracers with short equilibrating time scales, and, actually not surprisingly, a much better representation of the observed tracer distributions in the higher resolution model. However, they discus the causes and mechanisms which drive the differing uptake and storage of the tracers, and conclude that higher resolution models are needed, since the substantial discrepancies can not be avoided by improved model parameterization. Only sufficient representation of eddies, which are responsible for an enhanced (more realistic) stratification of the upper water layers and a weaker meridional transport (and also a weakened interbasin exchange), as obtained with enhanced horizontal resolution, allow an improved and more realistic tracer distribution and, thus, more realistic CO2 uptake and transport and its prediction for climate research issues. The authors also show that this aim is not achievable with a more sophisticated model parameterization (i.e. Gent-McWilliams).

The focus of the paper is on the transient tracer uptake in the southern hemisphere. The authors find large differences between the coarse and the fine model type in uptake and meridional transport of CFC-11 and CO2 (with relative short equilibration time scales) and almost no differences in the 14C distribution (with relative long equilibration time scale). Furthermore they find good agreement of the globally integrated high resolution model tracer distribution and observations.

They describe the model set up sufficiently and validate convincingly their simplifications and approaches to reduce computation expense (offline model run and perturbation approach for tracer distributions) and tracer boundary conditions. Next they compare the low and high resolution model results of uptake and storage of the different tracer types, finding largest discrepancies for the short equilibrium time tracers (CFC-11 -22% and CO2 -18% global in fine model) and small discrepancies for the long equilibrium time tracer (14C -5%). Just subsequently they compare the model results with observations, which, in my opinion, should have been the first attempt before comparing the both models!

However, in the following discussion they analyze the mechanisms how and why the

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fine resolution model better fits to the observations and how and why eddies resolved in the fine model provide a better representation of the observed tracer distributions: Eddies resolved in the model give shallower mixed layer depths, strengthen the stratification and weaken the meridional transport. The authors discuss this with focus on southern upper water masses (Antarctic Intermediate Water). What I miss in this regard, is an - at least short - discussion of the tracer uptake in lower water masses like the North Atlantic Deep Water and Antarctic Bottom Water. These water masses provide a large pool to store CO2 and other substances, particularly on global and longer time scales. Even if the vertical resolution of both model types is low at higher depth (250 m at the bottom) the tracer storage in these deeper water masses is worth (and more generally: necessary) to be discussed.

This paper should be of interest for climate and ocean modellers, tracer oceanographers, and physical oceanographers. I like to recommend the publication of this intensive work in Ocean Science very much. Here and there some corrections and further remarks, additions, explanations, and corrections are needed and might improve the quality of the paper without (as I believe) consuming too much effort or further model computations. Even if I listed many points below, most of them are just minor comments or even suggestions only, and their number should not discourage the authors to bring their publication into a satisfying shape.

Specific comments: requests, open questions, suggestions

p. 1012/8-10: “increased eddy activity reduces” is somehow misleading. The eddies are always present, since they are real, but they are resolved in the high resolution model only. It took me a while to figure out that the mentioned “reduction” is between the two model types and not between model and observations (which are also real). These kind of misleading statements (model A, model B, but reality (i.e. the observations like real eddies or observed tracers) is not a just another C but reality!) occur some times within the text.

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p.1013/7-9: It is not clear on the first reading what is meant: disagreement between different models or disagreement between model(s) and observations.

p. 1015/11-12: There are many other publications outside model community papers dealing with this particular issue. More from the direct tracer approach I may mention Beining and Roether (GJR 1996, Vol. 101, No. C7, pp. 16455-16464), Sonnerup (GRL 2001, Vol. 28, No. 9, pp. 1739-1742), Waugh, Hall, and Haine (JGR 2003, doi: 10.1029/2002JC001325), and Waugh, Haine, and Hall (DSR 2004, doi: 10.1016/j.dsr.2004.06.011). At least the latter two papers would be worth to be noticed by the authors of this present study. They also deal with ocean tracer uptake, but from a different approach. At least they encourage future investigations in computing and predicting oceanic CO2 uptake and storage in any type of approach.

p. 1016/3-7: It might by worth to give an estimate or statement - here or later in the summary - to what extend “full resolution” might change the findings presented here. Is it negligible or small or is it possible to quantify it?

p. 1017/9-11: What about a difference between online and offline performance for the eddying model?!?!

p. 1018/6-9. I understand that for a direct comparison it is necessary to keep both model types as identical as possible, except for the resolution of cause. The authors motivate, convincingly for me, why they do not apply enhanced model parameterization (i.e. Gent-McWilliams) to their model, even not to the coarse model. Later they actually show that GM does not improve or even change the modelled tracer distribution significantly in both model resolutions. But here they introduce the usage of two different formulations of horizontal viscosity. In the eddying model they use biharmonic formulation due to shorter scale lengths. But why do they use a different formulation (Laplacian) for the non-eddy model??? Is it necessary? Does it change model results additionally to enhancing resolution? This needs at least a comment!

p. 1019/18: “concentrations of CFC-11 are measured with high precision.” Hopefully

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yes! But if such a statement: What precision and what about the other two tracers?

p. 1021/7: “280 ppm” versus p. 1020/17: “278 ppm” made me think. Nobody worries about the 2 ppm but is that from rounding or did Siegenthaler and Joos (1992) used a different number or is this important? If not write both times “280 ppm” to avoid that reader starts to think about this.

p. 1022-1025: This section compares the two different model results. If local features are discussed (air-sea fluxes, inventories) they are discussed on results from the coarse resolution model. E.g. Figure 3, Figure 4, Figure 6. Why they are not discussed on the - more realistic - eddy resolving model results?

p. 1022/17-18: It is not the differences in air-sea equilibration times and solubilities which produces these dissimilarities, since they are the same everywhere. But together with locally different stratification and surface layer residence time they lead do different uptake values (as the authors show themselves below!). The statement as it is written might be misleading or could be interpreted wrong. Furthermore, there is an “in” too much in the sentence?

p. 1023/6-7: Table 1 shows to me that 14C is uniform, but not CO2!

p. 1023/23-25: “Ĕ is smaller.” Smaller than what? Smaller than CFC-11 or smaller than in the other basins?

p. 1024/22: In Figure 7 I see a decrease between 0°N and 45°N, largest at 35°N. What is meant?

p. 1025/3-4: What motivates this hypothesis??? An explanation is necessary! The subsequent part of this paragraph is not understandable to me at all. Furthermore, in line 7 there is a “transport” too much.

p. 1025/18/Figure 8: Is there any reason to compare now inventories instead of normalized inventories as in Figure 3a (or the other way round)?

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p. 1025/21-24/Figure 9: It is hard to see anything from the three subplots. Might it be better to present: a) mixed layer depth observations, b) mixed layer depth observations minus course model, and c) mixed layer depth observations minus fine model?

p. 1026/1027: What I get from subsection 3.4 and Table 3 is that the low resolution model results into too high CFC-11 inventories (globally!) and the high resolution model results into to low (but closer to the observed) inventories, while for CO2 and 14C no significant difference is apparent (regarding to the uncertainties of the observations). This might (!) lead to the statement that for the issue of CO2 uptake and storage in climate modelling it is pointless to refine the resolution, since globally (by coincident) the total CO2 storage is in the right order. But I am sure, that the authors don't want the reader to come to that - off course - misleading conclusion! This has to be discussed carefully. Firstly the "very good agreement between high resolution model and observation" is weaker than it might seem, since with accounting for an enhanced error (due to tracer boundary conditions, etc.) even the course model CFC-results could be brought to agreement. And CO2 and 14C agree anyhow. Secondly, but much more important is, that the agreement of CO2 and 14C in the high resolution model is just valid for the global inventory. This is more or less a coincidence. Locally the disagreement between coarse model and observations is striking, possibly even more since the deeper layers of the ocean are not compared in detail as it is done for Antarctic Intermediate Water further below.

p. 1030/6-10: Antarctic Intermediate Water is ventilated from below? Ventilation should be done by subducting surface water, exposed to the atmosphere. Even upwelling of Circumpolar Deep Water or North Atlantic Deep Water - far off their sources - should not lead to a sufficient transient tracer addition (at least not for CFC-11). This has to be clarified!

p. 1031: Once again - since it belongs somewhere here and is part of my general comments and requests: What I miss is a general discussion of tracer distributions in the deeper ocean. Even if it might be hard to compare the model results with obser-

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vations, since the vertical resolution at larger depths is coarse (250 m at the bottom) a comparison of the two different horizontal resolution models should be easy, at least in a qualitative way.

p. 1033/26: Not only “heat” but “heat and CO2”?!

Figures generally: Many of the smaller figures have axis labels which are very hard to read. This might be due to the conversion of the original manuscript into the pdf-version. However, authors and the responsible persons from Ocean Science should keep an eye on this!

Figures generally: Units on some of the axis are missing completely or are formated not consequently. For example the authors sometimes write CFC [10ˆ6 mole] and sometimes [Mmole]. It would be good to format this unified.

Figure 1: The EKE map for the non-eddy resolving model is somehow redundant. Could a change of the colour scale improve this? Or, if the EKE in the non-eddy model is close to zero everywhere, omit this subplot and mention it in words.

Figure 9: Once again: hard to see from the figures Ĕ Might it not be better to present: a) mixed layer depth observations, b) mixed layer depth observations minus course model, and c) mixed layer depth observations minus fine model?

Figure 10: No axis label on y-axis.

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]. Actually, this is not important but somehow confusing.

Technical comments: typing errors, corrections, suggestions

p. 1014/4: One “space” before “Vallies (2000)” too much?

p. 1014/24: All references should be given in the same format (if this is appropriate). In this line it should be “Broecker and Peng (1974)” instead of “(Broecker and Peng,

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1974)"?

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

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

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

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

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

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

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

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.

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

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

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

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

Figure 14: It was hard to me to figure out how the penetration depth actually was computed. Of cause it is easy of one uses the right units. To help the reader, write something like: "tracer inventories [mol] divided by its surface concentration [mol/m3]".

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