Comments on

"^{14}C-age tracers in global ocean circulation models"

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submitted to Geoscientific Model Development (GMD)

The approach and methodology in this paper are interesting. The use of specific tracers for tracking reservoir ages is new in modeling studies of radiocarbon. With these new tracers the authors are able to illustrate and quantify the respective roles of circulation, mixing and boundary conditions in setting apparent ages (or radiocarbon ages) of water masses. The most important outcome of this study is that the preformed component of radiocarbon age in seawater may simply be obtained from the difference between the actual ^{14}C-age and the ideal or ventilation age.
This is not too surprising a result since it is known that mixing only weakly affects radiocarbon ages when compared to ventilation ages (e.g., Deleersnijder et al, 2001; Delhez et al., 2003; Khatiwala et al., 2012). Up to now, however, nobody had demonstrated that the preformed or reservoir age in models may easily be obtained. The new tracers suggested by this work will be beneficial in model assessment and inter-comparison studies.

However before accepting it for publication there are several points which should be improved or corrected.

Main issues

1) The general presentation is rather confused. The text should be reworked in order to gain structure and fluency.
I would suggest that the authors systematically make use of the dedicated name (as given in Table 1) when referring to tracers. Some tracers experience change of name or natation within the same paragraph and among sections. It doesn't help the reader.
Another point would be to describe the experimental set-up when presenting the experiments. Section 2.3 should be shortened to the reference model runs and the other material moved to the relevant sections.

2) Line 10, page 7042, line 20 on page 7047 to line 1 on page 7048: Age and residence time are different concepts. The quantity computed here is the time elapsed since water left the deep sea. Conceptually it is an age, not a residence time. The latter is the time for water leaving the surface reservoir (Bolin and Rohde, 1973; Takeoka, 1984).

Further this diagnostic does not seem appropriate to evaluate the capacity of waters to equilibrate with the atmosphere.
Indeed water parcels may remain close to the boundary and meander in and out of the surface layer. Their total exposure to the atmosphere would actually be much larger than indicated by the age with respect to depth. Indeed this age is reset to zero any time the parcel re-enters the surface layer. A diagnostic like exposure time (Monsen et al, 2002; de Brouwere et al., 2011) would be more appropriate.

3) Lines 12-20, page 7043: I expected the apparently opposite behavior of ^{14}C-age and ventilation age among models to be further investigated or discussed later on in the paper.

4) Lines 9-10, page 7045: I have concerns about the duration length of experiments; 2500 yr is rather short with respect to the ^{14}C lifetime. I doubt equilibrium with the new boundary conditions was achieved. OCMIP2 suggested objective criteria for achieving equilibrium: 98% of the ocean volume should have a drift of less than 0.001 permil/year (http://ocmip5.ipsl.jussieu.fr/OCMIP/, OCMIP2 HowTo Docs). Did the various experiments meet that criteria? The models used in the present work are efficient enough so that several thousand year simulations may be achieved within a reasonable time.

5) Section 3.2, pages 7045-7046: The discussion of the impact of mixing on apparent ages should

be shortened since this is not really new material. The bias toward smaller ages when in presence of mixing has been established in many previous works: e.g., Jenkins (1987), Delhez et al. (2003) and references therein. The results should be discussed in light of these previous works.
Figure 5 is clearly not needed.

6) Lines 21-23, page 7046: "*The largest difference is found between age^{ideal} and ^{14}C -age^{decay}. … Over much of the Pacific Ocean it is equivalent to about 15% of ideal age.*"
This difference is much larger than reported in previous studies (5% in Deleersnijder et al., 2001; less than 50 yr in Khatiwala et al. 2012).
Is this a consequence of not-well equilibrated deep water masses? Is this difference model-dependent?

7) Lines 4-29, page 7049 and lines 1-4, page 7050: there are several issues to be cleared in this discussion of the impact of the gas exchange coefficient.
First, the data-based bomb radiocarbon inventory has been shown to be underestimated (Naegler, 2009; Mouchet, 2013). The reasons are inherent to the available measurements. The drastic decrease in the exchange coefficient suggested here seems unjustified in light of these works. To assess the value to be used proper simulations with CFC and bomb-radiocarbon should be performed.
Second, the conditions at the sea surface differ among the 3 models used in this study: UVIC is constrained with different wind fields than MIT2.8 and ECCO; each model has its own sea-ice climatology. I would also compare models with respect to gross air-sea fluxes.
Third, a good correspondence between model and data ^{14}C may be obtained either by adjusting the level of vertical diffusivity or the gas exchange or both. This is an aspect which needs to be more carefully addressed than it is done in the present text. There are many previous works on this topic (e.g., England and Rahmstorf, 1999; Cao and Jain, 2005; Müller et al., 2006; Müller et al., 2008).
Eventually, the sentence "*In fact this is occasionally seen in the literature (e.g. Cao and Jain, 2005; their Fig. 8d).*" is inappropriate.

8) Lines 4-10, page 7050: the result that ice cover does not impact ^{14}C preformed ages seems in contradiction with previous works (e.g., Campin et al, 1999). Does present-day ice cover sufficiently affect areas of the Southern Ocean where large preformed ^{14}C ages are observed? Wouldn't it be more sensible to test this point by extending the northern extent of sea-ice in the Southern Ocean?

9) Section 4: the declared aim of this section is "... *to demonstrate the adverse effects of neglecting the preformed component of ^{14}C-age* ..." (lines 18-19, page 7050). The material is available for such a purpose but the results are not fully exploited and the discussion is a bit confused.
In my opinion Figs 13 and 14 are not needed. Global vertical profiles such as in Fig. 8 for the various Kv (bulk and preformed ^{14}C-ages) would be more illustrative. The impact of both Kv and gas exchange coefficient on these profiles should also be put into perspective.

10) Lines 1-2, page 7039: "*For UVIC the ^{14}C-simulations are made alongside a normal, biotic model run.*" Does biology in UVIC affect ^{14}C? In which ways?

Minor comments

Lines 20-21, page 7035: "*Surface water in equilibrium with the preindustrial atmosphere (1890 AD) would have a Δ^{14}C = 0‰ and a ^{14}C-age of 0 yr.*"
This is incorrect; even if considering a constant atmospheric ^{14}C-production and steady-state ocean and climate the Δ^{14}C of ocean surface water and their age would not be 0. This is in contradiction with what is stated in the paper on page 7036, lines 7-19.

Line 12, page 7035: suppress "*to reach rather constant atmospheric levels*"; atmospheric ^{14}C is not constant neither on the anthropogenic (Suess, bomb...) nor on millenial time scales. This affirmation is confusing and in contradiction with what is stated on page 7036, lines 24-26.

Line 16, page 7035: suppress "*(for equations see Sect. 2)*"; not needed here

Line 19, page 7035: why the reference to 1890 AD here?

Lines 5-6, page 7036: "*Thirdly, it is usually assumed that the transport of $^{14}C/C$ from the surface to the deep sea via sinking organic particles can be neglected (Fiadeiro, 1982).*"
The authors state that the neglect of the ^{14}C transport to depth via POC is a problem. They nevertheless make the same assumption in their model (page 7038, line 28) without discussing this point.

Line 23, page 7036: is the use of the word "corrected" appropriate in this context?

Lines 20-21, page 7036: "*In the context of ocean biogeochemistry the time elapsed since the last contact of a water parcel with the atmosphere, i.e. water of age zero, is of particular interest.*"
Reference to previous works on the ventilation age tracer (e.g., Thiele and Sarmiento, 1990; England, 1995) is missing.

Lines 21-25, page 7036: "*For example, the estimation of rates of ocean respiration or $CaCO_3$-dissolution from cumulative tracer changes requires corrected reliable age determinations (Jenkins, 1982; Sarmiento et al., 1990; Feely et al., 2002). ^{14}C-ages of several hundred years for waters actually in contact with the atmosphere can thus pose a severe problem*."
The authors of the quoted studies did not rely on ^{14}C for their estimate of oxygen utilization or $CaCO_3$ dissolution rates. Hence this paragraph should be reformulated.

Lines 20-27, page 7036: this paragraph should be reworked. I do agree with the idea behind it, but the topic is presented in a rather confused way.

Line 26, page 7038: "DIC and ^{14}C-DIC are prognostic model tracers of total dissolved CO_2 and $^{14}CO_2$ respectively." In order not to confuse between dissolved CO_2 (gazeous) and total carbon I would recommend to replace CO_2 by carbon or C in the above sentence.

Line 12, page 7039: *dissolved* is misspelled

Formula (3) page 7039: the mean life of 8033 yr does not agree with the half-life of 5730 yr given on line 14, page 7035. What is the decay rate of ^{14}C in the model experiments?

Lines 20-21, page 7040: "*... and adds up any ^{14}C-decay of the ^{14}C-DIC tracer in the interior*"
This sentence is hardly intelligible. Do you mean $^{14}C\text{-}DIC^{decay}$ *undergoes radioactive decay as does* $^{14}C\text{-}DIC^{bulk}$?

Section 2.3: Only the reference runs and short description of the other experiments should be given here. Too many details are given here for the gas exchange, vertical diffusivity, and "residence" time; this should be moved to the relevant sections.

Lines 4-7, page 7045: there is some redundancy in these lines.

Line 12, page 7048: "... *to moderately negative surface $\Delta^{14}C$ and ^{14}C-ages*..." do you really mean negative ages?

Table 1, page 7060:
Under its present form this table is of no real utility. I would suggest to transform it as illustrated below, it would greatly help the reader to follow the text.

Tracer name	**Age name**	**Source/sink**	**Sea-surface B.C.**	**Component**	**Comments**
$^{14}C\text{-}DIC^{decay}$	$^{14}C\text{-}age^{decay}$	radioactive decay	0	Circulation	^{14}C-ages: subject to nonlinear mixing effect.
$^{14}C\text{-}DIC^{pre}$	$^{14}C\text{-}age^{pre}$	-	$^{14}C\text{-}DIC^{bulk}$	Preformed	
$^{14}C\text{-}DIC^{bulk}$	$^{14}C\text{-}age^{bulk}$	radioactive decay	Eq(1b)	Total	
age^{ideal}		aging	0	Circulation	Ages: not subject to nonlinear mixing effect.
age^{pre}		-	$^{14}C\text{-}age^{bulk}$	Preformed	
age^{bulk}		aging	$^{14}C\text{-}age^{bulk}$	Total	

The affirmation "^{14}C-*based tracers: are subject to non-linear tracer mixing effect*" is not correct. The ^{14}C-ages are subject to nonlinear mixing effects.

Figure 5: not needed

Caption of Figure 12: *patterns* is misspelled

Figures 13 & 14: suppress and replace with one similar to Fig. 8 but for the different Kv.

Line 5, page 7056: *Fiadeiro* is misspelled

References

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