Author responses to Referee #2

Comment 1 General comments

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 pCO2, δ^{13}C of atmospheric CO2 and δ13C in the ocean, and more specifically on the global mean vertical gradient of δ13C in dissolved inorganic carbon (DIC), quantified as the difference between DIC δ^{13}C in the surface and the deep ocean, denoted Δδ13C. 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 δ13C is depleted or enriched. It is of course DIC that is depleted or enriched in 13C. A relative deviation — such as δ13C — 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). Δδ13C, 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 CO2 problem studies (1980s). It was then used as a proxy for glacial-interglacial pCO2 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, Δδ13C stood for the difference between δ13C 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 δ13C 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 ∆δ13C from latitude-depth transects of δ13C 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!

Author’s response to Comment 1

Thank you for your detailed and thorough review of our manuscript. We appreciate the effort you have made to improve it: See below for a detailed reply to your comments. Regarding the points you only make in Comment 1, we will improve the precision of the language in general, and when discussing δ13C depletion/enrichment/increasing/decreasing. We will also extend the introduction to include a paragraph on the development of ∆δ13C research.

Author’s changes in the manuscript in response to Comment 1

- Adjust the mention of ‘four sensitivity experiments’ to ‘a set of sensitivity experiments’
- Replacing enriched/depleted when referring to δ13C with increased/decreased or higher/lower throughout the manuscript. Check language in the manuscript and improve where not precise.
- Add information on the development of ∆δ13C research in the introduction, based on a selection of the papers mentioned in Comment 2.1.

Comment 2.1 Literature

Since the ∆δ13C 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
[... literature list provided by Referee #2 ...]
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.

Author’s response to Comment 2.1

Thank you for providing us with this literature list. We understand your wish for a stronger literature review and discussion, and made a selection of the literature you provided for our discussion/introduction.

Author’s changes in the manuscript in response to Comment 2.1

- Where relevant, we will refer to and discuss additional previous studies. Specifically, we will to focus on

  Broecker (1982)
  Boyle (1988)
  Shackleton et al. (1983)
  Oppo et al (1990)
  Keir (1991)
  Mulitza et al. (1998)
  Toggweiler (1999)
  Murnane and Sarmiento (2000)
  Köhler et al. (2010)
  Lisiecki (2010)
  Lourantou et al (2010)
  Tschumi et al (2011)
  Menviel et al (2015)
  Eggleston et al (2016)
  Lear et al. (2016)
  Menviel et al. (2017)
  Duplessy et al. (1988)

Comment 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 MaierReimer, 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.

Author’s response to Comment 2.2.1

We will correct the description of the POC sinking (Page 3, lines 13-14) to agree with the technical report (Heinze and MaierReimer, 1999). Furthermore, we will provide additional information on how sea ice, nutrient uptake and air-sea gas exchange are parameterised in the model, so that the reader can better understand the changes we made for the sensitivity experiments.

Author’s changes in the manuscript in response to Comment 2.2.1

- Add reference to the technical report on HAMOCC2 (Heinze and Maier-Reimer, 1999) in addition to the currently used reference to Heinze et al. (2016) - which contains the model version closest to the current one
- Provide information on parameterisation POC remineralisation/sinking
- Extend the paragraph on Page 4, lines 4-13 to complement Table 1 in how the experiments were set up. Thereby explain how changing Vmax relates to the Michaelis Menten kinetics and how the air-sea gas exchange experiments change the gas transfer coefficient. We will also add that the model is set up to make sea ice limit gas exchange and light penetration based on ice fraction.

Comment 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 13C? – what is the δ13C 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 δ13C and PO4 concentration resulting from this spin-up? I would expect that they go together with global 13C and PO4 inventory mismatches as well, which, according to the description given here, are constraints.

Author’s response to Comment 2.2.2

The weather flux is determined as described on P.3 lines 24-26: ‘The ‘best-fit’ weathering value was found by running the model with a restored (to a value of -6.5 ‰) atmospheric δ13C until the prognostic burial rate reached equilibrium with weathering (after ~110000 model years).’ So initially, the weathering flux was set to equal the prognostic sediment burial, and atmospheric carbon was restored to -6.5 ‰ (δ13C) and 278 ppm (pCO2atm). After ~110000 model years the burial (and thus weathering) flux equilibrated to a constant value. This also led to an ocean δ13C distribution that was closer to observed values. In consecutive model runs, we removed the restoring of the atmosphere and fixed the weathering rate to the value we obtained at the end of the 110000 year run, while keeping the burial rate prognostic. In this way, the ocean inventories remained close to observed, while permitting free atmospheric change.
Weathering fluxes are added homogeneously over the first ocean layer as dissolved matter. They are added in a fixed stoichiometric ratio for 12C, 13C, 14C, Alk, PO4 and Si. The 13C/12C ratio in the weathering flux would be equivalent to a δ13C of DIC of 14 ‰.

Author’s changes in the manuscript in response to Comment 2.2.2

- Extend the explanation of the spinup procedure regarding burial/weathering to include tracers, stoichiometry, more details about the procedure and quantitative measures used
- The model description will be improved in general in the manuscript, see comment 2.2.1

Comment 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.

Author’s response to Comment 2.2.3

The length of the sensitivity experiments is chosen to be 2000 years, as we observed the air-sea gas exchange rate and sea ice cover experiments to show very little change in atmospheric carbon signature after this time (new SI Figure on atmospheric development, see last page of response to Referee #1). We agree that the effects of changing the biological pump (i.e., the POC and Vmax experiments) are still ongoing after 2000 years. To reach full isotopic equilibrium in the ocean however, over 200 000 model years of runtime could be needed (Roth (2014), see adjustments made to Page 7, lines 16-17). In an open system, the sediment loss of nutrients and carbon over time will empty the whole ocean of nutrients, which would not give very meaningful results. Besides that, over 200 000 years other feedback processes would happen as well in reality. To show the continued effect of a change in the biological pump efficiency, we will provide the reader with atmospheric development results of an extra 10 000 years for the fast POC sinking rate experiment in the SI.

Author’s changes in the manuscript in response to Comment 2.2.3

- Provide the reader with the atmospheric development per sensitivity experiment of pCO2 and δ13C in the SI, see last page of response to Referee #1 document
- Clarify in sections 3.3.2 and 3.3.3 that there are still ongoing changes in the model
- Add δ13C and pCO2 development figure in SI for the fast POC sinking rate experiment for an additional 10 000 years

Comment 2.3.1 Up- and downward fluxes, equilibrium δ13C

Up- and downward fluxes, equilibrium δ13C Analysis of the results involves up- and downward fluxes Fup and Fdown: how are these obtained? To my best knowledge, it is only the net exchange flux Fnet which is proportional to the pCO2 difference between the surface water and the overlying atmosphere that can be calculated. The equilibrium δ13C (δ 13Ceq, first mentioned on page 7 at line 1) is not defined and an explanation how this is calculated is missing as well.

Author's response to Comment 2.3.1

In the model, separate fluxes Fup and Fdown are calculated by splitting the gas transfer formulation into to parts.
The gas transfer formulation as for example described in for example Orr et al. (2017):
'FA = kw ([A]sat − [A]), where for gas A, kw is its gas transfer velocity, [A] is its simulated surface-ocean dissolved concentration, and [A]sat is its corresponding saturation concentration in equilibrium with the water-vapor-saturated atmosphere at a total atmospheric pressure Pa.'
In the model, Fup=kw*[A]sat and Fdown=kw*[A] and Fnet=Fup-Fdown. This splitting up is useful for the calculation of air-sea fractionation of the carbon isotopes, as the fractionation factor only needs to be multiplied with Fup when calculating the effects of the air-sea gas exchange. It also proved to be useful for our discussion, because the total amount of exchange Fu+d influences δ13C.
The paragraph above will be added to the SI in order to explain our calculation method.

For clarification of the use of δ13Cdiseq and δ13Ceq, we add a definition of both to Page 6. L15: '[...] depending on the prior disequilibrium δ13Cdiseq (δ13Cdiseq = δ13C - δ13Ceq, where δ13Ceq represents the δ13C signature a water parcel would have had if it would have fully equilibrated with the atmosphere).'
δ13Ceq is not calculated, but the Gas Fast experiment provides insight into where the surface ocean is over or undersaturated with respect to δ13C, as the air-sea gas exchange rate is increased. Galbraith et al. 2015 provide more information on δ13Ceq and δ13Cdiseq and the study by Schmittner et al. 2013 also explored an infinite gas exchange rate.

Author's changes in the manuscript in response to Comment 2.3.1

- Adding a section in the SI on the calculation of Fup and Fdown
- Add to Page 6. L15: '[...] depending on the prior disequilibrium δ13Cdiseq (δ13Cdiseq = δ13C - δ13Ceq, where δ13Ceq represents the δ13C signature a water parcel would have had if it would have fully equilibrated with the atmosphere).'

Comment 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 δ13C is generally rapidly decreasing (see e.g., Kroopnick (1985), but this should also be visible from the model results). Accordingly, the average surface ocean δ13C will be strongly biased towards lower values and the deep ocean slightly towards higher values. As a consequence, the amplitude of the vertical gradient, |∆δ13C|, is thus systematically underestimated. I think that surface ocean δ13C 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.

Author’s response to Comment 2.3.2

We see the potential problem with choosing the surface ocean as above 250 m depth and the deep ocean as below 250 m depth. We will adjust the definitions to better fit the model design and to prevent the strong surface ocean δ13C gradient to influence the averaging too much. In order to do so, we define the model photic layer (top 50 meter) as the ‘surface ocean’, because this is where biological production and fractionation during air-sea gas will mostly increase δ13C. We define ‘the deep ocean’ as the lowest model layer above the sea floor (if this over 3 km depth), as this is were benthic foraminifera will dwell and this ocean volume will be least influenced by the strong gradient in the vertical δ13C profiles (which could influence ∆δ13C). We realise that due to the different definitions used for ∆δ13C over the past decades/in different studies, no definition chosen by us will make direct comparison with a previous study possible. We feel however that by providing basin-averaged vertical gradients of δ13C, the reader could deduce their gradient of interest, or directly use the ∆δ13C we will report.

Author’s changes in the manuscript in response to Comment 2.3.2

- Adjust ∆δ13C definition on Page 4, lines 1-3 to include the surface ocean as the ocean above 50 m depth/the photic zone and the deep ocean to be the lowermost wet layer in the ocean, if above 3 km depth.
- All reported ∆δ13C values changed to fit the new definition of ∆δ13C

Comment 2.3.3 Regionalization

∆δ13C results are only shown in the global mean. The three-dimensional HAMOCC2s should allow for a finer analysis than that. In the text, regional ∆δ13C 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.

Author’s response to Comment 2.3.3

We see the need for a less generalized and more basin/specific discussion of the results. This would also support a better comparison with sediment core studies/observational data.

Author’s changes in the manuscript in response to Comment 2.3.3

- We will revise Figure 4 to show basin-mean δ13C profiles per global sensitivity experiment for the North Atlantic, South Atlantic, North Pacific, South Pacific, Southern Ocean and Indian Ocean, with the value for ∆δ13C stated besides the profile. See also comment 1 to Referee #1. Part of this plot might be put in the SI, in order to not overwhelm the reader. The ∆δ13C value is based on the new ∆δ13C definition.
- Adjust the results, discussion and conclusion sections to use and describe the adjusted Figure 4 (i.e. results on basin scale)

Comment 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 ∆δ13C varies no more than 0.5‰” on the other hand that “[the] reconstructed intra-millennial variability in ∆δ13C 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 ∆δ13C in several instances, to learn now that, except for the North Atlantic, “data are too sparse to get a coherent picture of ∆δ13C variations”. Previously we have been shown that in the North Atlantic the deep-sea δ13C is mainly controlled by the air-sea exchange δ13C.

Author’s response to Comment 2.4

We will clarify the discussion by putting it in a broader context (extended literature study, see comment 2.1) and by discussing results on a basin scale.

Author’s changes in the manuscript in response to Comment 2.4

- Incorporation in the discussion of additional literature (see comment 2.1)
- Discussing the results on a basin scale
- Restructure section 3.4, remove Page 10 lines 21-23

Comment 3 Technical comments

Page 1, lines 3 and 27–28: “The standardised 13C isotope, δ 13C, [. . .]”: I have never seen this denomination in the peer-reviewed literature before. δ13C expresses the molar 13C/12C 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 13C 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 13C.
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 13C:PO4 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 δ13C perc bio and δ13Cpercatm 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 12C needs to speciate [. . .]”: this does not make sense. 12C can only equilibrate at the same time as 13C – there are only the two of them. 12C should probably be corrected to DIC or CO2 (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: “Fu” should read “Fup”
Page 6, line 22: “Fd” should read “Fdown”
Page 6, line 30: “[. . .] lowers the surface ocean δ13C −0.2 to −0.9 ‰ in the lower latitudes [. . .]” should read “[. . .] lowers the surface ocean δ13C 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 δ13C 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 pCOatm 2 and δ13C 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 δ13C and DIC anomalies with respect to the control run. Having readers derive information about Δδ13C from that figure is really asking too much. Why not provide the latitudinal evolution of the Δδ13C 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: δ13Ceq: see above

Page 10, lines 23–24: “Analysis of SO Δδ13C 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 Δδ13C variations.” This is not correct. Charles et al. (2010) show that there is a tight correlation between SO Δδ13C and “Northern Hemisphere climate fluctuations”; their paper does not even mention any Δδ13C record outside the SO.

Figures: if Δδ13C informations are to be read from a figure, this latter should then also show Δδ 13C.

Page 24, Figure 8b: units for pCO2 on the vertical axis should be ppm or µatm on the vertical axis, not ‰.

Author response to Comment 3

We apologise for the mistakes/lacking information at the points you have listed. We will clarify and correct the manuscript accordingly. See below for details.

Author’s changes in the manuscript in response to Comment 3

Page 1, lines 3 and 27–28: Replaced

‘The vertical marine δ13C gradient is the surface-to-deep difference in δ13C, the standardised 13C isotope (Stenström et al., 2011; Stuiver and Pollack, 1977). 13C is slightly heavier than the 12C isotope, which causes a fractionation effect during air-sea gas exchange and biogenic carbon uptake during photosynthesis (Laws et al., 1997; Mackenzie and Lerman, 2006; Zhang et al., 1995).’

By

‘The vertical marine δ13C gradient is the surface-to-deep difference in δ13C, the standardised 13C/12C ratio expressed in permil (Eq. 1) (Zeebe, 2001). 13C is slightly heavier than the 12C isotope, which causes a fractionation effect during air-sea gas exchange and biogenic carbon uptake during photosynthesis thereby changing the 13C/12C ratio (Laws et al., 1997; Mackenzie and Lerman, 2006; Zhang et al., 1995).’

$$\delta^{13}\mathrm{C} = \left(\frac{\left(\frac{^{13}\mathrm{C}}{^{12}\mathrm{C}}\right)_{\mathrm{sample}}}{\left(\frac{^{13}\mathrm{C}}{^{12}\mathrm{C}}\right)_{\mathrm{standard}}} - 1 \right) * 1000\ ^{o}/_{oo}$$

, (Eq. 1)

where we used the PDB (13C/12C)standard (0.0112372).

Zeebe, R., & Wolf-Gladrow, D. (2001). CO2 in Seawater: Equilibrium, Kinetics, Isotopes (Vol. 65). Amsterdam, The Netherlands: Elsevier Science B.V.

Page 2, line 3: Corrected as suggested

Page 2, line 6: Corrected as suggested

Page 2, line 24: Added reference to Shackleton and Pisias (1985)

Page 3, line 17: Corrected to HAMOCC2s

Page 3, line 23: Corrected to HAMOCC2s

Page 4, line 23: This should be 2017b, corrected accordingly

Page 4, line 23: Lines 18-19 are meant here, this should indeed be 3.3 - corrected

Page 5, line 12: Corrected to “The modelled global export POC production is [. . .]”

Page 5, lines 26–27: Partitioning in air-sea gas exchange and biological components does mean something in ice-covered regions, as the upstream signal will be visible in such regions, and if the water mass transports POC, the biological-remineralization signal can increase with water mass age under the ice as well.

Page 6, line 4: Corrected as suggested

Page 6, line 9: Corrected as suggested

‘This difference in equilibration time is due to the fact that 12C needs to speciate into all marine carbon species to reach equilibrium (~20x slower than O2), after which 13 10 C needs to go through full isotopic exchange between all carbon species to reach equilibrium (~10x slower than 12C) (Jones et al., 2014; Galbraith et al., 2015).’

to

‘This difference in equilibration time is due to the fact that DIC needs to speciate into all marine carbon species to reach equilibrium (~20x slower than O2), while 13C needs to go through full isotopic exchange between all carbon species to reach equilibrium (~10x slower than DIC) (Jones et al., 2014; Galbraith et al., 2015).’

Page 6, line 19: “[. . .] 22% of the global ocean area [. . .]” corrected to “[. . .] 22% of the global ice-free ocean area [. . .]”

Page 6, line 21: Corrected as suggested

Page 6, line 22: Corrected as suggested

Page 6, line 30: Corrected as suggested

Page 6, line 31: Corrected as suggested

Page 6, line 31–32: “These results indicate the sign of the thermodynamic δ13C disequilibrium between surface ocean and atmosphere.” adjusted to “These results show whether the thermodynamic δ13C disequilibrium δ13Cdiseq is positive or negative.”

For clarification of the use of δ13Cdiseq and δ13Ceq, we add to Page 6. L15: ‘[...] depending on the prior disequilibrium δ13Cdiseq (δ13Cdiseq = δ13C - δ13Ceq, where δ13Ceq represents the δ13C signature a water parcel would have had if it would have fully equilibrated with the atmosphere).

Page 7, line 7: Corrected as suggested

Page 7, lines 16–17: The authors agree that these effects have not yet developed to their full strength, and will adjust the sentence to ‘A more efficient biological pump (here, a higher POC sinking rate) leads to a loss of carbon to the sediments, which affects pCO2atm and δ13Catm long-term (reference to new SI Figure on atmospheric development during the experiments, see last page of response to Referee #1 document), as found in a model study by Roth (2014).’

Page 7, lines 24–25: “remineralisation horizon” replaced by ‘POC remineralisation’

Page 7, lines 29–30: We will rephrase this sentence to “With a lower POC sinking rate, the remineralisation is confined to the surface ocean.”

Page 8, lines 5–9: We will address the issue with the visualisation of ∆δ13C by presenting basin-specific δ13C profiles in an adjusted Figure 4 (see also comment 1 to Referee #1), with a basin-average ∆δ13C noted next to each profile. Referral to that new figure instead of Figure 5 should provide the reader with enough information to understand the effects of the sensitivity experiment on ∆δ13C.
Page 9, section 3.3.4: We will add a sentence on Page 9, line 12 to state 'Ocean circulation changes that could result from a changed sea ice cover are not taken into account, as we want to study the potential isolated effect of sea ice on biological production and air-sea gas exchange.'
Page 9, line 25: See response to Page 6, line 31–32
Page 10, lines 23–24: In discussing our results in view of more literature, we will also correct the comparison and discussion with Charles et al. (2010) their results.
Figures: When referring to a figure when discussing or presenting ∆δ13C, we will in the adjusted manuscript refer to the basin-specific δ13C profiles that is presented in an adjusted Figure 4, and includes a value for ∆δ13C for each basin. The ∆δ13C value is based on the new ∆δ13C definition.
Page 24, Figure 8b: Corrected to ppm

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