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Interactive comment on "High resolution ^{14}C bomb-peak dating and climate response analyses of subseasonal stable isotope signals in wood of the African baobab – A case study from Oman" *by* Franziska Slotta et al.

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Associate Editor, Dr Aninda Mazumdar, asked me to act as a Referee of this article, although I had already placed a short comment earlier. I am happy to be identified as a reviewer and have my comments passed on to the authors. Therefore, I am expending my previous short comment here. I hope that these further comments and suggestions will help the authors to improve their work.

General comments

The paper presents annual 14C data from an African baobab (Adansonia digitata) tree from Oman, for the interval AD 1941 to 2005. This work is important in that it provides a fairly detailed pre/post-bomb 14C time-series for a region that has not yet being part of the atmospheric 14C global compilation. This is actually one of the main goals of the manuscript. The authors have also improved the quality of the data set by providing intra-annual analyses of δ13C and δ18O, as well as F14C for the calendar years of 1962 and 1963.

While the high number of consecutive single tree-rings measured by radiocarbon allowed confirming the annual nature of the baobab species, a significant mismatch between the baobab F14C values and the post-bomb atmospheric curve NH3 was detected. This mismatch prompted an alternative explanation, i.e. mixed pool of slow-turnover non-structural carbon (NSC) into the structural ring cellulose fraction - a strong functional trait of parenchyma-rich tree species (maybe ?!).

The Baobab terminal parenchyma bands F14C values presented here, definitely demonstrate that a large percent of the parenchyma in this tree species is relatively young, and as such, it provides valuable perspectives in the field of plant physiology. On the other hand, mixed carbon pools in putative structural ring cellulose fraction (in this case, slow-turnover NSC residue in holocellulose extracts) put into question the use of tree rings of this group of woody plant when reconstructing atmospheric 14C.

I appreciate that in view of the perplexing results of the 14C data of the baobab tree rings, an alternative explanation should be considered. However, for the mixed pool NSC-ring cellulose assumption works, all other possible bias must be carefully ruled out. Robust methodologies must be properly done and explained in detail, as well as the use of reference materials/internal standard, or equivalent (i.e. interlaboratory measurements), and the use of further chemical extractions. All of those are missing here.

Given the absence of an independent benchmark, e.g. a short F14C sequence of consecutive single tree-rings from a non-parenchyma-rich woody plant in Oman, I cannot tell whether slow-turnover NSC detected in holocellulose extracts of baobab tree rings is a feasible explanation for the 14C offset observed here or not. For starters, 14C analysis of incomplete single tree rings (material that do not represent a full growing season) could contribute in 14C offsets (see specific comments/suggestions). Furthermore, we had to keep in view that other factors must also play some role in those 14C offsets (atmospheric circulation and carbon dioxide from human activities, for example). Previous records across zones NH3, SH3 and SH1-2 are very scarce. Therefore, the possibility of multiple sources of air-14CO2 influencing Oman should be discussed. One cannot ignore the fact that during the assembly of the atmospheric post-AD 1950 14C global compilation by Hua et al. (2013) some datasets were disregarded due to its mismatches with other regional datasets. Therefore, a thorough evaluation of possible external effects should also be offered.

Finally, procedures described here need further explanations and details. The result and discussion part is quite jumpy and very tricky to follow. It does not quite convey the ideas of the underlying assumption offered to explain the baobab tree F14C offsets. I recommend a complete re-organization of the manuscript, by focusing on placing the absolutely necessary data, figures and tables (for the purpose of the paper) in the main text. The stable isotope findings were not particularly striking. Although important, they are currently creating a lot of distraction. I strongly suggest moving them (most of its description, associated material and discussions) to a supplementary text or appendix.

Specific Comments/Suggestions

I am going to focus here on just major topics that are in need of clarification to verify the fitness of the data shown.

- p4, l111. It is stated that 10 trees were sampled by increment cores from four different orientations (NE, SE, SW, and NW). Do you mean four radii were collected per tree?! If yes, random tree rings were used for 14C analysis or just one tree and radii's? Please,

clarify.

- p5, l148 to l55. How the tree specimen selected was dendrochronologically-secure? How the chronosequence of tree rings (prior 14C dating) was obtained without a master chronology for Baboab species?! The passage selected here describes just figure 2. Later (at p.6, l190 to 194), it is explained that no dated tree-ring width chronology from the study region is currently available. Therefore to anchor the chronosequence of tree rings (prior counting of all baobab tree rings) the F14C of the TPBs and Oxcal was used instead. Is this correct?!... If yes, this explanation should appear early on in the text. The fitness of the chronosequence is the backbone of the atmospheric 14C record production using tree rings. Plus, add what type of juniper species you are referring to.

- p5&6, l159 to l65, and l177 to l178. Passages explaining the wood material used for radiocarbon and stable isotope analysis are confusing, and very troubling. It appears that the full dataset was produced in two phases, a pre-screening phase with 5 calendar years or so, where just 1/3 of the tree ring (cut parallel to the fiber orientation, in radial direction from the cambial zone) was used. In a second phase, in order to measure the remaining calendar years, just 2/3 of the single tree ring was used for 14C dating. The remaining material was then used for 13C. This description gives the idea that the tree ring cutting for isotopic analysis was selective, before chemical extractions took place.

Normally a homogenized cellulose-extract of a full single whole-ring (from early- to late-wood) is used to reconstruct atmospheric 14C data. It is understandable that since the baobab contains 69-88 % parenchyma cells, mostly concentrated at the terminal parenchyma bands (TPBs) or late-wood, this portion was removed. But if the remaining material was further sub-divided by removing wood material representative of the growth season (Figure 2), unexpected 14C offsets would then be expected, especially at the slopes of the bomb peak. Accurate cutting of the tree rings is paramount for the reconstruction of atmospheric 14C data. This was already demonstrated by the intra-annual analyses of F14C for the calendar years of 1962 and 1963 shown here. Moreover, if the wood cutting was indeed selective (prior chemical extractions, as mentioned above), I do not understand how the ms can assert at the abstract that "considerable autocorrelation was found in the d13C series, confirming incorporation of previous years' carbon significantly affecting the average age of derived wood", if the wood material tested was not the same. Analyses of δ13C, δ18O, as well as 14C should be done from homogenized cellulose-extracts from the same wood aliquots. Please, clarify.

-p5, l164 & 165. Some of the TPBs removed were selected for 14C dating in phase one (4 or 5 samples). There is no mentioning of the chemical treatment they were subjected to prior sample processing for 14C-AMS. Please, explain...

-p.6, l182 to 190. This portion is very confusing. The TPBs F14C and OxCal were used to anchor the chronosequence. This would give a general idea of the calendar ages of these tree rings, which is ok. But since no chemical extraction appear to have being applied to TPB samples (no description of such is offered), I do not understand why one should expect that they would match with the NH3. Please, rephrase statements.

Regarding figure 3, and text portion between l187 to 190. What do you mean w/ "the baobab samples' position on the time axis is relative to their position within the tree ring of a growing season lasting from June until September"? Were the calendar years in the "x-axis" of figures 3 (and figure 8, as well) adjusted to match w/ the growing season of the baobab species as shown in Fig. 1C (June to September)? It is hard to see if such adjustment was applied in figure 3, as the figure is small. But I think that this adjustment was not applied to figure 8, as it should, and therefore the entire baobab F14C values are too far to the left. Have you take this monthly shift in account in the modelling as well? This calendar year adjustment should also appear at Table 2, second column to avoid confusing between growth date and dendro-date.

On figure 3A, I am left unsure (without checking all records in Hua et al. 2013 supplementary material) the main differences in uncertainties between SH3, SH1-2 and NH3 records beyond about 1972 (orange shaded area). Why is this shaded area particularly

different from all others, when the SH3 record (based on Muna Island data) stopped in 1979? Beyond this calendar year most records assume no differences between hemispheres due to scarcity of data in the tropics. Please, explain.

Figure 3B, I appreciate the effort of showing F14C values between the calendar year of 1962 to 1964, but further discussions on air mass circulation (as mentioned earlier) are still lacking. Since the citation of Nydal & Lovseth 1983 is already listed in the article, all other records in the same zonal band in this article should be added to the plot. Second, most of the citations in this figure legend are not in the reference list. Third, replace Turnball et al. 2017 by Turnbull et al. 2017.

-p.9, section 3.1. I do not understand why one should expect that the TPbs would match with the NH3, or even match with the TRs (holocellulose extracts, Table 2). I don't see how this part is relevant. Most importantly would be comparisons between F14C data of TRs and alternative alpha-cellulose treatments that target the removal of starches and sugars (e.g., "Soxhlet"-type extractions using solvents). Note that the alpha-cellulose extraction described here was attained by adding an extra step of 17.5% NaOH to the holocellulose procedure. Incomplete removal of resinous compounds during chemical pretreatment of tree rings biasing 14C data has been shown by others (Cain and Suess 1976, Westbrook et al. 2006, for example).

-p12, section 4.1. I found this section highly speculative; especially when no 14C dating targeting starch extracts from the baobab parenchyma-dominated wood was attempted. Richardson et al. (2013), cited in this section, indeed found direct evidence for 'fast' and 'slow' cycling reserves in stemwood. However, Richardson et al. (2013) also stated that even though aboveground temperate forest trees contained very old pools of starch and sugars, stressed trees would still use up first all available present-day fast cycling carbon pool to support growth and metabolism. This would include even the most recently added starch molecules. Therefore, the usage of "older" NSC reserves was set for times of stress. Richardson et al. (2013) did not mentioned that ring cellulose 14C results were inaccurate after direct comparison with the northern hemisphere atmospheric record, just the NSCs extracts (sugars and starches) were. Those compound fractions were chemically extracted separately by standard protocols for the purpose of 14C dating. I think it will be important to make this distinction in the text to avoid misleading the reader.

While I do not think that it is completely impossible that the baobab tree species incorporate slow-turnover NSC into its ring cellulose structural carbon fraction year-after-year (regardless of the environment stress conditions surrounding it), I think that the ms fail to: 1) clearly demonstrate this phenomenon, 2) properly justify it, and discuss external bias for the 14C offsets. A short sequence of a non-parenchyma-dominated wood chronosequence of tree rings dated by 14C bomb peak from this region, such as the juniper species mentioned in text, should resolve most (if not all) the issues raised here. Therefore, I cannot see how this manuscript can be published without major revisions, or even further demonstrations.

Reference list cited here:

Cain and Suess 1976. Journal of Geophysical Research 81(21):3688 Hua et al. 2013. Journal of Geophysical Research: Oceans 88(C6): 3621. Richardson et al. 2013. New Phytologist, 197: 850. Turnbull et al. 2017. Atmospheric Chemistry and Physics 17(2). Westbrook et al. 2006, IAWA Journal 27(2): 193

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