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Climate of the Past Discussions

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.

Anonymous Referee #2

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Sarnthein et al. provide a review of the plateau tuning technique that hinges on an alignment of atmospheric and oceanic radiocarbon records. The method has not been extensively used outside the first author's group, which might be associated with some sceptisism towards this method. 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. Assuming the robustness of the method, the authors suggest that it provides "precise" chronostratigraphic control for marine sediment cores (e.g., in the LGM

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when other methods are weak), and the opportunity to obtain surface and deep-ocean reservoir ages at high resolution. Additionally, the study covers important aspects such as chronological uncertainties, calendar age controls, bioturbation, ocean ventilation ages, seasonality or habtiat shifts of foraminifera, and the global carbon cycle. 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. For the sake of its clarity and impact, the paper should re-focus on the advantages and disadvantages of the 14C plateau tuning technique, and resulting surface ocean reservoir ages (with potential comparison to models and existing databases, e.g. Skinner et al. (2017) or Zhao et al., (2018)). 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 paper presents important insights into aspects mentioned above, and emphasizes the benefits and advantages of the 14C plateau tuning technique. 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?

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. Some of the findings are presented as “new features of the MOC and carbon cycle” but are in fact based on previous work by the authors (Sarnthein et al., 2013, 2015; Balmer et al., 2016; Balmer and Sarnthein, 2017) and many other studies cited by the authors, so it should be more clearly highlighted what are the new findings. Furthermore, some of the sections appear as a recap of previous papers, e.g., section 3.5.2 (Sarnthein et al., 2015) and section 3.5.3 (Sarnthein et al., 2013), and do not seem to provide new insights. They too might be considered to be

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shortened or removed for the sake of clarity.

Below I list major and minor points of criticism that I think should be considered in a revision of the study. I hope that the authors find these comments useful.

Major points: 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 tiepoints (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).

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?

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 CO2), sea ice and/or wind changes might have affected this one-to-one assumption. In my view, this poses serious challenges to the 14C plateau tuning technique (essentially because these have the potential to cause a phase-shift in atmospheric and oceanic 14C evolution, or skew one independent of the other). These potential caveats are not fully discussed. In light of these issues, I would recommend to address these potential disadvantages of the technique, in particular because

the community has not really embraced this technique yet. It might also be beneficial to tone down some of the overselling language used to describe the technique (e.g., “provides far superior [. . .] evidence” line 218, “sweeping loss” line 266, “clearly, [. . .] a tool indispensable to uncover functional chains in paleoceanography” line 555-556 or the method works “wherever [sediment is] retrieved in the global ocean” line 583). Here I would urge the authors to be more nuanced when addressing the value of the 14C plateau tuning method.

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. See for instance: “may most likely be regarded as suite of ‘real’ structures” (line 253). In my view, variations in ocean reservoir ages should be discussed in the light of uncertainties associated with these assumptions, in particular beyond the tree ring chronology.

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. What causes an 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?

3) Data in preparation by Küssner et al. and Ausin et al. or personal 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. It is simply not enough to refer to two in prep. papers, if results are used in the present

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manuscript. Full metadata are needed: location, study area, methodology, raw planktic and benthic 14C data and age model strategies/tuning. Given the lack of these pieces of information, statements as in lines 522-529 and in lines 723-733 are difficult to follow, and should be either entirely shifted to the Küssner and Ausin papers in preparation or explained in more detail (with figures and metadata) in the present study.

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

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.

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? How were

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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. 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. 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?

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.

Minor comments: Line 110-113: should cite paper(s) on coral reservoir ages here.

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”

Line 118: “benthic carbonate particles” should be “benthic foraminifera”, also "reflect" might be a better word for "sum" here.

Line 121: Remove “the 14C level of”

Line 135: “provided” instead of “given”

Line 143-144: unclear what iv refers to.

Line 173: jumps

Line 286: remove “developed a special computer program”

Line 442: remove “ones of”

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

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

Line 509: “assess” instead of “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?

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

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

Fig. 6. (c) in figure should probably be (b)

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