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Biogeosciences Discussions

Interactive comment on "Seasonal and mesoscale variability of oceanic transport of anthropogenic CO_2" *by* Z. Lachkar et al.

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This manuscript describes model results designed to study the temporal variability of oceanic transport of heat and anthropogenic CO2.

1) In general, it is difficult to understand the importance of this study. The overall objective is not clearly defined. Why one would like to know the variability of transport of oceanic anthropogenic CO2? Today we know there are large uncertainties in the quantification of the penetration of anthropogenic carbon in the ocean and we also know there are even larger uncertainties in the quantification of water-mass transport. There is still a significant potential for progress in ocean modelling.

The variability of the meridional transport of anthropogenic CO_2 is worth inves-

tigating because it represents one of the major uncertainties in the traditional data-based estimates of the annual mean meridional transport of CO_2 (Holfort et al, 1998; Wallace, 2001; Alvarez et al, 2003). These methods generally rely on one-time hydrographic measurements and can be biased toward summer months during which most observations are collected. The objective of this study is to evaluate this potential bias and also to assess the part of uncertainty associated with the mesoscale variability. The study has very practical and concrete implications because it reveals, for instance, regions and latitudes where there is little bias due to temporal variability is minimum, and thus are suited for hydrographic surveys. Additionally, this work provides some hints regarding the minimum sampling resolution needed to minimize the eddy-induced noise in the data-based estimates of CO_2 transport. For more clarity, the objective of the study is now better defined in the abstract (lines 2-4) as well as in the introduction (Pg 4, lines 7-8) following the reviewer suggestion.

2) There are a few details I could not understand in the strategy used to perform this study. For instance why a series of finer GCM runs were not already performed to confirm (or provide some uncertainties on) the results of the intermediate-resolution model?

Our model has an eddy-permitting resolution (34 km on average). Given the currently available computing resources, this is the current state of the art given that it is the highest resolution that any ocean carbon modeling group has been able to afford for a sufficiently long global simulation (235 years to properly simulate anthropogenic CO_2). To our knowledge, no other global ocean CO_2 simulation has ever been made at equal or higher resolution.

3) In order to model the penetration of anthropogenic carbon across the air-sea interface it is necessary to introduce initial conditions such as the total CO2 and the total alkalinity fields; what are they and what are the uncertainties associated with these fields?

In this study we used the perturbation approach to model anthropogenic CO_2 (Siegenthaler and Joos, 1992; Sarmiento et al, 1992, Orr et al., 2001). This perturbation approach treats anthropogenic CO_2 as a passive tracer, the concentration of which, by definition, is initialized to zero at the beginning of the industrial era (1765). The model is then run until year 2000. The rational behind using this approach is to reduce the computational resources needed to make global carbon simulations at eddy-permitting resolution to near-steady state conditions. Such simulations are typically several thousand years long, and hence, would require computing resources that are far beyond what is presently available. To remain concise (see Referee's comments 7 and 12) we did not add more details about the perturbation approach in the revised paper. However, in the manuscript, we invite readers who wish to know more about this approach and the details of the the boundary conditions to refer to Lachkar et al (2007).

4) What are the uncertainties associated with the computation of the air-sea fluxes?

It is well established that simulated anthropogenic CO_2 is not very sensitive to the gas exchange formulation. Sarmiento et al. (1992) show that when one doubles the gas exchange, there is only a 10% increase in anthropogenic CO_2 uptake. This weak sensitivity occurs because it is not gas exchange that is the major limitation of the penetration of anthropogenic CO_2 below the mixed layer where most of it ends up. Rather, this anthropogenic CO_2 penetrations is limited mainly by exchange between the surface and deep ocean.

5) How results from a time-varying (non-steady state) model can be compared with GLODAP data ?

We compared the annual mean of year 1994 from the model simulation to the GLODAP data because this data-based compilation is centered around 1994 and is best representative of this year (Key et al 2004). Our aim was to evaluate the model in terms of the simulated vertical penetration and inventory of anthropogenic CO_2 for the modern era and at the global scale. Both of these diagnostics are cumulative in time, i.e., they accumulate from one year to the next, so that at present seasonal variations are quite small by comparison. Our previous model sensitivity tests confirm that neither the inventory nor the penetration depth vary significantly with season.

6) Anthropogenic CO2 concentrations cannot be measured. There are always determined from a model. Many recent comparative studies have demonstrated that the anthropogenic CO2 estimates included in the GLODAP data base are probably not the best. Therefore, for comparison, it would be best to use anthropogenic CO2 results from other models too.

We agree with Referee #2 in that anthropogenic CO_2 is not a measured quantity but is derived from observations using different approaches and assumptions. We are also aware that the uncertainties associated with these data-based estimates may be on the order of 15% in the case of GLODAP (Key et al., 2004). However, GLODAP is the only available global data-based product for anthropogenic CO_2. Thus it is worthwhile to compare our simulated anthropogenic CO2 to the the GLODAP data-based estimates, although by themselves such a model evaluation effort would be inadequate.

This comparison should be viewed as a complement to our previous evaluations of our model with observed CFC-11 (Lachkar et al., 2007; Lachkar et al., 2009), a transient tracer similar to anthropogenic CO_2 but for which the comparison was made with direct, precise measurements along several WOCE sections. We have been clear about this point in our manuscript and we have now added new supporting references : (see last paragraph of section 3.1)

"Despite adequate data-model agreement, potential systematic errors in the GLODAP data-based estimates for anthropogenic DIC (Matsumoto and Gruber, 2005; Waugh et al., 2006; Alvarez et al., 2009; Vazquez-Rodriguez et al., 2009)

compromise our ability to use that tracer by itself as a reference to validate models (Orr et al., 2001). Anthropogenic DIC is clearly not of the same value as CFC-11, which is measured directly. Previously though, we have evaluated the same model with CFC-11 in Lachkar et al. (2007) and Lachkar et al. (2009)."

7) The sections results-discussion-conclusion are too long,

The text has been significantly shortened in the revised manuscript and the number of figures reduced. The total number of pages has been reduced from 44 to only 37 pages. More specifically, the discussion about the annual mean transport in the Results section has been reduced. In addition, we substantially shortened the summary-conclusion section by focusing only on the most important results. Finally, we reduced the number of figures by removing figure 8 and moving figures 2, 3a, and 6 to the supplementary materials.

8) some of the figures are of poor quality and/or inaccurate (it is misleading to label "data" results from a model).

Most figures have been remade with improved labels. Actually, the "data" label refers to GLODAP data-based product while the "model" label refers to the GCM model simulation. For more clarity and in order to avoid any confusion, we replaced in all figures and captions "data" by "data-based".

9) There are too many figures.

The number of figures has been substantially reduced (see the response to comment 7).

10) If the aim of this study is the "seasonal and mesoscale variability" as indicated in the title, why describe and show figures of annual means, of global-, basin-, and zonally- integrated transports?

Referee #2 is correct to point out that these details may dilute the main message of this paper, which is focused on the temporal variability of the transport.

Therefore, we have reduced the discussion of the annual mean transport (sections 3.2.1 and 3.2.2), keeping only a minimum to allow us to put the temporal variability in context. Likewise, we have deleted figure 8 and moved figure 3.a to the supplementary materials.

11) It would be expected to see true seasonal cycles (not like in Fig.3 where the "seasonal cycle" is just a difference of July minus January).

The main goal of this study is to quantify the potential bias in data-based estimates of transport which may result from temporal variability. Therefore, we are essentially interested in the amplitude of the seasonal cycle which is well represented by the difference between July and January. This is exactly the same metric that has been used in several previous studies dealing with the seasonal variability of the heat transport (Lee and Marotzke, 1998; Jayne and Marotzke, 2001, Böning et al, 2001). For consistency, we prefer to use the same approach here. In so doing, it also allows us to reduce the length of the manuscript.

12) In summary, although the authors could raise some interesting points, the present manuscript is relatively difficult to read because it is too long and the objective is not clearly defined. I would suggest the authors to be much more concise and to show clearly their results in very few figures and Tables.

Following the reviewer suggestions, we have substantially shortened the text and have reduced the number of figures in the revised manuscript. We also added text to the Abstract and Introduction in order to better emphasize the objective of the study (please also see our responses to comments 1, 7, 9 and 14).

13) It is also essential to provide uncertainties associated to each quantity. This would give an indication on how meaningful the results are.

The model has been evaluated in detail in previous studies, revealing generally good overall agreement with observations but also regions where there are sys-

tematic biases (Lachkar et al 2007 and Lachkar et al 2009). Systematic biases are also revealed by our comparison of a typical coarse-resolution model to a state-of-the-art version of the same model (with higher resolution). Our goal was not to make comparisons with other coarse-resolution models, as such comparisons have already been reported (Orr et al., 2001; Dutay et al., 2002). Rather a major emphasis was to investigate what coarse-resolution models and observational studies might be missing. As this is the first study to use a eddy permitting global ocean model to simulate anthropogenic CO_2, results cannot be compared to other models of the same nature. This must be left for future work.

14) The conclusion should point out the most significant and new scientific progress that stem from this study.

The conclusion has been shortened to focus on the most important results.

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