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Interactive comment on "Biology and air–sea gas exchange controls on the distribution of carbon isotope ratios (δ^{13}C) in the ocean" *by* A. Schmittner et al.

Anonymous Referee #3

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Review of Schmittner et al, Biology and air-sea gas exchange controls on the distribution of carbon isotope ratios (delta13C) in the ocean.

This manuscript covers an important topic relevant to the readers of Biogeosciences: Analysing and quantifying the specific processes that lead to the observed distribution of ocean d13C in modern and preindustrial times. The manuscript uses a set of idealised numerical model experiments to decompose the total d13C distribution into components from each process. A numerical model is integrated many times using different idealised set-ups, where each model set-up includes representations of a different set of processes. The model set-ups are well-chosen so that the differences between simulated d13C distribution allows the d13C ascribed to each process to be calculated.

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This type of approach contains, in general, non-additive 'delta' terms; meaning that the differences between the numerical model experiments may not precisely be equal to the amount 'due' to each process. This method, whereby highly idealised experiments are compared to each other to quantify the effects of different processes, is used successfully in the literature. The authors have discussed the possibility of non-additive 'delta' terms and considered the potential limitations of this in the manuscript.

I think a slight re-write is needed to give more emphasis to the results being model dependent. While I agree that the model has been tuned and seems to well simulate the spatial distribution of d13C – there could be other parameter combinations in different models that would seem equally well tuned. Subject to minor alteration to increase the prominence of the model-dependence of their conclusions in the text, and the clarification of a point raised below, I support the publication of the manuscript.

Specific issues:

Equations (4) and (5):

I think the details of the treatment of air-sea fluxes after Zhang et al (1995) of 13C needs some clarification in the text.

In Zhang et al (1995), epsilon_aq_g is a function of temperature. Since Zhang et al's epsilon = (alpha − 1.0)*1000 this also means that alpha_aq_g is a function of temperature. Why is the alpha_aq_g adopted here a constant, when according to Zhang et al's epsilon function, alpha should be function of sea surface temperature?

Also, Zhang et al (1995) use the fractions of DIC in the forms CO2*, HCO3- and CO32- to calculate alpha_DIC_g. In this manuscript it appears this is not done – with a straight linear temperature relation given (equation 5), irrespective of the component concentrations of DIC species.

Could the authors justify or explain these apparent differences between the full relationships of Zhang et al (1995) and their equations. It may be that I have misunder-

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stood, but if there are approximations simplifying the full equations of Zhang et al then could these approximations be highlighted better in the text? I am not suggesting that the model is coded incorrectly, just that the text does not clarify quite how the equations/approximations given are arrived at from the full DIC species-dependent relations in Zhang et al (1995).

Interactive comment on Biogeosciences Discuss., 10, 8415, 2013.

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