

The present paper by Andreas Schmittner and co-authors serves the useful purpose of elucidating the role of air-sea exchange and biology on the distribution of carbon 13 in the ocean. It goes beyond earlier efforts in this direction by the depth of analysis as facilitated by a host of idealized experiments and by comparisons with the most complete set of recent ocean $\delta^{13}C$ data up to now, a data set compiled by the authors for this purpose. I have a few relatively-minor issues with the paper as detailed below but if these are adequately addressed the paper would make a valuable addition to *Biogeosciences* and to the field in general.

One of the main conclusions of this paper is that "...Biological fractionation dominates the distribution of $\delta^{13}C_{DIC}$ of dissolved inorganic carbon (DIC)". It seems to me that the bulk of evidence in the literature (also as reviewed in the Introduction of the paper) points toward similar importance for both biology and air-sea exchange and that conclusions of the relative importance of one of these over another is basically dependent on the model used in the analysis. In the present paper with the UVic model the piston velocity constant k_0 was chosen to be 0.253 but no reason was given for this particular choice beyond the fact that it is similar (but even less) than a recent radiocarbon-based estimate of 0.27 (Sweeney et al, 2007). The chosen k_0 represents a 25% reduction from a value of 0.337 used in a major previous study with the UVic model (Schmittner et al, 2008). In that study that boasted good model-data agreement in general, slightly lower model radiocarbon levels than those in ocean data led those authors to conclude that, if anything, their air-sea exchange was too weak. Does this mean that the present model does not achieve a good radiocarbon simulation (which would be a step backwards)? In fact the authors have now set the stage for what would be a very useful future application of their model: Calculation of ocean radiocarbon distributions including air-sea exchange and biology (using $\alpha_{14C} = 1-2*(1-\alpha_{13C})$) and comparison of such results (converted to $\Delta^{14}C$ values using their $\delta^{13}C_{DIC}$ results) with ocean $\Delta^{14}C$ data as well as with an abiotic $\Delta^{14}C$ simulation with the same model setup.

The authors' own results in the present paper show that the use of a higher k_0 would of course increase the role of air-sea exchange relative to biology on the distribution of $\delta^{13}C_{DIC}$. Taken together with the (over?)simplicity of the treatment in the present paper of fractionation during photosynthesis, expressed for example in the model-data mismatch of $\delta^{13}C$ in POC of northern mid- and high latitudes shown in Figure 8, this should motivate the authors to be careful in overstating the conclusion of the dominance of biology over air-sea exchange. Rather they should qualify this conclusion with phrases like "in our analysis" or "as found with our UVic model setup".

The temperature-dependent equilibrium fractionation factor from gaseous CO_2 to DIC, α_{DIC-g} , plays a key role in fractionation during both air-sea exchange and photosynthesis. The authors use a formulation found in Zhang et al (1995), $\alpha_{DIC-g} = 1.01051 - 1.05 \times 10^{-4}T$. This formulation is based on direct sea water measurements at a pH of about 8.15. But Zhang et al also found a secondary dependency on pH which they encapsulated into a formulation dependent on temperature and on the carbonate ion fraction of DIC. Since the present model has been touted as a useful tool for paleoclimate studies, the authors should perhaps comment on possible errors in leaving out any such pH dependency, if only to show them to be small.

A number of definitions and formulations in the paper use Redfield ratios but the values used for these ratios are not given nor is any reference given from which such values have been taken.

The horizontally averaged vertical distributions of the authors' Figure 5 prove to be an effective way of summarizing the effects of different processes on distribution of $\delta^{13}C_{DIC}$. Another effective

use of such distributions would be to compare the results for the std and Fel simulations (1980-1999 or 1990s averages) with such a distribution calculated from the new $\delta^{13}C_{DIC}$ data set they showcase. Such a simple new figure could be inserted between Figures 9 and 10 and would improve the basis for their discussion in sections 5.2.2 and 5.2.3.

The paper contains myriads of symbols, begging typographical errors. One such error may be found in the second line below equation 5 where there should be a comma, not a raised period/multiplication sign, before R_{DIC} . I encourage all the authors to make still another proofreading effort.

References:

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