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Discussions

Interactive comment on “Southern Ocean controls of the vertical marine δ^{13}C gradient – a modelling study” *by* Anne L. Morée et al.

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1 General comments

1.1 Appreciation of the manuscript

Anne L. Morée and co-authors use the HAMburg Ocean Carbon Cycle Model in its configuration for long-term simulations, HAMOCC2s (Heinze and Maier-Reimer, 1999). The authors report the results of four sensitivity experiments (actually four plus two, as two out of the four are run in duplicate, once for the global ocean and once for the Southern Ocean) to analyse (1) the effect of variations of the air-sea exchange parameters, (2) the sequestration efficiency of the organic pump via changed particulate organic carbon (POC) sinking rates, (3) the sequestration efficiency of the organic pump

via increased nutrient utilisation efficiency, (4) changing sea-ice cover on atmospheric pCO$_2$, δ^{13}C of atmospheric CO$_2$ and δ^{13}C in the ocean, and more specifically on the global mean vertical gradient of δ^{13}C in dissolved inorganic carbon (DIC), quantified as the difference between DIC δ^{13}C in the surface and the deep ocean, denoted $\Delta\delta^{13}$C.

Upon reading the abstract of this paper I got really excited. Carbon isotopes are a particularly useful tool for studying carbon cycling between the different spheres that make it up. They have been used for a long time for this purpose, but over the past years a wealth of new data have been published and more and more comprehensive global data compilations have become available. The time is thus ready to re-assess the different mechanisms with a model that offers an excellent balance between the comprehensiveness of the processes taken into account and their complexity and execution time, so that meaningful simulation experiments can be carried out for time scales of tens to hundreds of millennia.

The manuscript itself, however, did unfortunately not meet my expectations, far from.

The language used, albeit generally fluent, seriously lacks precision and is rather colloquial. As an example, we repeatedly read that δ^{13}C is depleted or enriched. It is of course DIC that is depleted or enriched in ^{13}C. A relative deviation — such as δ^{13}C — cannot be enriched or depleted; it can be high(er), greater or low(er), decreased or increased.

The literature review is very poor; the same holds for comparison of the results obtained here to those of previous studies. Many important previous studies that called upon carbon isotopes for the study of glacial-interglacial carbon cycle changes are not cited (see below for details). $\Delta\delta^{13}$C, the proxy that is central to the paper really ought to be introduced with a more solid background. It was probably first used by Broecker (1982), at the very beginning of the “gold rush” time of the glacial-interglacial atmospheric CO$_2$

problem studies (1980s). It was then used as a proxy for glacial-interglacial pCO_2 variations, later fell out of favour, but has resurfaced over the past few years. One thing that would be important to emphasize here is, that it evolved in time: during those early stages, $\Delta\delta^{13}$C stood for the difference between δ^{13}C in the deep and the surface ocean DIC. In the recent studies (e.g., Ziegler et al. (2013)), it now most often stands for the difference between δ^{13}C of DIC in the deep sea and intermediate-depth (typically 400 m).

The model description is incomplete. The processes that are relevant for the study are not described at all, only a reference to a previous paper is given.

The experimental design leaves quite a number of questions open: the duration of the simulation experiments is only 2000 years. The separation between surface and deep ocean waters is questionable and as it obviously has an important influence on the results, the side-effects of this choice should have been discussed.

Not all of the figures are reader-friendly: on Figure 5, e.g., readers are expected to visually extract $\Delta\delta^{13}$C from latitude-depth transects of δ^{13}C by first averaging the topmost 250 m, then the depths below and to subtract both averages from each other.

As a consequence, I cannot recommend this manuscript for publication in *Biogeosciences* at this stage. It should nevertheless be possible to reconsider it after a major revision and I strongly encourage the authors to prepare a version that remedies to all the shortcomings mentioned here. Please provide us with a better description of what is done, how it is done and why it is done that way. The study deals with an interesting and timely subject. The biogeochemical model at hand perfectly fits the needs. Please take full advantage of the possibilities it offers!

2 Specific comments

2.1 Literature

Since the $\Delta\delta^{13}$C proxy has been in usage for more than 35 years, there is a wealth of studies that are available. They range from data-oriented studies to model-based studies, covering very similar approaches as done here. Only very few of them are cited in the manuscript and it is not entirely clear for what reasons they are included and others are excluded. There are more than 20 papers that come to my mind right away in this framework and that have not been considered in the literature review and the discussion of this paper

1. Broecker (1982)
2. Boyle (1988)
3. Keir (1988)
4. Oppo and Fairbanks (1990)
5. Oppo et al. (1990)
6. Keir (1991)
7. Mulitza et al. (1998)
8. Toggweiler (1999)
9. Flower et al. (2000)
10. Murnane and Sarmiento (2000)

11. Brovkin et al. (2002)
12. Hodell et al. (2003)
13. Köhler et al. (2005)
14. Köhler et al. (2010)
15. Lisiecki (2010)
16. Lourantou et al. (2010)
17. Tschumi et al. (2011)
18. Brovkin et al. (2012)
19. Menviel et al. (2012)
20. Peterson et al. (2014)
21. Menviel et al. (2015)
22. Eggleston et al. (2016)
23. Lear et al. (2016)
24. Menviel et al. (2017)

Please do not get me wrong: I do not expect all of these papers to be cited. However, even this "out-of-the-mind" list is simply so long (and still far from exhaustive) that it is incomprehensible that none of these studies has been cited or taken into account for the purpose of discussing the results.

2.2 Model description and experimental design

2.2.1 Model description is insufficient

The model description given in the paper neither allows to reproduce the results reported here without a lot of guesswork, nor does it allow to fully understand the results. The provided description is in some instances even confusing: on page 3 (lines 13–14), we read that "POC is carried as a tracer as well as transported downwards according to an exponential penetration depth and constant settling velocity, [. . .]". The exponential penetration profile and the constant settling velocity are of course not independent of each other. In the original technical reference to HAMOCC2s (Heinze and Maier-Reimer, 1999) – not cited in the manuscript – we read that "The vertical flux of biogenic particulate matter is parametrised through exponential redistribution profiles which implicitly include both sinking velocity and re-dissolution rate." This is not the same! It is quite easy to establish that the characteristic length scale in the exponential profile is equal to ω/k, if the (constant) settling velocity is denoted ω and POC respiration is assumed to follow first order kinetics with a rate constant k. Since one of the experiments involves changes of the settling velocity, the adopted parametrisations must be correctly described.

2.2.2 Model spin-up procedure

The description of the model spin-up procedure lacks important details. We only read that "[. . .] a fixed weathering input is used to tune the ocean inventories to values comparable to the observations." (page 3, line 24). On the basis of what quantitative constraints is this weathering flux determined? Are there separate fluxes

- for nutrients (phosphate)? – which would be necessary if organic matter is buried in the sediment together with the nutrients they lock up

- for DIC and alkalinity? – which would have to be separated if organic matter and carbonate are buried in the sediment
- for dissolved silica? – opal is also included in the model
- for ^{13}C? – what is the δ^{13}C signature of the DIC flux?

A decent model description would have answered half of the questions already. . . To what extent are the mismatches in the deep-ocean δ^{13}C and PO_4 concentration resulting from this spin-up? I would expect that they go together with global ^{13}C and PO_4 inventory mismatches as well, which, according to the description given here, are constraints.

2.2.3 Sensitivity experiment duration

The quality of the spin-up experiment is well quantified (residual drifts etc.). Unfortunately, nothing similar is reported for the sensitivity experiments. Readers are only told that these have been run for 2000 yr with the steady-state control run as initial condition. The strength of the model design for allowing long-term simulation experiments is initially emphasized (page 3, lines 10–11), a 110 000 yr spin-up run is carried out, and then the core experiments for the paper are run over a comparatively short duration of 2000 years only. For some of the perturbations (e.g., POC penetration depth changes. . .), the model carbon cycle is still in the transient phase 2000 years after the onset of the perturbation. The choice of such short simulation experiments is thus rather incomprehensible.

2.3 Analysis of the results

2.3.1 Up- and downward fluxes, equilibrium δ^{13}C

Analysis of the results involves up- and downward fluxes F_{up} and F_{down}: how are these obtained? To my best knowledge, it is only the net exchange flux F_{net} which is proportional to the p_{CO_2} difference between the surface water and the overlying atmosphere that can be calculated.

The equilibrium δ^{13}C ($\delta^{13}C_{\text{eq}}$, first mentioned on page 7 at line 1) is not defined and an explanation how this is calculated is missing as well.

2.3.2 Separation between surface and deep realms

In this study, the ocean is simply partitioned into a surface part, which encompasses the water masses above a 250 m depth horizon, and a deep part for the rest. No justification or explanation regarding this choice are given.

First of all, it is a choice that leads to complications. Information gathered from previous publications based up HAMOCC2s (Heinze et al., 1999, 2016) indicates that the eleven-layer configuration has no layer interface at 250 m depth, but a layer centred on 250 m depth. A more natural separation would be located at layer boundaries.

Secondly, this choice is critical as it controls the results of the study to a large extent extent. At 250 m depth, the depth profile of DIC δ^{13}C is generally rapidly decreasing (see e.g., Kroopnick (1985), but this should also be visible from the model results). Accordingly, the average surface ocean δ^{13}C will be strongly biased towards lower values and the deep ocean slightly towards higher values. As a consequence, the amplitude of the vertical gradient, $|\Delta\delta^{13}C|$, is thus systematically underestimated. I think that surface ocean δ^{13}C would more conveniently be taken from the surface layer down to 50 or 112.5 m depth (these are layer boundary depths in the 11-layer HAMOCC2s configuration, or even regionally variable in case information on the local mixed-layer depth would be available), and the deep ocean from the 1500 or the 2500 m depth horizons down to the sea floor.

In any case care must be taken in the model-data comparison to make sure that surface-to-deep model gradients are compared to surface-to-deep data gradients and not to intermediate-to-deep data gradients.

2.3.3 Regionalization

$\Delta\delta^{13}$C results are only shown in the global mean. The three-dimensional HAMOCC2s should allow for a finer analysis than that. In the text, regional $\Delta\delta^{13}$C outcomes are sometimes mentioned, but it would be useful to have these results reported graphically as well, at least for basins or sub-basins (e.g., North Atlantic, South Atlantic, North Pacific, South Pacific, Southern Ocean). Figure 4 could be easily adapted to show such more regionalized values in a useful and expressive way.

2.4 Discussion shortcomings

Parts of the discussion are rather confusing. Section 3.4 is one of them. On one hand, we read that "The idealised and large perturbations [. . .] show that mean $\Delta\delta^{13}$C varies no more than 0.5 ‰" on the other hand that "[the] reconstructed intra-millennial variability in $\Delta\delta^{13}$C could be driven more by changes in the biogeochemical state than by changes in ocean circulation because (bio)geochemical changes might occur more rapidly than whole-ocean circulation changes." Are *large* and *whole-ocean* changes in the biogeochemical state of the ocean really that more realistic on the time scales of a

few millennia than circulation changes?

At the latest from page 10, lines 29–30 on it is not clear any more which conclusions to draw from this study. Readers that have come this far will have seen the discussion revolve around SO $\Delta\delta^{13}$C in several instances, to learn now that, except for the North Atlantic, "data are too sparse to get a coherent picture of $\Delta\delta^{13}$C variations". Previously we have been shown that in the North Atlantic the deep-sea δ^{13}C is mainly controlled by the air-sea exchange δ^{13}C.

3 Technical comments

Throughout the paper: please check precision of the language. . .

Page 1, lines 3 and 27–28: "The standardised ^{13}C isotope, δ^{13}C, [. . .]": I have never seen this denomination in the peer-reviewed literature before. δ^{13}C expresses the molar ^{13}C/^{12}C ratio of a sample in terms of its relative deviation from the ratio in a standard (initially PDB, now VPDB), generally expressed in permille. The references provided for this "standardised ^{13}C isotope" are incomprehensible: Stenström et al. (2011) is a non peer-reviewed internal university report, Stuiver and Polach (1977) deals with radiocarbon. It should be straightforward to find an appropriate textbook reference for δ^{13}C.

Page 2, line 3: "air-se" should read "air-sea"

Page 2, line 6: "10s" should be written out as "tens"

Page 2, line 24: the study by Shackleton and Pisias (1985) absolutely needs to be cited here alongside Charles et al. (2010) and Oliver et al. (2010).

Page 3, line 17: should “HAMOCC2” not read “HAMOCC2s”?

Page 3, line 23: should “HAMOCC2” not read “HAMOCC2s” again?

Page 4, line 23: “Eide (2017)”: 2017a or 2017b?

Page 4, line 23: on the basis of the provided mean values, the intercept of the δ^{13}C:PO$_4$ relationship is 3.27733, which would normally be rounded to 3.3, not to 3.4. Please check the numbers.

Page 5, line 12: “The modelled global POC production is [...]”: I guess this is the *new* or the *export* production – please clarify!

Page 5, lines 26–27: “[...] with the exception of the Arctic Ocean where no POC production is modelled due to the sea ice cover [...]”: elsewhere in the paper we read that the sea-ice cover also isolates the surface ocean with respect to air-sea exchange. Does the partitioning into $\delta^{13}\mathrm{C}^{\mathrm{perc}}_{\mathrm{bio}}$ and $\delta^{13}\mathrm{C}^{\mathrm{perc}}_{\mathrm{atm}}$ make sense in ice-covered regions?

Page 6, line 4: “change more than” should read “change by more than”

Page 6, line 9: “[...] due to the fact that ^{12}C needs to speciate [...]”: this does not make sense. ^{12}C can only equilibrate at the same time as ^{13}C – there are only the two of them. ^{12}C should probably be corrected to DIC or $\mathrm{CO}_{2\,(\mathrm{aq})}$.

Page 6, line 19: “[...] 22% of the global ocean area [...]”: does this include the ice-covered parts of the SO? – please specify

Page 6, line 21: “F_{u}” should read “F_{up}”

Page 6, line 22: “F_{d}” should read “F_{down}”

Page 6, line 30: “[...] lowers the surface ocean δ^{13}C -0.2 to -0.9‰ in the lower latitudes [...]” should read “[...] lowers the surface ocean δ^{13}C by -0.2 to -0.9‰ at the lower latitudes [...]”

Page 6, line 31: “in high latitudes” should read “at high latitudes”

Page 6, line 31–32: “These results indicate the sign of the thermodynamic δ^{13}C disequilibrium between surface ocean and atmosphere.” – this sentence does not make sense, please reformulate.

Page 7, line 7: please add the ‰ sign to the 0.65 and the 1.00

Page 7, lines 16–17: “A more efficient biological pump [...] leads to a loss of carbon to the sediments, which dominates the effects on $p\mathrm{CO}_2^{\mathrm{atm}}$ and $\delta^{13}\mathrm{C}^{\mathrm{atm}}$.”: after 2000 years of simulation these effects have certainly not yet developed to their full strength.

Page 7, lines 24–25: “remineralisation horizon”: a horizon depicts, in my understanding, a surface or a narrow zone, such as the calcite saturation horizon. I am not aware of the existence of a POC remineralisation horizon (and not even a carbonate remineralization horizon). Please rewrite.

Page 7, lines 29–30: “When reducing the biological pump efficiency both remineralisation and POC production are confined to the surface ocean.”: as far as I know

HAMOCC2s, the POC production is always confined to the surface and the remineralisation is taking place in subsurface intermediate and greater depths. Would “With a lower POC sinking rate, the remineralisation is confined to shallower depths.” not be more correct?

Page 8, lines 5–9: Figure 5 which is referred to here, depicts δ^{13}C and DIC anomalies with respect to the control run. Having readers derive information about $\Delta\delta^{13}$C from that figure is really asking too much. Why not provide the latitudinal evolution of the $\Delta\delta^{13}$C alongside? This would be a straightforward line plot.

Page 9, section 3.3.4: I would expect that such large ice-cover changes would also lead to circulation changes. A comment on this would be of order, wouldn’t it?

Page 9, line 25: $\delta^{13}\mathrm{C}_{\mathrm{eq}}$: see above

Page 10, lines 23–24: “Analysis of SO $\Delta\delta^{13}$C reconstructions from sediment cores at 42°S and 46°S (Charles et al., 2010) shows that there is a strong correlation between these cores and Northern Hemisphere $\Delta\delta^{13}$C variations.” This is not correct. Charles et al. (2010) show that there is a tight correlation between SO $\Delta\delta^{13}$C and “Northern Hemisphere climate fluctuations”; their paper does not even mention any $\Delta\delta^{13}$C record outside the SO.

Figures: if $\Delta\delta^{13}$C informations are to be read from a figure, this latter should then also show $\Delta\delta^{13}$C.

Page 24, Figure 8b: units for $p\mathrm{CO}_2$ on the vertical axis should be ppm or μatm on the vertical axis, not ‰.

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