Dr. Michael Sarnthein, Prof. emer.
Institut für Geowissenschaften
University of Kiel
Olshausenstr. 40
24098 K i e l, Germanys
<michael.sarnthein@ifg.uni-kiel.de>

26 April 2022

COMMENTS to Review #2

by Michael Sarnthein and Pieter M. Grootes (authors)

In this manuscript, the authors seek to establish the existence and timing of 'plateaus' in the atmospheric radiocarbon record, and to demonstrate that these are also present in marine records from around the world. On this basis, the authors seek to argue that radiocarbon plateaus identified in marine records can be stratigraphically aligned to correlative plateaus identified in the atmospheric record, allowing calendar ages to be transferred to the marine records (and therefore allowing for 'marine reservoir age' offsets to be determined). This method of chronostratigraphic alignment has been termed 'plateau tuning' (PT).

This paragraph nicely describes PT. This was the topic of Sarnthein et al., 2020, and of the objections of Bard and Heaton, 2021.

This manuscript is quite unusual, as it does not appear to advance any new observations/data, arguments, models or insights. Some adjustments are made (again) to the proposed timing of plateaus identified in the atmospheric radiocarbon record, but this does not really make any difference to what has been proposed by the authors in several papers since 2007.

#1. - Indeed the present manuscript does not provide new primary data. Different from the view of Rev.#2, however, our manuscript presents three major lines of "new evidence" that deserve publication, **(1)** a novel confirmation of the authenticity of atmospheric ^{14}C plateau structures by means of a Bayesian spline plot in $\Delta^{14}C$/age space (courtesy of Bard & Heaton, 2021 (B&H), that now are clearly reproducing the structures of the Suigetsu atmospheric ^{14}C record independently identified by our previous approaches, especially when all techniques use the updated Bronk Ramsey et al-2020 Suigetsu time scale. **(2)** We provide an adaption of the absolute age of all plateau boundaries in our marine ^{14}C records, now solely based on a revised age control only published by Bronk Ramsey et al. (2020), that was coeval with the publication our CP synthesis article. The age revision has now been applied to all 19 ocean sediment records. This is crucial for the validity of PT, that is, for any proper use of plateau boundaries as global age tie points. (3) With great, yet unpublished detail our manuscript is meeting the unfounded allegation, also based on a misunderstanding of the importance of a suite of plateaus (pointed out in our comments but ignored), that sediment distortions by differential bioturbational mixing may form a major source of "fake" ^{14}C plateaus.

A recent 'review' of the 'PT method' and its results was published by the authors just last year in this same journal. Primarily, it seems, the manuscript seeks to publish a rebuttal of a prior piece of work produced by Bard & Heaton (B&H) that was also reviewed and published in Climate of the Past last year. The latter was also accompanied by several pages of commentary by Sarnthein and Grootes, which was in turn responded to by Bard and Heaton over the course of the discussion phase of the manuscript.

#2.- Since many arguments in our commentary were simply ignored by B&H (2021) a partial rebuttal of B&H theses was unavoidable in the present manuscript.

Unfortunately, I find it impossible to recommend that this manuscript be accepted for publication. There are three main reasons for this: 1) it does not appear to present an original piece of research, and insofar as it presents adjustments, they are not important enough for publication on their own merit;

#3.- As said before, our manuscript shows that the centennial atmospheric ^{14}C structures obtained using three different techniques are largely the same. The primary data may not be new but the outlined agreement, not pointed out by B&H, is telling a new perspective.

2) its arguments against B&H are not coherent (regardless of whether or to B&H are correct);

#4.- The paper does not want to argue - again- against B&H. It just aims to show that the Suigetsu atmospheric ^{14}C data set, though noisy and with limited coverage, can provide an authentic centennial-resolution signal of global significance extending beyond 14 ka, the present range of continuous tree-ring data. This data set can thus provide a valuable correlation target for the interpretation and global correlation of ocean sediment records.

3) the vast bulk of figures and tables referred to in the manuscript are included in a 'supplement' that has not actually been produced/included. On the latter point, the promise of a compilation of all the available PT data in useful tables would have been at least one welcome contribution: but it turns out that the intention of the authors was to include ~20 disparate data tables that are already available on PANGAEA and that are not at all useful in reproducing the PT data that have been published to date by the authors (it took me days to do this, and the results are not the same as what the authors have published in many cases, which is both worrying and annoying).

#5.- All data tables necessarily contain the same primary depth and ^{14}C information. However, a new Suigetsu time scale means new imported data and, potentially, a new correlation of plateaus. Those will be different and provided an upgrade replacing the earlier ones. Our recent compilation of ~20 data tables in a supplement has also been stored at PANGAEA under "https://doi.org/10.1594/PANGAEA.940604". Once this manuscript may be accepted, they are given with the explicit intention to replace (though properly cite) age tables previously published, somewhat diverging data tables that are obsolete after revision of the reference age scale and three minor revisions of plateau definition. All tables represent the same scheme of presentation. In addition, the tables of course need to take care of some local specialties of a sediment site recovered, such as listing paired benthic ^{14}C ages in case available. Otherwise, we see no disparity. Minor differences in the sequence of ^{14}C and sediment properties listed have now been adjusted.

The fact that the PT data (and associated MRA etc.) that have been produced by the authors over several years, and presented in a series of 'global synopsis' papers, cannot be easily reproduced by others using the multitude of available data tables, is particularly worrisome.

We do not see a problem of a multitude of data tables, since the present set of tables will be clearly marked at PANGAEA as latest version 2022 and/or "latest state of the art" tables.

The same can be said for the fact that only one (?) PT study exists that does not include the authors of this study (the champions of the PT approach). Incidentally, this might already answer the question of whether or not it is a 'trend setting' tool.

#6.- Thanks for the kind remark. We feel worried by the traditional hesitation to accept a new higher ^{14}C variability, combined with the strong warning written by B&H, that has discouraged use of PT in oceanography. Though it is certainly good to question the sometimes controversial new results and demand substantiation, the statement by B&H that PT should be verified by independent research (which is correct) is counteracted by their listing of objections that are partly based on misconceptions.

With regard to the second point raised above, the authors state that they reject the arguments of B&H based on the basis of how plateaus are identified (i.e. as 'sequences', like a sort of Morse code), and on the basis that B&H use a 1998 box model to support their arguments. Regardless of the validity of B&H's remarks, I don't see how either of these points represent a coherent basis on which to reject a criticism of the PT method, where that criticism is founded in large part on the proposed difficulty of objectively identifying plateaus (let alone sequences of plateaus) in a noisy marine radiocarbon record whose offset from atmospheric radiocarbon varies over time, as well as the proposal that sedimentary processes (such as simple - and highly likely - sedimentation rate changes during periods such as Heinrich Stadial 1, or the Younger Dryas) can also produce 'plateaus' in the 14C age-depth domain, without these being causally linked to atmospheric radiocarbon variability.

#7.- The difficulty of unambiguously identifying and correlating plateaus is real but also not new. When the GISP2 and GRIP ice cores in Greenland provided in their ^{18}O records highly detailed evidence for large and rapid climate variability paleoceanography followed with ^{18}O signals in plankton in higher-resolution ocean cores, the number of peaks and valleys in both ice and ocean records was high and sediment dating not very detailed. Moreover, the ice core record has low accumulation and different thinning in cold phases, while sediments often have higher accumulation in cold phases and are subject to sedimentation and bioturbation problems as detailed by B&H. Yet, the patterns of D/O 14-13, 12-9, and 8-5 (Bond cycles) could be identified in various expressions in ocean sediment cores and provided a valuable link for improved dating and ocean-atmosphere correlation.

The situation for PT is more difficult, because the atmospheric and oceanographic ^{14}C signals are less clear than the ^{18}O signals. Yet, the principle of correlating a full **suite** of ^{14}C fluctuations/plateaus is similar and makes it possible to correlate the 'good' plateaus of a plateau sequence when one or more were destroyed/falsified by the mechanisms discussed by B&H. Again, PT is a 'tool' to explore whether more environmental information can be obtained from a sediment record. However, it does not provide a simple cookbook but rather shows a direction of additional analyses and comparisons needed to substantiate an initial plateau tuning with a consistent picture of the local oceanography and global climate recorded in the sediment core. If this succeeds valuable information has been gained.

In addition, the claim that a 1998 box-model is somehow incorrect because of its vintage seems to miss the point: the key purpose of deploying such a model is surely to illustrate in a very simple way how the phasing and amplitude-attenuation of an input signal will be altered (filtered) in the ocean, depending on the timescale on which the signal can be communicated to the ocean, and the frequency/duration of the signal variability. You can do this with a very complex biogeochemical coupled ocean-atmosphere numerical model if you like, but if it did not show a simple phase-attenuation relationship like the box model, it would mean that the complex model had a problem! In fact, by playing around with numerical model outputs it can be shown that they do show the same principles as a 2 box-model, and that should not be surprising, as it is an expression of a simple and fundamental physical principle: parts of the ocean that have small MRA offsets (such as the tropical ocean, MRA ~400 14Cyears) can respond quickly and can pick up shorter fluctuations from the atmosphere, whereas parts of the ocean that have large MRA offsets will take longer to pick up the atmospheric signal (since a larger MRA means that the isotopic exchange timescale for that water is longer) and will pick up a smoothed and lagged response. The limits of applicability of the PT method could readily be analysed and qualified in such a theoretical context, but the authors don't do this unfortunately.

#8.- On the basis of a coupled ocean GCM Lohmann et al. (2020) clearly show that ^{14}C reservoir ages vary over small scale ocean regions, different from the assumptions of a simple box model simulation.

Ultimately, the manuscript sets out to answer the question posed in the title: "is the 'plateau tuning' (PT) approach a misleading approach or a trend-setting tool"? I would note that, at worst, PT could be both misleading and trend-setting, and my major concern is that the authors clearly wish for it to be the latter, but have not really (either in the present manuscript, or over the course of several publications that appear to present the same datasets repeatedly) demonstrated that the PT approach is indeed viable, either in theory or in practice.

#9.- For a first time the present manuscript is documenting the authenticity of plateau structures, i.e., a major basis in support of the PT method. Admittedly, trend-setting' is may be a bit optimistic although new methods to analyze complex data often become quite trendy. 'Misleading' should not be possible if the researchers using PT 'do their homework' and carefully collect all circumstantial evidence they can to falsify interpretations till they are left with one that is verified by all available data.

As suggested above, this is not to say that some sort of defence cannot be made, in theory at least. But the authors (still) have not managed to do this. My own view is that the chronostratigraphic principles that the authors wish to apply are not completely crazy: yes, the atmospheric radiocarbon record has 'wiggles' and these would be transferred to other reservoirs that exchange CO2 with the atmosphere rapidly enough to pick them up. However, the conditions under which these wiggles can be recorded in other reservoirs, such as the ocean, and the biases (in amplitude and phasing especially) that will inevitably and predictably arise (even prior to the complications of sedimentation changes, bioturbation, sampling/analytical noise etc.) need to be accepted and addressed by the authors at some point if this debate is to move in a useful direction.

#10.- Complications of regional ocean circulation changes like local upwelling, sediments rate changes, bioturbation, minimum sampling density and analytical noise have extensively been discussed and minimum qualities defined in our 2020 synthesis (and various papers since 2007), admittedly labor-intensive to read. A renewed lengthy repetition of all this reasoning appears unjustified and can now been avoided by citation of the synthesis paper.
If there is a discrepancy between observations and theoretical predictions it can be that the interpretation of the observations is wrong (implied here). It is also possible that the system knowledge formalized in the model was still incomplete and that the level of detail used the model used, that was sufficient to answer research questions at the time it was developed, no longer can address the present problems. Considering the enormous gains in oceanographic knowledge over the

past decades and the work of e.g. Lohmann et al (2020), our guess is that a reevaluation of the theoretical restrictions and biases posed by local oceanography on local signals may be very valuable to move the debate.

#11.- Continuing our PT research, we now plan to add a statistical "BINNED correlation coefficient" to test the quality of correlation between the atmospheric reference record and each ^{14}C record derived from ocean sediments as listed in supplement Tables S1 - S20.

I can think of a variety of ways to test the PT method in theory (using models), and in practice using data, and I wonder why the authors have never done something similar.

#12.- By now we have been not able to generate a model to test the PT method in theory, since the variation of ^{14}C reservoir ages follows a broad and highly complex multitude of factors of ocean circulation and carbon exchange, possibly a target for a future follow-up project of the group like that of Lohmann et al. (2020). Also, the reviewer ignores 15+ years of continued application and testing of PT in practice, which also served to gradually convince ourselves of its use. The testing in theory, using models, is easier said than done. First highly specialized modeling skills are needed and secondly detailed oceanographic insights are needed to improve models to the state that they can usefully provide local information over time interacting with climate. The authors would have loved to do this, but it is far beyond their reach.

If a scientific study that achieved such goals was produced, it would be a welcome and useful addition to the literature (as B&H has proven to be, insofar as it stimulates critical thinking). Such a study would best come from the authors of the present study, who appear to be the main (if not the only?) champions of the PT method; however, this is not what the current manuscript provides.

For all arguments listed above, we like to plea that the present manuscript may be given a fair chance of publication in CP, certainly after a number of minor and major additions and revisions of the manuscript.

REFERENCES

Bard, E. and Heaton, T.J.: On the tuning of plateaus in atmospheric and oceanic 14C records to derive calendar chronologies of deep-sea cores and records of 14C marine reservoir age changes. Climate of the Past, 17, 1701–1725. https://doi.org/10.5194/cp-17-1701-2021, 2021.

Bronk Ramsey, C., Heaton, T.J., Schlolaut, G., Staff, R.A., Bryant, C.L., Lamb, H.F., Marshall, M.H., Nakagawa, T.: Reanalysis of the atmospheric radiocarbon calibration record from Lake Suigetsu, Japan. Radiocarbon, 62, 989–999, https://doi.org/10.1017/RDC.2020.18, 2020.

Lohmann, G., Butzin, M., Eissner, N., Shi, X., Stepanek, C.: Abrupt climate and weather changes across time scales. Paleoceanography and Paleoclimatology, 35, e2019PA003782, https://doi.org/10.1029/2019PA003782, 2020.

Sarnthein, M., Küssner, K., Grootes, P.M., Ausin, B., Eglinton, T., Muglia, J., Muscheler, R.,Schlolaut, G. Plateaus and jumps in the atmospheric radiocarbon record – Potential origin and value as global age markers for glacial-to-deglacial paleoceanography, a synthesis. Climate of the Past, 16, 2547–2571, doi: 10.5194/cp-16-2547-2020, 2020.

Sarnthein, Michael; Grootes, Pieter Meiert.: World Ocean ^{14}C plateau tuning. PANGAEA, "https://doi.org/10.1594/PANGAEA.940604", 2022.

,,