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www.clim-past-discuss.net/11/C381/2015/

Climate of the Past Discussions

Interactive comment on "A millennial summer temperature reconstruction for northeastern Canada using oxygen isotopes in subfossil trees" *by* M. Naulier et al.

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Scientific Significance

[2.1] Does the manuscript represent a substantial contribution to scientific progress within the scope of Climate of the Past (substantial new concepts, ideas, methods, or data)? YES- new and novel data carefully compiled. A large amount of effort has been invested to produce this record which represents an important step in understanding the isotope and climate variability of this region.

Scientific Quality

[2.2.1] Are the scientific approaches and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)? Yes, the methods are appropriate for the dataset and significant background work has been conducted prior to completion of the long timeseries. Some detail on the signal strength during the calibration period might be helpful to gauge the suitability of the levels of replication.

We have added information on the calibration/verification results (lines 281-284): "However, it appears that the calibration coefficient and verification are changing according to the choice of the period. This fact can be explained by the fact that the correlation between maximal temperature and δ18O series is not always stable during the last century even if the correlation stays significant."

[2.2.2] The join point plus off-set pooling approach used has been demonstrated to work, but without doubt sampling more longer-lived trees which cross cohorts would strengthen the lower frequency signal further still. Is there any evidence of a juvenile effect in the oxygen isotopes?

Black spruce trees are recognized for having lifespan generally shorter than 400 years. In our study, the living trees sampled did not exceeded 200 years. Considering in addition that (1) the boreal forest is known for having a high fire recurrence, and (2) the high rate of rejection during sampling due to strong wood degradation, it was extremely difficult to find longer-lived trees at the study site. Nevertheless, we think that using long-lived trees which cross cohorts could be a good approach for improving the correction robustness of cohort off-sets.

We know that a juvenile effect can exist with δ^2H (or δ18O) tree-ring series during the first twenty years of growth (Lipp et al., 1993). In order to determine if this effect existed in black spruce trees from northeastern Canada, we sampled all tree rings of living trees and analyzed their δ18O series individually. As can be seen on Figure III, we determined that there is no juvenile effect when oxygen isotopes are used in black

spruce trees from northeastern Canada.

[2.3.1] The interpretation of the record as temperature provides a general interpretation based upon the calibration period, however, and as the authors correctly state, oxygen isotopes are not a single instrumental temperature variable, but relate instead to circulation, hydroclimate, temperature etc. Interpretation as temperature may be more "accessible" but may only tell a part of the story. In this respect, I would personally be cautious in reporting differences in "temperature" during the past without a more detailed discussion of the isotope climate of the region, particularly when the recent past (last 5 years) do not closely calibrate (the decision not to calibrate with this truncated period could be misinterpreted so perhaps the full - period calibration could also be presented and reconstructed).

As stated by the referee, temperature is not the only climatic parameter controlling the oxygen isotopic fractionation in northeastern Québec. For that reason we had already attested for that fact in the discussion. It is worth noting that recent studies of living trees in the same region have already shown and discussed why summer temperature exerted the main control on the δ18O variations (Naulier et al., 2014, Bégin et al., 2015). We suggest not repeating that discussion here. We have also verified that during the period of divergence, there is no other correlation with other parameters gaining in statistical strength (i.e., VPD, snow cover, humidity, precipitation). Concerning the presentation of the full-period calibration, it has been tested and unfortunately it leads to a much weaker statistical link (r^2=0.35) than when calibrating after removing the divergent period (r^2=0.64).

[2.3.2] However, that said, reporting this as a "divergence" in the dendroclimatological - sense may equally be misunderstood by some dendrochronologists/climatologists less familiar with the approach, since there is no evidence that the relationship in isotope fractionation has changed, just that the relationship between the isotopes sampled by the trees and summer temperature has changed. It may be possible to draw support for this hypothesis using local GNIP data. Alternatively, if the species is suitable for

densitometry then there may be scope for a detrended reconstruction of temperature against which to compare the oxygen isotopes through time. Interpretation against solar variability is appropriately cautious.

Please read reply to comment 1.5.1 by M. Way concerning the divergence problem: "The comment suggesting the possibility of changes in growing season can have occurred in previous phases of the millennial record is valid. Inevitably, this sort of consideration referring to past changes in climatic regimes is always pertinent when discussing reconstructions of climatic parameters based on statistical models. This limitation is inherent to the calibration method which assumes that present conditions are warrant of the climatic past. The validity of the assumption could eventually be assessed by combining ecophysiological approaches with isotopic reconstruction."

The change in length of the growing season is a fact depicted by regional instrumental records. This change over the last decade, namely an extension of growth duration, from June-August to May-September is likely the cause of divergence, which was specifically operated through a change in source water isotopic ratio (Naulier et al., in press). Unfortunately, this assumption cannot be reinforced by using GNIP data because at the L20 area, the δ18O series of precipitation is only available for a very short period (1960-1969) which does not include the divergence period. However, the option of using ring densitometry to obtain a detrended reconstruction of temperature to be compared with the δ18O series is an excellent suggestion that should be applied in a new project.

Presentation Quality

[2.4] Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)? YES – the paper is clearly written and well written. Some axis labels require attention (maximale etc.) otherwise the figures are of good quality.

The mentioned problem was related to Figure 4 of the paper and it has been corrected.

In the full review and interactive discussion the referees and other interested members of the scientific community are asked to take into account all of the following aspects:

[2.5] Does the paper address relevant scientific questions within the scope of CP? YES

[2.6] Does the paper present novel concepts, ideas, tools, or data? YES – new data for the region; a terrestrial isotope record.

[2.7] Are substantial conclusions reached? Isotope climatology is at an early - stage, but appropriate conclusions are drawn.

[2.8] Are the scientific methods and assumptions valid and clearly outlined? This work has been VERY carefully conducted and thoughtfully compiled. Important background work has been conducted over many years. The join-point approach seems to work well here, but additional replication at both join-points and within the body of the reconstruction would undoubtedly strengthen the record, as would incorporation of longer-lived trees. Interpretation as temperature is only part of the "isotope story". The truncated calibration should be addressed in more detail here.

Indeed, additional replication at both join-points and points within the body of the reconstruction would undoubtedly strengthen the record: in statistical reconstruction procedures, the higher is the number of samples; the more robust is the reconstruction. However, we assure the referee that we have made hard and long work in order to obtain the maximum number of these lake-extracted subfossil trees for the reconstruction, having had to discard trees with short lifespan, rings too small for mechanical sampling or with textural degradation. After this sorting, cohorts of 5 trees per years were available to cover the entire millennium, and between 10 and 25 trees for join points were available. The region investigated is far and isolated, the option of increasing these numbers would require substantial research funds and demanding logistics, but in the end, we do not think it would change the main conclusions presented in the article.

[2.9] Are the results sufficient to support the interpretations and conclusions? Yes, within the limits of the calibration as presented.

We have explained why the calibration period is reducing to 1930-2000 in the methodology section. Please note that the divergence problem is the specific subject of Naulier et al., in press).

[2.10] Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Although some elements of the methods are brief. The cited papers identify the many elements of this work have been evaluated by the team elsewhere, so this information is available I believe. I could not see/access supplementary data (see note below) so it was not possible to attempt to recalculate.

In the methodology section, we present briefly the main steps of the method because the sampling strategy is well explained in Gagen et al., 2012. However, the changes made to the approach are discussed in details both in the methodology and the discussion. Additionally, all isotopic results are available on-line in Naulier, 2015 (Ph.D. thesis).

[2.11]. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

[2.12]. Does the title clearly reflect the contents of the paper? Yes.

[2.13]. Does the abstract provide a concise and complete summary? Yes.

[2.14]. Is the overall presentation well-structured and clear? Yes.

[2.15]. Is the language fluent and precise? Yes.

[2.16]. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.

[2.17]. Should any parts of the paper (text, formulae, figures, tables) be clarified, re-

duced, combined, or eliminated? See review notes.

[2.18]. Are the number and quality of references appropriate? Yes.

[2.19]. Is the amount and quality of supplementary material appropriate? I was not able to access /see a link to any supplementary data – but as a minimum the raw and join point corrected data should be presented versus calendar age in addition to the final reconstruction.

We have added the reference to the Ph.D. thesis of the first author (Naulier, 2015) that is available online. Any interested scientists can access the tables presenting all results, δ18O values of join points and cohorts before and after their adjustments, etc. The supplementary material presented (Table I, II and III) completes the database with information on lifespan of trees and sampled periods and i-STREC data.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/11/C381/2015/cpd-11-C381-2015-supplement.pdf

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Interactive comment on Clim. Past Discuss., 11, 521, 2015.

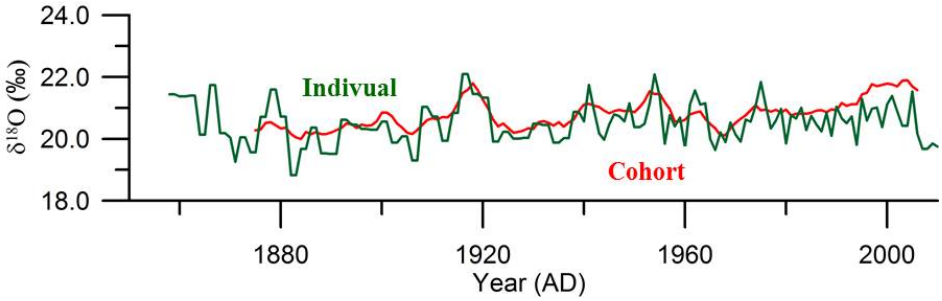

Fig. 1. Figure III. The δ18O series of 4 living trees sampled at an annual resolution (green curve) and for 5 trees sampled according to the cohort method (red curve).