# Review of ‘Simulating oceanic radiocarbon with the FAMOUS GCM: implications for its use as a proxy for ventilation and carbon uptake’

by Dentith et al., submitted to Biogeosciences (manuscript bg-2019-365).

In this work Dentith et al. describe the implementation of radiocarbon and age tracers in the ocean component of the FAMOUS model. Model performances are evaluated against data for the pre- and post- bomb periods. They then assess the role of biological processes in driving ocean radiocarbon distributions. Eventually, an analysis of the departures between radiocarbon ages and water ages is provided.

The paper is very well written and structured. I also appreciate the throughout model assessment and careful comparison with archives and data.

However I have several concerns with respect to the interpretation of the $\delta^{14}C$ and age distributions. In addition, the way radiocarbon is represented in the model calls for a strong assumption on air-sea $CO_2$ equilibrium state. This is why I do not recommend immediate publication of this paper.

## Main comments

### 1) Modeling of radiocarbon

- The method presented in Section 2.2.2. is not that described in the OCMIP-2 protocol. In OCMIP-2, $^{14}C$ and DIC were both prognostic variables constrained by adequate boundary conditions at the air-sea interface (Orr et al., 1999); $\Delta^{14}C$ was then obtained by computing the ratio of these two quantities.

  The method in the present manuscript (modeling of the $^{14}C/C$ ratio) is that first suggested by Fiadeiro (1982) and popularized by Toggweiler et al. (1989). The only difference is that here the DIC value used for scaling the air-sea flux of the ratio (Eq. 6) is not constant, similar to what is done in Butzin et al. (2017); the impact of such a change is expected to be minimal.

  Though ideal for assessing the ocean ventilation (Broecker et al., 1961; Maier-Reimer, 1993) this method is not fit for addressing bomb radiocarbon at a time of major change in atmospheric $CO_2$ since it implicitly assumes that local air-sea $CO_2$ disequilibrium remains constant with time (Mouchet, 2013); this significantly affects the $^{14}C$ invasion rate into the ocean.

- I seriously wonder why not represent in the model the individual carbon species $^{13}C$ and $^{14}C$ rather than their ratios. It would not call for additional tracers. Proceeding so would also guarantee that all important processes and timescales are considered; this is especially important when addressing the anthropogenic era during which rapid and significant changes occur in all three carbon species.

  An additional advantage would be that all fluxes are more straightforward to implement in the model reducing so the risk of mistakes.

## 2) Interpretation of radiocarbon anomalies

- There is some confusion among $\Delta^{14}C$ and $\delta^{14}C$ in section 2.2.4 (page 9, lines 1 to 5). $\Delta^{14}C$ is the normalized $^{14}C/C$ ratio corrected for isotopic fractionation; that is $\Delta^{14}C$ reflects the $^{14}C/C$ ratio which would be observed if there was no fractionation during any of the processes involved in the building of the material under study.

  In the 'abiotic' framework one hypothesizes that fractionation is negligible – this assumption applies to all fractionation processes; or in short there is no fractionation whatever the process, and $\Delta^{14}C = \delta^{14}C$. Hence the $\Delta^{14}C$ values predicted by the 'abiotic' model may be directly compared to measured $\Delta^{14}C$ values, as has been done by many (Toggweiler et al. 1989; De Vries and Primeau, 2011; Mouchet, 2013; Butzin et al., 2017).

  The 'biotic' $\delta^{14}C$ must be corrected for fractionation as in authors' Eq. (8) to be compared to observed $\Delta^{14}C$. In the end the 'biotic' $\Delta^{14}C$ and the 'abiotic' $\Delta^{14}C$ should be very close (Bacastow and Maier-Reimer, 1990).

  In contrast, $\delta^{14}C$ or the inventory (not the normalized ratio) of $^{14}C$ atoms is lower by about 5% when neglecting fractionation; this aspect is thoroughly discussed in Orr et al. (2017).

- There is further confusion in the definition of 'Abiotic' and 'Biotic' processes (sections 2.2.2 and 2.2.3). Fractionation during air-sea exchange is not a biological process. Impact from the biology is twofold: 1) fractionation during soft tissue production, with a preference for the lighter isotopes, and 2) modification of the air-sea $CO_2$ gradient pattern.

- The dominant control on the differences between $\delta^{14}C$-biotic and $\delta^{14}C$-abiotic values (section 3.4) is fractionation during the air-sea transfer, not the biological pump.

  Indeed, according to the authors' equations (B7) and (B8), air-sea fractionation at equilibrium results in an ocean $^{14}C$ enrichment of 21 ‰ at 0°c to 15.8 ‰ at 25°C (consider the first part of Eq. (B11) while assuming equilibrium). Biological activity would increase by only 4 ‰ the surface values and slightly decrease the ratios at depth. However, globally the difference between $\delta^{14}C$-biotic and $\delta^{14}C$-abiotic is nearly completely due to fractionation during air-sea processes.

- It would be of most interest to evaluate the departures between $\Delta^{14}C$ with and without biology, as well as departures between the amount of bomb radiocarbon in the ocean obtained by each method.

## 3) Water age

I must acknowledge that I am not happy at all with the water age results nor with the interpretation of the differences between radiocarbon ages and water ages. Something seems wrong in the implementation of the water age. The discussion of the factors controlling differences between radiocarbon-based and water ages is not based on any evidence.

- It is striking that modeled water ages are everywhere much larger than the radiocarbon age in the deep ocean (Figs 14 and 16) at the exception of polar areas. Given the non-zero preformed radiocarbon ages (reservoir ages) one would expect the opposite relationship; i.e., that water ages are smaller than radiocarbon ages in the deep (e.g., Campin et al., 1999;

Franke et al., 2008; Khatiwala et al. 2012; Koeve et al. 2015). How exactly is the water age computed in the model? Which processes do control its distribution?

- On page 18, lines 15-16 it is mentioned that "… water ages ... are a simple function of advection.". Should we interpret that water age does not experience mixing or diffusion? This would be erroneous.
- In the discussion of regional distributions (page 18, lines 18-25) how do you assess the relative roles of circulation and solubility in controlling the difference between water age and radiocarbon age? There are no solid arguments allowing to conclude in the domination of the one or the other in any region. This paragraph is not based on any firm evidence.
- Similar wishful thinking occurs on lines 28-32 on the same page. The solubility (surface temperature) has nothing to do with water properties at depth in the Indian Ocean.
- Differences between water age and radiocarbon age have already been addressed in numerous academic works; e.g., Campin et al. (1999), Delhez et al. (2003), Gebbie and Huybers (2012), Koeve et al. (2015). This is a non-exhaustive list that I recommend as a start.

## Miscellaneous

Abstract, line 13 : What do you mean by 'over-deep' NADW?

Page 5, line 9: "*...post-bomb deep ocean $\Delta^{14}C$ (i.e. natural $^{14}C$ distributions)*" I would not qualify the post-bomb deep radiocarbon as 'natural'; while some large areas might still hold the pristine signal many other deep ocean areas with younger age could be contaminated by the bomb signal.

Pages 7 and 8, lines 14-18 and 24-26: these lines are unnecessary since the 'Abiotic $^{14}C/^{12}C$' flux (Eq. 6) does not call for the computation of aqueous $CO_2$.

Page 7, equation 2: is the constant 100 or 1000?

Page 7, lines 17-18: "*In the calculation of aqueous CO2, we use the carbonic acid constants of Roy et al. (1993) as opposed to Millero (1995) because this is consistent with the formulation of CO2 solubility used in other areas of the model.*" What is the rationale for requesting coherency between solubility and carbonate dissociation constants? These are two totally different topics. Shouldn't the dissociation constants be consistent among each others and with the pH scale used in the model?

Page 9, subsection title and line 16 (L term): does the transport only include advection in the model?

Section 3.3: why restrict the transient study to the North Atlantic? There are existing coral records in various other locations too (e.g. Druffel, 2002).

Page 16, lines 31-32: "*We propose that this asymmetry relates to the age of the waters that are being upwelled in each basin.*" The 'we propose' formulation is confusing with respect to the correct explanation that follows.

Page 19, lines 5-6: "*However, Campin et al. (1999) did not account for isotopic fractionation in their study, nor was their $^{14}C$ tracer cycled through the marine biological pump.*" This sentence is

out of topic. Campin et al. (1999) represented the normalized $\Delta^{14}C$ ratio in their model. Fractionation effects are therefore canceled and would not explain any difference in age behavior. Campin et al. (1999) did indeed not consider biological activity, but its impact is rather small on $\Delta^{14}C$ contours (Bacastow and Maier-Reimer, 1990). Ages are computed with the help of $\Delta^{14}C$ not with the $\delta^{14}C$ ratios.

Article by Dentith et al. (submitted), quoted at different places in the text, is nowhere to be found on the editor (gmd) site. It would have been an advantage to have been able to consult it in order of understanding the methodology briefly presented in section 2.2.3.

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