REVIEW OF "Oceanic tracer and proxy time scales revisited" by C. Siberlin and C. Wunsch, submitted to *Climates of the Past, Discussion*

The authors discuss how passive tracers suddenly introduced at the sea surface approach a steady state distribution in the global ocean. Numerical solutions of a tracer transport equation where the velocity field is derived from an ocean general circulation model are presented to illustrate some of the main ideas. Different boundary conditions for the tracers at the sea surface, i.e., Dirichlet conditions and Neumann conditions, and different regions of tracer injection are considered. Tracers are taken as conservative or affected by first-order decay such as radioactive decay. A variety of conclusions are drawn, including implications for the interpretation of tracer records from deep-sea sediment cores.

The manuscript (ms. hereafter) contains a series of interesting results, such as (1) the times to equilibrium exceed a thousand years for any one region of the global ocean outside of the injection and convective regions, (2) the equilibrium times can be much longer than radiocarbon ages, and (3) pulse-like inputs can produce very different transient approaches to equilibrium in different parts of the global ocean, leading to event identification problems. These results are certainly relevant – in particular to the field of paleoceanography – and the ms. is generally very well written. Nonetheless, I think that a few comments would need to be addressed before this ms. be considered acceptable for publication. Two major comments are reproduced below, followed by a list of minor ones. I hope these will help the authors revise their interesting work.

MAJOR COMMENTS

1) A weakness of the ms. is perhaps the apparent lack of a specific hypothesis or question that is being addressed, in particular in the context of recent work on the subject. The Introduction section of the ms. contains a useful summary of two recent studies by Wunsch & Heimbach (2008) (WH08) and Primeau & Deleersnijder (2009) (PD09). It is probably worth summarizing both studies here as well. WH08 used tracer simulations with an ocean general circulation model (OGCM) to show that, following injection at the sea surface, differences in tracer concentration can persist for periods longer than 2000 yr in deep water between the Atlantic Ocean and the Pacific Ocean. WH08 used this result to argue that the apparent lag of 3.9 kyr in the deglacial $\delta^{18}O$ anomaly between sediment records from the Iberian Margin and the Eastern Equatorial Pacific could be due to the finite rates of ocean circulation, i.e., the lag would not require changes in local hydrography as postulated by Skinner & Shackleton (2005). PD09, however, questioned the nature of the surface boundary condition employed by WH08 in their tracer simulations. Whereas WH08 used a Dirichlet condition (in which surface concentration is fixed), PD09 argued that a Neumann condition (in which surface flux is fixed) is more appropriate for $\delta^{18}O$. PD09 showed from an eigenvalue analysis of the tracer transport problem in a 3-box model and in an OGCM that the approach to tracer equilibrium is much faster with a Neumann condition than with a Dirichlet condition.

I think that the authors of the present ms. should be more explicit about the specific question(s) which are addressed in the ms. and about the nature of the new results which are obtained, in particular in the context of the recent studies by WH08 and PD09. Such elaboration could be reported in the Abstract, Introduction, and/or Final Remarks.

2) A second and perhaps more important comment relates to the nature of the surface boundary condition for radiocarbon which is being assumed in the ms. The radiocarbon simulation reported in the ms. (Figs. 9-10) is based on a Dirichlet condition at the sea surface, i.e., the surface concentration of radiocarbon is maintained to a constant value over a region in the North Atlantic and the surface flux of radiocarbon is fixed to zero elsewhere. The use of a Dirichlet condition to simulate radiocarbon in the ocean is unorthodox, as radiocarbon simulations typically assume a flux condition at the surface, in which the flux is proportional to the difference between the concentration simulated by the model and the concentration expected from dissolution equilibrium with the atmosphere (for a pioneering work see Toggweiler et al. 1989). As argued by PD09, a Dirichlet condition would be appropriate for tracers which enter the ocean through air-sea gas exchange, provided that the exchange rate is sufficiently rapid and that the Dirichlet condition is applied to the entire ocean surface. Although radiocarbon does enter the ocean in gas form (as $^{14}CO_2$), the time scale associated with the air-sea gas exchange of carbon isotopes is estimated to be of the order of 10 years (Lynch-Stieglitz et al., 1995; and references therein), which is much longer that the time scales of some of the dynamical processes transporting radiocarbon in the ocean. As a result, the radiocarbon concentration in ocean surface waters (as expressed by $\Delta^{14}C$) is less than in the atmosphere – the natural concentration difference corresponding to an age difference of about 400 years except at high latitudes in the Southern Ocean and in the Pacific Ocean (Bard 1988). The issue of the most appropriate surface boundary condition for decaying tracers is touched on in the ms. (p. 1606, 1st full paragraph). However, I think that the authors of the ms. should provide arguments that a Dirichlet condition does represent an appropriate surface boundary condition for radiocarbon or consider a more conventional representation of this condition in their simulation (or not label the simulated tracer "radiocarbon").

MINOR COMMENTS

Page 1593: write "... $\lambda = 1/(8267$ yr$)$ is the radiocarbon decay constant ..." (5730 yr is the value of the radiocarbon half life)

Page 1593, 6th line from below: write "... because $\exp(-\lambda t_2) < \exp(-\lambda t_1)$ if $t_2 > t_1$ and $\exp(-\lambda t_1) < \exp(-\lambda t_2)$ if $t_1 > t_2$."

Page 1593, 3rd line from below: write "If the second arrival took much longer than the radiocarbon decay time, so that ..., then $\tau_{RC} = t_1 + \ln 2/\lambda = t_1 + t_{1/2}$, where $t_{1/2} = 5730$ yr is the radiocarbon half life." Indeed, from the defining relation of C_{OBS}, C_{OBS} would then be approximated by $(C_0/2)\exp(-\lambda t_1)$. Am I missing something?

Page 1594: Please provide details about the derivation of the equation. Note that this equation is not correct dimensionally.

Page 1594, sentence "The main message is that, assuming that initial surface values of C are not greatly different ...". It should be mentioned here that the assumption may be particularly questionable for radiocarbon to the extent that its natural concentration in surface waters appears very different between the northern North Atlantic and the Southern Ocean (see, e.g., Bard [1988], which is quoted in the ms.).

Page 1596, sentence "In other experiments, a sub-area ...". Part of the sentence seems to be missing.

Page 1597, equation (3): The function A(**r**) should be defined.

Page 1598: write "... and thus the spatial differences of C are clearly bounded ..."

Page 1599: write "... the diffusive part. **Bq**(*t*) is ..."

Page 1600: It is implicitly assumed that the matrix $\mathbf{I} - \mathbf{A}_\infty$ is invertible. Could $\mathbf{I} - \mathbf{A}_\infty$ be singular, at least in principle? If this is the case, is there a physical interpretation for the situation where singularity occurs.

Page 1600, 2nd line from below: write "... but with the concentration initially set to $C = 0$ outside ..."

Page 1604: write "... For $T_{ext} = 1$ yr, t_{90} is shorter than ..."

Page 1605: "origination" is not in my dictionary.

Page 1605, 4th line from below: write "The smaller the decay constant λ, the longer ..."

Page 1605, last line, to page 1606, 1st line, "Figure 10 shows the calculated radiocarbon age from this same experiment, and which is approximately conventional": do the authors mean that the model results are consistent with observations? Please clarify and, if needed, provide appropriate reference(s) for these observations.

Page 1607, "If we consider sufficiently small changes, temperature is expected to behave as a passive tracer": Please clarify.

Page 1607: "Furthermore, in the modern world, high latitude warming is much larger than in the tropics ...". Please provide reference(s).

Page 1610, 1st full paragraph: the 4th sentence does not seem to make sense. Perhaps it should be "When using ... MITgcm of Marshall et al. (1997) ... WH08 note some evidence ..."

Page 1611, 1st paragraph: one may write "Anomalous density fields can increase or decrease pressure gradients and hence flows, ...".

Page 1611, last paragraph, "... from the usual age-model uncertainties ...": One could be more specific, e.g., "... from the usual chronological uncertainties in deep-sea records ..."

Caption of figures 4, 6, 7, 8, and 9: please specify the latitude range of the "Mid-Pacific".

Figure 9 should be discussed in the main text.

REFERENCES

All references quoted in this review can be found in the ms. except the following:

Lynch-Stieglitz J., Stocker T. F., Broecker W. S., and Fairbanks R. G., The influence of air-sea exchange on the isotopic composition of oceanic carbon: Observations and modeling, Global Biogeochemical Cycles, 9, 653-665, 1995

Toggweiler J. R., Dixon K., and Bryan K., Simulations of radiocarbon in a coarse-resolution work ocean model. 1. Steady state prebomb distributions, Journal of Geophysical Research, 94, 8217-8242, 1989