Response to reviewer 1

(Original reviewer comments are in red, our response in black, and changes to the text <mark>highlighted yellow</mark>.)

Thank you for a thoughtful and constructive review.

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.

The literature agrees that biology is more important than air-sea gas exchange for the depth distribution of d13C_DIC. For the surface distribution it has been suggested that both effects are equally important. So, our conclusions agree with the previous assessments on the depth distribution. However, they differ with regard to the surface distribution where we find the effect of biology to be larger than that of air-sea gas exchange.

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)?

No, the radiocarbon simulation is similar, if not even slightly improved, compared with Schmittner et al. (2008). Our speculation in that paper that air-sea gas exchange was too weak and that this caused the deep water to be slightly too old was probably wrong. Many other factors influence D14C distribution such as circulation and diffusivities. Schmittner et al. (2008) used UVic model version 2.7, that is different from version 2.8 used in this study in many ways, e.g. atmospheric parameters. Therefore, it is impossible for us to determine the exact reasons for the different D14C simulations between the different models.

A larger k0 exacerbates the high bias in global mean d13C_DIC.

In the revised manuscript we added the following text in subsection 2.1:

<mark>A 25 % reduced air-sea gas exchange leads to about a 0.1 ‰ lower global mean δ13CDIC and ~14 ‰ reduction in global mean Δ14C in our model. Note that the Δ14C simulation is consistent with observations and similar, perhaps even slightly improved, compared to that of Schmittner et al. (2008), who used the previous UVic</mark>

The authors' own results in the present paper show that the use of a higher **k0** would of course increase the role of air-sea exchange relative to biology on the distribution of **δ13CDIC**. 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 **δ13C** 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".

We agree that our conclusion with regard to the surface d13C_DIC depends on the accuracy of the k0 estimate. The conclusions section has been modified and now includes the following discussion.

This conclusion depends on the accuracy of the k0 estimate (Eq. 3). Sensitivity tests with a slightly different model version than the one presented here show improvements of the δ13CDIC and Δ14CDIC simulations by using k0 = 0.253 (NRMSEs of 0.53 and 0.26) compared with k0 = 0.337 (NRMSEs of 0.56 and 0.29) supporting Sweeney et al. (2007) and Graven et al. (2012). Although we cannot exclude somewhat higher values of k0 than used here, we believe that our results will be robust for modestly faster rates of air-sea gas exchange.

The temperature-dependent equilibrium fractionation factor from gaseous CO2 to DIC, **αDIC-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), **αDIC-g** = 1.01051 – 1.05 x 10-4T . 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.

We added the following paragraph to section 2.1:

Zhang et al. (1995) also found a secondary dependency of αDIC←g on pH, which they expressed as a function of the carbonate ion fraction fCO3. In the modern surface ocean fCO3 varies from about 5 % at high latitudes to about 20 % at low latitudes. This would cause αDIC←g to be about 0.05 ‰ higher in the tropics than at the poles. This is much smaller than the 3 ‰ effect of temperature alone. A sensitivity experiment including the pH effect showed a small (0.05 ‰) increase in simulated δ13CDIC in tropical surface waters. For all experiments presented in the reminder of this paper this effect has been neglected. For paleo applications in which dramatic changes in fCO3 can be expected it will be easy to include this effect.

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.

We have added this information in section 4 below eq. (12):

… $rC:O = 0.66$ is the stoichiometric ratio of carbon to oxygen in organic matter (Anderson and Sarmiento, 1994), …

after eq. (18)

with $rC:P = 112$

and after eq. (22)

($rP:O = rC:O/rC:P$)

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 $\delta 13CDIC$. 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 $\delta 13CDIC$ 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.

Thank you for the suggestion. We included such a figure as new Fig. 10 together with a brief discussion at the beginning of subsection 5.2.3.

Global mean surface-to-deep gradients of 1.0 ‰ in the observations are well captured in both models (Fig. 10). This is a considerable reduction compared to the preindustrial gradients of 1.7 ‰ (Fig. 8; subsection 5.1.2). Both models are biased high (globally by 0.09 ‰) particularly at mid depths. A high bias is not uncommon. For example, Sonnerup and Quay (2012) tested several models all of which were biased high by 0.13–0.51 ‰. Similarly the Tagliabue and Bopp (2008) models were also all high biased (e.g. below 1 km depth all were more than 0.45 ‰ too high) but the numbers cannot be precisely compared with ours because they did not present volume weighted averages and used a much sparser dataset.

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.

Done.