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Climate of the Past Discussions

Interactive comment on "The oxygen isotopic composition of phytoliths from tropical rainforest soils (Queensland, Australia): application of a new paleoenvironmental tool" *by* A. Alexandre et al.

Anonymous Referee #1

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This manuscript describes observations of the oxygen isotope composition of wood phytoliths (plant derived silicate biominerals) preserved in tropical rainforest soils in northern Queensland, Australia . The work is novel, since the majority of previous studies have focused on grass phytoliths, and the approach has definite potential. In particular, because the deposition of wood phytoliths within trees is not associated with any known evaporation, their isotopic composition (δ18O-wood phytolith) is less likely to be altered by evapotranspiration effects, as is the case with grass phytoliths. In this respect, this paper represents an interesting first look at δ18Owood phytolith and attempts to validate its use as a palaeoclimate proxy through comparison with spatial climate and isotopic variables. However, few of these potential environmental controls

are constrained in this study, leaving a good deal of uncertainty surrounding interpretation. The authors have to resort to estimating or assuming a variety of factors, including local climate (temperature and precipitation), surface water evaporation (assumed to be negligible), the age of the soil samples (based on one radiocarbon measurement of >600 years) and the isotopic composition of rainwater and soil water. The need to estimate soil water isotope ratios is particularly problematic when discussing the climatic signature preserved in δ18O-wood phytolith. Ultimately, this leads to a case of comparing two unknowns. I feel that some of these concerns can be dealt with through a more detailed and open discussion and illustration of the various unknowns, and some suggestions are made below. However, even if such corrections are made, I am unsure as to whether this work warrants publication in CPD.

Major comments:

Title: I suggest to add reference to "wood phytoliths" in the title. Also, this paper is not so much an application of a new tool, rather an attempt to validate, calibrate or assess, so I suggest replacing "application" with "validation".

Prediction of δ18Oprecipitation. I feel this is the major limitation of this work, and the paper would benefit greatly by a more detailed and apparent discussion and illustration of how this is done. The errors associated with these calculations should be incorporated into Figures 4 and 5. It is suggested (in minor comments) that the various corrections applied to the δ18O-silica data could be compared in an additional figure and perhaps the same could be carried out with various permutations for δ18O-precipitation.

The assumption of no soil water evaporation is also questionable. Ultimately, the absence of actual analyses of soil water δ18O is a major drawback. The amount of soil water evaporation and the associated isotopic fractionation can be estimated from climate data, and this should be attempted here, with the various permutations compared as above. In addition, more thought should be had as to lateral throughflow of soil water, and possible influence of groundwater.

No mention is made concerning the seasonal/interannual weighting of water uptake/silica synthesis. If, as the authors suggest, phytoliths are formed within a matter of days, and that the majority of rainfall (and presumably vegetation growth) takes place during the summer monsoons, then what effect does a summer weighting have on the final temperature-fractionation plot? As above, it would be interesting to see various seasonal scenarios tested and compared. Similarly, over ~600 years a good deal of climate variability has occurred. How does such variability effect the interpretation?

Minor comments:

Page 1694, line 13: "Queensland rainforests". This implies that Queensland is the name of the rainforests, so replace with either a more specific reference to the name of the forests, or with "... tropical rainforests of northeast Queensland (Australia)".

P 1694, l 21: "δ18Oprecipitation estimates ...". First you describe the apparent fractionation relationship, before using this relationship to assessing the estimates for δ18Oprecipitation. This is both confusing and unnecessary, so best to stick with the former alone.

P 1694, l26: "estimates uncertainties". Change to "uncertainties in estimated parameters".

P 1697, l9: Point (1) I think what you are trying to say here is that soil tops account for the majority of phytoliths found in lake sediments. This needs to be more clearly stated.

P 1697, l10: "lake's" – change to "lake"

P 1697, l12: Point (2) I don't understand why weak concentration of phytoliths in soils, rivers and lakes is a reason to be positive. Is your argument that it's better to look at soils than lake sediments? The argument that soils represent a smaller period of time than lake sediments is not supported by the >600 year age of your soil sample. Perhaps a better argument would be that by analysing soil samples, you can examine

the conditions closer to the phytolith source.

P 1697, l20: "for comparison, mean age of 100s of years" change to "a mean age of several hundred years", or be specific.

P 1697, l22: Are these calibrated radiocarbon years? If so, how were the ages calibrated?

P 1697, l24: I suggest to remove the mention of phytolith-occluded 14C analysis. This is not required here.

P 1697, l26: Point (3). This sub-section needs re-writing to improve clarity. The argument that wood phytoliths are not effected by evaporation is a good one, but it is not a reason to carry out the research in tropical Queensland, as stated at the beginning of the paragraph.

P 1697, l28: "Indeed, they are constituted by more than 80% of a type of phytolith... named as Globular...". This sentence does not follow from the previous – i.e. it is not a reason that phytoliths from rainforests are suitable for palaeoenvironmental interpretation. If the reason for the above is that the soils consist of primarily wood-phytoliths, then this needs to be mentioned earlier. Also, the sentence needs re-wording to "Soils consist of more than 80% of a single phytolith type, in this case Globular granulate..." or similar.

P 1698, l5: Point (4) this sub-section is a little unclear. Also, is it not a problem that the main variable of interest – temperature – varies relatively little?

P 1701, l15: "Mont Edith" - should this be English "Mount"?

P 1702, l17 μsm – should be μm?

P 1706, l18: "classified according to the classification of Twiss" better written as "classified following Twiss..."

P 1704, l9: "... values were the ones expected.." better written as "... values conform with those expected from..".

P 1704, l13-16: "Moreover. . ." The meaning of this sentence is not clear to me, please re-write for clarity.

P 1705, l9-13: Correction for quartz particles. Is this done simply on number of particles, or particle area or volume relative to phytoliths? What is the estimated error due to this correction? How are these corrections for contamination treated with respect to the CIE calculations?

P 1705, l20: Correction for grass phytoliths. Is it justifiable to apply a relationship derived from the dry North American Praries to the humid Australian tropics? More argument is required here. There are a number of approaches to modelling the evaporation and isotopic enrichment of surface waters, so it would be interesting to know how these compare with these estimates. Similarly, errors should be quoted here.

P1706, l12: No correction for presence of organic matter. Sample sizes of 1.6mg are analysed for δ18O. What is the effect of measuring samples smaller than this in terms of accuracy and precision? If samples are contaminated by upto 12% organic matter, then effectively the sample size is reduced. Are sample sizes adjusted to accommodate this contamination, and what are the effects on the analysis?

P1706, l22: Remove "fast".

P1707, l7: "amount weight mean" change to "weighted"

P 1707, l22-26: "Driving factors. . .". This statement is misleading. The Bowen and Wilkinson (2002) equation is based entirely on spatial data, and doesn't factor in changes in temperature or any of the variables mentioned. We are simply assuming that those correlations between climate and space apply with time and space.

P 1708, l3: Eq. 5 and 6 given in brackets – is this necessary? I'd leave out of the brackets.

P 1708, l6 onwards. "The dominance. . .". This is a very long sentence and it becomes very difficult to follow after the list of references in line 9. From "and for the leeward. . .". The sentence needs fragmenting and re-written for clarity.

P 1708, l14: ". . . the cumulative difference.." change to "the cumulative change in altitude ($\triangle$ALT) along a SE-NW transect (i.e. parallel with the trajectory of the dominant trade winds) was used to estimate dP at both windward and leeward sites."

P 1708, l16: Change "The" to "This procedure".

P 1708, l16: How is this procedure justified? Are there any obervational data to suggest that the predictions applied here have any grounding in reality?

P1709, l2: Inland distance term (-0.08(DIST)). Again, what is the justification for this? How was this factor estimated?

P1709 Eqs 7 and 8. Why use different equations for <200m and >200m altitudes? Does using the >200 m equation lead to markedly different predictions?

P1709, l10: Comparison with summer δ18Oprecipitation. Assumption that mean seasonal values are representative for mean annual values. Then why not simply calculate seasonal values, rather than annual ones? Or better still, why not calculate a range of seasonal values for comparison?

P1709, l16: Comparison with observed data. Were the above amendments to the δ18Oprecipitation predictions made to aid comparison with these data? If not, how were the above modifications justified and where were the parameters derived from?

P1710, l25: "presented in Table 2."

P1711, l1: "Whatever is the non-wood". Change to "The non-wood phytolith correction is consistently <0.4‰"

P1711, l9: It would be better to discuss the data with Bartle Frere included before justifying their exclusion from the regression.

P 1712: Comparison of fractionation estimates made using various corrections. A figure here would be very useful, e.g. showing different regression lines according to presence/absence of different corrections.

P 1713, l3: “close negative linear” remove “close” – slight exaggeration.

P 1713, l24: “previously accounted” change to “previously suggested to account for..”

P 1714, l3: “exchanges” – change to “exchange”

P 1714, l4: “Extents of” change to “The extent of..”

P 1714, l5: “In the same time” change to “At the same time”.

P1714, l18: “the relationships”. Which relationships? State explicitly what you mean here.

P 1714, l18: “It can be..”. Again, state explicitly “The relative difference between x and y can be explained if..”. In general, this paragraph could do with some extra attention to aid clarity.

P 1714, l20: Evaporation of surface water. See note above, this can be estimated from climate data and the sensitivity of the δ18O signal tested numerically.

P1715, l1: Malanda and Walkamine – at first I thought this was a reference with the year missing. Perhaps better if these names are not put in brackets.

P 1715, l4: “At least” I think you mean “Finally, ”

P 1715, l14: “instead of a” change to either “in contrast to” or “compared to”.

Figure 4: Please add units to y axes of two figures on the right. Also, please add y axis error bars which account for both analytical error (0.5‰ and error associated with correction for δ18O data. Error bars for prediction of δ18Owater, and also those associated with estimation of MAT and MAP should also be added.

Figure 5: Add units to both axes and error bars on both x and y axes. The resolution

quality of this figure could be improved.

Interactive comment on Clim. Past Discuss., 7, 1693, 2011.