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Interactive comment on "Plateaus and jumps in the atmospheric radiocarbon record – Potential origin and value as global age markers for glacial-to-deglacial paleoceanography, a synthesis" *by* M. Sarnthein et al.

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–> General statements (paragraphs on page C1 - C2) (the author's response is marked by –> signs)

–>Thank you for numerous very helpful comments to improve the quality of our manuscript.

The method has not been extensively used outside the first author's group, which might be associated with some sceptisism towards this method.

–>It is common to novel hypotheses and techniques to take some time until their acceptance by a dominantly conservative community that even may try to suppress a new approach. By now, the approach of 14C plateau tuning has been successfully applied to more than 20 records of marine sediment cores, yielding a consistent output from regions with high sedimentation rates (>10cm/ky).

This paper is therefore very welcoming as it presents new findings regarding the age scale of the target atmospheric (Suigetsu) 14C record, uncertainties of this age scale at plateau boundaries, and 14C reservoir age changes in the ocean.

–>The positive judgement of our efforts is acknowledged.

Assuming the robustness of the method, the authors suggest that it provides "precise" chronostratigraphic control for marine sediment cores

–>We now replace "precise" by "accurate", a term that meets more precisely the quality of our results.

In my view, all of these topics overload the paper, as the paper goes off too many tangents, and in fact blurs the main message(s) of the paper.

(and:)

The paper appears too "crowded", as a number of aspects are discussed: the state of the AMOC and PMOC during the LGM, global circulation changes during the last deglaciation, the global carbon cycle and a model-data comparison. These topics in fact merit their own studies, so I can just recommend once more to streamline the paper and remove redundancy.

–>In our view a 'synthesis paper' on a new, purely 'technical' approach needs to display the wealth of published and unpublished aspects and tangents that result from the technique, an effort that necessarily includes elements of previous publications but does not rank under redundancy. We consider it as genuine trait of a 'synthesis' to include both some recaps of important results already published and various tests of the value of the plateau tuning technique to produce global age markers. A synthesis needs to show major paleoclimatic implications that make the dry report on a new dating technique attractive to readers of CP. Inversely, we followed the suggestion and deleted the subchapter on Zoophycos burrows. Also, we carved out a clearer separation of the RESULTS from the DISCUSSION AND IMPLICATIONS section.

, , , the paper should re-focus on the advantages and disadvantages of the 14C plateau tuning technique

(and:)

I however miss a more nuanced discussion of potential disadvantages of the technique and the underlying assumptions in places. For instance, when is the technique best applied? What are the underlying assumptions, and are there uncertainties associated with these assumptions? What resolution of the tuning record is required?

–>We follow this helpful suggestion and broaden our Discussion section by a one-page long recap of our previous publications in the new Subsection 2.1 named “Suite of planktic 14C plateaus: Means to separate global atmospheric from local oceanographic forcings ". In this way we address potential weak points of the 14C plateau tuning method. We explicitly reject the referee’s term “disadvantages”. Subsection 2.1 also recaps some technical needs (e.g., minimum sedimentation rates of 10 cm/ky of hemipelagic deposits, minimum 14C dating resolution of ∼100-150 yr, as published in Sarnthein et al., 2007).

This would mean to significantly shorten or remove the bioturbation section and/or repetitions of previous published work e.g. on the carbon cycle. –>The bioturbation section has now been deleted. Other recaps are maintained, as necessary parts of this synthesis paper.

The paper appears too “crowded”, as a number of aspects are discussed: , , , , so I can just recommend once more to streamline the paper and remove redundancy.

–>Our philosophy on the traits of a ’synthesis paper’ has been outlined above.

, , , so it should be more clearly highlighted what are the new findings

–>Thanks for a good suggestion. These are ’new findings’ since our last synthesis paper 2015: âĂć Switch to U/Th-based model ages for 14C plateaus boundaries in 18/20 sediment cores, âĂć Inclusion of 14C records from nine new sediment cores into our synthesis, âĂć Global data-model intercomparison for LGM and Heinrich-1 surface waters, âĂć Generation of global deep-water transects.

–>The listing is now incorporated in the text at the end of the Introduction and in Section 3.1.

–> Response to ’major points’ and specific questions (page C3-C4-C5)

(1) Comparison with latest compilation of surface ocean reservoir age variations of Skinner et al. (2019) and Stern and Lisiecki (2013). The authors have synthesized surface ocean reservoir age records based on the 14C plateau tuning technique that are interpreted for potential driving mechanisms and implications regarding changes in atmospheric CO2. However, it is not clear why other reservoir age estimates have been neglected for instance those based on paired tephra-foraminifera 14C analyses (Skinner et al., 2015; Sikes and Guilderson, 2016) or those resulting from stratigraphic tie points (e.g., Waelbroeck et al., 2001). The fact that these estimates are low in resolution, should not diminish their veracity. I strongly recommend that plateau-tuned surface ocean reservoir age estimates are compared with results from other techniques, in particular Skinner et al. (2019).

–>Detailed comparisons to other methods for tie points to reconstruct past variations in surface ocean reservoir age were given in Subsection 1.2 of the Introduction, in particular in "paragraph 2", that we do not like to repeat in the discussion. Now, however, no problem to enrich the paragraph by additional remarks and references to authors listed by Ref.#2.

–>âĂć To be fair, the paper of Skinner et al. (2019) appeared only two weeks after our manuscript submission. Of course, we appreciate to read that these authors – like ourselves – now fully recognize the concept of strongly variable spatiotemporal patterns of surface water reservoir age. We feel happy to include into our discussion the results of this single-core study, that also includes detailed (though in part somewhat debatable) alignment of paleoclimate records.

–>âĂć We agree, each low-resolution age tie point per se has full veracity. Being spaced over 5-10 ky, however, these tie points cannot depict the actual dramatic short-term variability of res. ages now revealed for glacial-to-deglacial records by means of 14C plateau tuning, hence won't provide the accurate timing of paleoclimatic events needed for proper age correlation.

–>âĂć Stern and Lisiecki (2013) indeed had been cited in "§2", but unfortunately deleted in a "streamlining action" prior to our manuscript submission. Based on pers. comm. with L. Lisiecki (Cambridge, 9-2018), however, we (M.S.) learned that the uncertainty range of their age assignments for the LGM amounts to $\pm$1.5Âň–$\pm$2.0 kyr, a range problematic for any proper correlation of multi-centennial-scale events in paleoceanography.

–>âĂć Valuable age tie points and reservoir ages were derived from 14C ages of planktic foraminifera paired with 14C dated tephra layers as cited in "§2" of Subsection 1.2. Most of these tie points, however, are far too wide-spaced in peak glacial-to-early deglacial sediment records to meet the need to specify millennial-scale LGM-to-deglacial changes in ocean circulation, which may involve wrong age correlations. A rare deglacial suite of four tephra-based reservoir ages off Chile (Siani et al., 2013) was clearly reproduced, its variability, however, was much refined by 14C plateau-based reservoir ages (Küssner et al. (subm.). Accordingly, we now broaden our discussion of tephra-based ages in "§2". – Tephra-based results of Waelbroeck et al., 2001 and 2011 had been properly cited in our text.

2) Drivers of 14C plateaus: The causes of 14C plateaus are seemingly not well understand. The plateau tuning technique assumes that oceanic and atmospheric 14C records occur simultaneously, with identical duration and without any temporal offsets, and can unequivocally be identified in the often low(er)-resolution ocean records (see lines 216-217, or line 223). Do all of these conditions always apply?

(and:)

The authors outline that "air-sea gas exchange transfers the atmospheric 14C fluctuations into the surface ocean" (line 178-179), but it remains unclear how ocean degassing (of 14C-depleted CO_2), sea ice and/or wind changes might have affected this one-to-one assumption.

–>We now added the new Subsection 2.1 on the "Suite of planktic 14C plateaus: Means to separate global atmospheric etc." to avoid this misunderstanding: Indeed, only plateaus during which local oceanograpy did not change significantly will correlate and without much delay or change in duration in view of the rapid atmosphere-surface ocean exchange. Thus, the possibility of local ocean effects necessitates the use of a complete suite of 14C plateaus (and their estimates of reservoir age) to achieve a best-possible tuning of each sediment core. In this case one or two disturbed plateaus (out of up to 19) won't hinder valuable results for the others and, at the same time, provide evidence of local short-term oceanographic changes. Finally, our records show that reservoir ages generally vary with climate changes on time scales of 1000 and more years in contrast to generally much shorter atmospheric plateaus.

In my view, this poses serious challenges to the 14C plateau tuning technique

–>These changes are at least as challenging for IntCal where a close correspondence between 14C levels in the surface ocean and in the atmosphere is assumed to justify the use of ocean carbonate records for the reconstruction of atmospheric 14C. A comparison of IntCal13, IntCal20, and MarineCal20 for the time period older than 14 cal. ka shows how much work still needs to be done and how plateau tuning can contribute valuable data. In contrast to Marine Cal20 and IntCal where the timescale of the ocean record is largely based on climate wiggle matching beyond 14 cal ka our plateau tuning is based on atmospheric wiggle matching.

„„ These potential caveats are not fully discussed.

–>To meet this objection we now added the new Subsection 2.1.

recommend to address potential disadvantages , , , ,

(and:)

the community has not really embraced this technique yet.

–>The tuning technique does not have "disadvantages", rather potential weak points that need consideration. At least the technique is better than most other ways to produce R data with high spatial and centennial-to-millennial-scale resolution.

It might also be beneficial to tone down some of the overselling language.

–>We agree, it never is good to oversell. E.g., we may replace "far" by "a bit" superior.

, , , the method works "wherever [sediment is] retrieved in the global ocean"

–>Indeed, we need to specify and say: "wherever cores with high hemipelagic sedimentation rates are sampled"

Furthermore, the authors seem to make clear that the assumption "[. . .] these plateau/jump structures are real and widely reproducible in marine sediment records" (lines 245-246) remains a speculation.

–>The objection that our assumption remains a speculation is simply not true. Our assumption is, as stated, general and common with those of IntCal. Each new sediment record that, after high-resolution dating, provides a suite of jumps and plateaus that can be correlated with the atmospheric master curve and our planktic records, confirms the practical applicability of the assumption. These findings are no "speculation" but apply

to 14C records of by now 20 (plus one more recent, yet unpublished) sediment cores from the global ocean.

–>Yet, we agree, "one always needs to be cautious". Therefore, we insert a "most likely" because signs of local disturbing influences must always be looked for.

Line 395-398 and line 477-483: The authors compare the timing of atmospheric 14C plateaus with major changes in the atmospheric CO2 record in order to emphasize the impact of ocean outgassing on the atmospheric CO2 record. Their arguments based on this comparison is inherently weak, as a number of 14C plateaus are not associated with a major shift in atmospheric CO2. This should be acknowledged and discussed in more detail.

–>In (former) L. 501-507 we had already acknowledged the weakness of our understanding of several causal links that might have controlled the origin of atmospheric plateaus and jumps. We broached pulsed ocean outgassing as one amongst various other forcings and now terminated (present) Subsection 2.3 with the following sentence (now L. 531): «However, there is still little information on the origin of several other peak glacial 14C plateaus 17.5–29 cal. ka. The actual linkages of these plateaus to events in ocean MOC still remain to be uncovered.»

What causes a temporal agreement between the two, why would the same process not operate at other times of 14C plateaus? What does this tell about the mechanisms driving atmospheric 14C plateaus, and in particular the assumed synchronicity between atmospheric and oceanic 14C changes?

–>Reformulated Subsection 2.3 tries to make clear that due to the many factors involved it is often not possible to pinpoint a single dominant forcing of an atmospheric 14C plateau. In some plateaus we can recognize the role of 14C production (via 10Be), while three others coincide with changes in CO2 level shown by ice cores that indicate ocean outgassing and oceanic changes. The difference in length between the plateaus and the outgassing spikes suggests that the spikes were part of longer lasting oceanic processes. Such processes may also be the origin of other plateaus without a recognizable CO2 signal. We here demonstrate that in some cases the origin of atmospheric 14C changes can be identified. Increased use of plateau tuning may provide data that will allow identification of further determining factors.

–>Also, the fairly fast air-sea transfer of atmospheric 14C signals that leads to a quasi-synchronicity was displayed at (former) L. 102-110.

3) Data in preparation by Küssner et al. and Ausin et al. or pers. communication 2018 (Line 263): The authors have compiled all existing 14C datasets obtained via the plateau tuning technique (mostly by the group involving the first author), among which are also two new datasets (Küssner et al. and Ausin et al.). I find it hard to follow the findings obtained from these datasets, as crucial metadata is lacking for these cores.

–>Küssner and Ausin are coauthors of this manuscript. In the meantime, the manuscript of Küssner et al. that we regard as important brick of this synthesis, has now been submitted to P&P. Also, the datasets of Küssner et al. are stored at PANGAEA.de, saved by a password until acceptance of this manuscript for publication.

–>For the 14C record of Ausin et al., we may refer to their records and results as "unpubl. data". Unfortunately, the manuscript of Ausin et al., perhaps particularly interesting to Ref.#2, will be ready for submission only later this spring.

4) Revision of the Suigetsu age scale: I consider this an important contribution of the paper to the community but I do not find Fig. 5 very informative. What role do siderite layers play (they are not mentioned in the main text)? And how does the figure show age uncertainties, as indicated in lines 352-254. Further elaboration is needed here.

–> Our text is supplemented in Section 2.2 and the caption of Fig. 5.

Also in lines 385-388, what are the age uncertainties of the Mono Lake and Laschamp paleomagnetic excursions? They should be considered when assessing and comparing different age scales.

–>We now refer to data of Lascu et al. (2016).

Why was plateau 2b chosen as test case? How successful is the comparison for any other of 10+ 14C plateaus?

–>Unerring question! In contrast to optical varve counts reaching back to >40 cal. ka, the base of 14C Plateau 2b marks the oldest tie point captured by XRF-based varve counts. Marshall et al. (2012) showed that the difference between optical and XRF-based varve numbers is modest back to ∼15 varve ka. Before, it has risen dramatically back to 17 ka.

I am surprised to see in Table 3 that some plateaus are combined to one long plateau, e.g. 6-7-8. What is the basis for that?

–>All right, we need to mention: Table 3 is grouping the 14C plateaus of some climate units that can be reproduced by the time slots employed for the model-based res. ages of Muglia et al. Within these slots, the plateau-based res. ages are largely constant.

5) Zoophycos burrows (lines 562-576): This whole section is somewhat dubious and not very clearly written. I wonder how useful it is for the review of the 14C plateau tuning technique. The authors seem to suggest that 14C plateaus in host sediments can flag 14C outliers as such, but it is not clear how initial 14C measurements a priori exclude bioturbated material for dating. This should be explained in detail. It is entirely unclear how Zoophyros burrows “help to corroborated changes in MOC and climate”. In general, I think that the paper goes off another tangent here. The authors could consider removing this section in my view.

–>The section on Zoophycos burrows has now been removed. Küssner et al., 2018, already gave all details on the principles how to separate foram tests of Z. burrows from those of the ambient host sediment.

6) Model comparison with Muglia et al. (2018): The authors compare their datasets with the model output of Muglia et al. (2018). Given that other modelling studies exist

that studied past ocean reservoir ages variations (e.g., Franke et al., 2008; Butzin et al., 2017), it is unclear why this particular study has been chosen. What characterizes the model run of Muglia et al. (2018), and in particular the modeled AMOC?

–>The choice of model data of Muglia et al. was linked to (1) the simple availability of pertinent recent model data and explanations, kindly provided by Juan Muglia as coauthor. (2) Butzin informed me, when testing his data as alternative dataset and preparing this manuscript in spring 2019, that his dataset (then not particularly successful by comparison with our dataset) was outdated and that his group was preparing a major improvement of their model concept. (3) With regard to res. ages modeled by Franke et al. (2008), we were discouraged by major age differences found both for high-latitude and upwelling regions. – In summary, we now introduce three different modelling studies at display and their availability for our test (L.589-598).

How were the simulations forced and what were the LGM boundary conditions? A comparison between the data and model results would be better facilitated by global plots of surface ocean reservoir ages.

–>To avoid extending the manuscript up to 'textbook' length, we now just add a brief remark on the background of the model simulations.

Is the distribution realistic? It is impossible for the reader to follow statements such as "with estimates of 13 Sv appearing somewhat more consistent with our results." (line 714-715) without any further elaboration or figures.

–>We now give a pertinent short explanation.

Is the time interval used for comparison the same between 14C data and model data? How was the LGM 14C data obtained? Were several plateaus averaged?

–>Time intervals and average of several plateaus are given in Table 3, as listed in Fig. 8 caption. Both model-based and observed reservoir ages focus on the Late LGM, 21-18.7 cal. ka (plateaus no. 4-5).

Response to 'minor comments'

The following long and/or complicated sentences are hard to follow and should be revised: lines 135-144, line 228-232, lines 302-307, line 357-361, line 461-466, and lines 646-649.

–>We now tried to simplify the sentences demurred by the reviewer.

Line 110-113: should cite paper(s) on coral reservoir ages here. –>Without expanding our manuscript up to 'textbook' length we now add a few refs in addition to Adkins & Boyle, 1997.

Line 116: "that finally turned out to be the most valuable tracer of oceanography" This is in the eye of the beholder, and should be rephrased to "became a valuable tracer for xxx/tool in oceanography" –>As authors we may regard it legitimate to present these findings in our eye as beholders.

Line 118: "benthic carbonate particles" should be "benthic foraminifera", also "reflect" might be a better word for "sum" here. –> o.k.

Line 121: Remove "the 14C level of" –> o.k.

Line 135: "provided" instead of "given" –> o.k.

Line 143-144: unclear what iv refers to.

–>Item 'iv' is now reworded to a separate sentence that spells out the great need for a generally accepted high-precision atmospheric reference record , , ,

Line 173: jumps –> o.k.

Line 286: remove "developed a special computer program" –> o.k.(paragraph was deleted)

Line 442: remove "ones of" –> o.k.

Line 450: consider "propagated error of calendar age uncertainty of a plateau boundary and the uncertainty in its determination”

–>We agree but do not follow the idea to include the fishy statement "and the uncertainty in its determination".

Line 484: Use (Skinner et al., 2010; Burke and Robinson, 2012) as Southern Ocean reference. –> o.k.

Line 509: “assess” instead of “rate”

–>We prefer "rate".

Line 639-641: This statement is unclear. Please specify. Do you mean the influence from eddies, AABW formation or the interference of bathymetry with ocean currents? -

–>o.k.: We mean a variability linked to small-scale frontal systems, upwelling cells, and the interference of ocean currents with small-scale bathymetry.

Line 649-640: Rephrase. “test” instead of “weigh more correctly”? –> o.k.

Fig. 1. It is unclear what the 1:1 line means

–> The 1:1 line shows a gradient of one 14C yr per cal. yr

Fig. 6. (c) in figure should probably be (b) –> o.k., thank you!

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