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Dear Dr. Paul / Dear André,

Thank you for handling with care our manuscript. Below you will find our point-to-point response to the most recent remarks of a referee, that (once more) helped us a lot to further improve the quality of our manuscript. Recent changes of the text are marked **red** in the manuscript we submit.

Major comments. Highlighting the major findings/points:

I welcome the added outline of the manuscript in lines 274-317 that simultaneously emphasizes the highlights of the manuscript. But paragraph 274-317 needs to be significantly shorted to one paragraph in my view. Any introduction of supplementary material is unnecessary here.

The paragraphs of lines 274-317 now were shortened by half a page to one paragraph.

A novel aspect of the study seems to be the extension of the plateau tuning technique to 23-29 kyr BP, which allows to define new chronostratigraphic markers for that time interval. However, reservoir age estimates from this time interval are not discussed at all

All right! We now added three lines of text (new lines 537-539). At least two records show reservoir ages covering HS-2 located prior to 23 cal. ka.

The reassessment of earlier records with the adjusted chronology for the LGM and younger time interval seems to support previous findings (judging from the references given to earlier work of the first authors' group in the discussion). I think the paper would benefit from highlighting the knowledge gained from adjusting the chronology of the records rather than reiterating what these records as a whole show because this was done in numerous previous publications of the first authors' group. at least this could be done in a more concise way. Currently, it is hard to distill the main messages of the paper, in particular in the "Discussion and implications" section.

The conclusions now spell out, paragraph-by-paragraph, which results we regard as main and "new" messages of this paper, that is, all results based on the global relevance of atmospheric ^{14}C structures for stratigraphy.

When the authors write "These new features of MOC and the carbon cycle" (line 55) and "a new understanding of Ocean MOC during the LGM and its reversal during HS-1" (line 316) it is difficult to say what is exactly the novel piece of information here, also in comparison to the previous knowledge obtained from other proxies. This should be rewritten and/or more clearly carved out.

Our new results have been compared with previous knowledge obtained from other proxies ($\delta^{13}\text{C}$, Nd isotopes), e.g., in Section 3.3.1 and 3.3.2 (new line 673-680, 695-696, 753, and 759-764).

In those instances, where new insights can be gained and are introduced, I have to admit that it is really hard to follow the argumentation, in particular when the (raw) data is not discussed or described (as in the case of the Küssner or Ausin data). This applies for instance to line 526: "reveal that changes began ~1400 yr earlier in the north (Fig. S2)", where it is unclear what is meant by "north", how this number of 1400 yr came about, and what is meant by changes.

The first two examples/statements in the delay of Northern Hemisphere deglaciation (Ch. 3.1; new lines 496-501) were now reworded. The '1400-yr delay' was deleted. Also, the 2nd statement was corrected to better specify the role of local warmings and coolings. The full argumentation of the conclusions i – vi, based on the raw data and their summary in Figs. 8, 10, and S2 cannot be given here because of restricted space.

The same applies to lines 529-532 or to line 709-710 “in particular due to a ‘thermal threshold’ (Abé-Ouchi, pers. comm.) overlooked in other model simulations.”.

The 'thermal threshold' includes the idea of a decreasing influence of the temperature influence on differential densities and deep-water formation near to the lower temperature limit of seawater (diagram of Cox, 1969).

Streamlining

I think some paragraphs are still long-winded and contain a lot of detail that are likely not necessary for the main message of that paragraph. This applies to the first and second paragraph of section 2.2. Also, I find the exercise performed in lines 440-464 to highlight the ocean carbon cycle influence on atmospheric 14C somewhat redundant. I think the same message (i.e. atmospheric 14C variability cannot be explained by production changes alone) can be conveyed by simply referring to existing literature, e.g. (Hain et al., 2014) or others; hence allowing to shorten the paragraph.

-- The 1st two paragraphs of section 2.2 form a basic portion of our results, a dilemma, since they also need to be digested by 'outsiders', say modelers. To follow the reviewer, however, we shortened them by ~2 lines.

-- Former lines 440-464.

We are happy to add a citation of Hain et al. (2014). Their statements, however, are solely based on model simulation whereas our text is based on actual data. Thus we regard this paragraph essential for our manuscript, however, we now have shortened it by two lines.

In lines 486-491, I find the calculation of F-modern equivalent of a ventilation age change unnecessary. This can be removed in my view.

In view of most recent evidence for improved plateau tuning at Site SO76-136 we now deleted the indicted paragraph.

Also the section on the influence of habitat changes on planktic foraminiferal 14C ages could be summarized in one sentence, to my mind (e.g. “The fact that 14C ages of co-existing planktic foraminifera were suggested to be influenced by habitat depths, in particular when comparing surface and sub-surface dwellers (refs), or seasonality (refs), makes a closer specification of model results as product of different seasonal extremes a further target.”).

(Former lines 647-660) The paragraph now was shortened by two lines. However, we regard these results essential to present specific findings (by now published in a micropaleontological journal only) instead of mere arm waiving.

Model-data comparison (Section 3.2.)

I am still confused why the study of Muglia et al., (2018) was chosen for model data comparison, and what the purpose of it is. This is in part because the background on the actual model simulation is missing (how were the reservoir ages estimated? How was the model forced?). Although some of this information is probably given in Muglia et al. (2018), in my view this information should be stand-alone in the paper. The comparison to previous surface reservoir modelling studies (Franke, Butzin) is quite limited and a reiteration of personal communications should be avoided. It seems that the authors have chosen the Muglia study because it seemingly reproduces the best match to their data, but what is the purpose then of the model data comparison?

- We now slightly modified and abbreviated the title of section 3.2. The aim of our data, generated by plateau tuning, is to provide additional boundary conditions on which to test and further refine ocean circulation models. Our comparison indicates that models using more detailed input and refinement better reproduce the spatial and temporal variability revealed in our data.
- On the whole, section 3.2 is summarizing a number of crucial aspects, figures, and caveats linked to foraminifera-based reservoir ages, that we do not like to delete.
- Six lines were abbreviated in §1, three lines in § 2. New line 575: The personal advise of Butzin was crucial for the generation of this manuscript version, thus needs to be reported, the wording, however, was further reduced.
- On the whole we shortened our manuscript text by ~30 lines.

Unpublished data

I still do not support the inclusion of the Ausin et al. unpublished data in the interpretation, and the Küssner et al. submitted, simply because this cannot be assessed by the reader at the moment and likely not at the time of publication. Simply referring to the Pangaea database (in the replies to the reviewers) is not a best practice to discuss the new data, explain how they have been obtained and how surface R were estimated, etc. I expect a better solution here.

and EDITOR's LETTER

Not all data are described and not all arguments are explained, there is merely a reference to a supplementary figure (S2) or unpublished data, which I do not find satisfactory.

The foraminifera-based data sets of Ausin and Küssner et al. are stored at PANGAEA databank, under embargo as the papers are still in preparation. As with all other ^{14}C records, the R values at these five sites were estimated precisely on the basis of the techniques shortly outlined in section 2.1, with more detail in Sarinthein et al. (2015). The caption of Fig. S2 now is explicitly mentioning the data storage at PANGAEA databank.

With regard to Küssner et al. (manuscript largely ready for submission), we feel sorry that for reasons of serious disease, the manuscript has not been further processed yet for submission, thus needs to be downgraded to "in prep.", though containing a rich collection of four crucial records of past changes in R values.

Minor comments:

Line 199. Far superior evidence -> is not adjusted although described in the replies to reviewers.

In our view the term 'far superior' appears justified since the plateau-tuning technique

provides more accuracy, less uncertainty and far better time resolution of other R value records. Also important, it provides a coherent set of R values instead of rare and widespread single estimates.

Line 265-269 Move “We prefer the Suigetsu record to IntCal [...] including carbonate-based speleothem and marine records.” to the preceding paragraph.

Thanks! We agree.

Line 415. Reference your earlier work here.f

A ref. to Sarnthein et al., 2015, is already given in the caption of Fig. 3.

Line 486. There is no Fig. 4c that references the Küssner data subm.

The paragraph was now deleted.

Throughout manuscript: replace plankton-based with planktic foraminifera-based (planktic foraminifera are plankton but not all plankton is planktic foraminifera)

Done.

Again we thank for your handling of our manuscript with that much care. Hoping that the quality of our manuscript may now be sufficient for being accepted for publication,

Sincerely,

Michael Sarnthein and coauthors