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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

[3.1.1] This is clearly an exciting new record, adding to the few millennium long isotope series that are currently emerging. It is a nicely written manuscript using an excellent dataset. Still, I would have liked to see a more critical discussion of the method (see below) as well as using isotope data from subfossil wood in terms of temporal stability of the signal (something I have thought about quite a lot regarding the data we collect in Sweden). As an example, it is stated that the last decade of data is not used due to changes in the growing season, but maybe also the "MWA" was a pe-



riod of increased/changed growing season? See also my comment below regarding fluctuations of lake levels.

This comment suggests the same reply as to comment 1.5.1: "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 reconstructions".

[3.1.2] Also, it is interesting of course to compare a new record to others to see how it "performs", but I feel that this new record warrants a deeper discussion of its pros and cons.

We have added new information about the pros and cons of the sampling method on lines 257-266. "Although the cohort sampling method has shown many positive points, it is nevertheless important to highlight some concerns about this procedure. Indeed, the sampling strategy produces a $\delta$18O series smoothed with a centered 9-years filter. This smooting leads in some cases to series requiring more care than non-smoothed series before they can be interpreted or used. For instance, the calibration of our smoothed $\delta$18O series required a centered 9-years filtering of the climatic series. Consequently, correlations between isotopic and climatic series are generally overestimated. It is important to highlight that even if the correlations are improved by smoothing due to the sampling method; they nevertheless represent a solid and real link, and do not create an artefact (see also the discussion on the calibration/verification method of next section, Naulier et al., 2014)."

Scientific quality

[3.2] The (cohort) method has been tested before, but personally I feel that the ap-

proach, where there is only a slight overlap between to neighbouring (short) cohorts, can possibly introduce some bias, which should be discussed.

The slight overlap between cohorts does not introduce bias because, as written in the paper, "the $\delta$18O values of intersection points between two successive cohorts and JP $\delta$18O means are surprisingly matching in nine cases out of eleven" (Figure 3A). These observations are expressing a robust coherence of the sub-populations of stems that were sampled in lake L20 of northeastern Canada.

[3.3.1] Could the age of the trees as well as a possible disintegration of the sapwood cause any impacy on the results?

As written in the methodology section, we have removed all trees that presented textural degradation and subsequently assessed the integrity of the isotopic ratios in subfossil cellulose by comparing it with lignin values in modern living trees and in modern and very old subfossil trees. In addition, as shown in Figure IV, we have addressed the issue of using subfossil trees and of the reliability of their isotopic ratios. Indeed, as written in our article, we have used the textural pre-selection of wood combined with verifying the coherence of the cellulose isotopic values as proposed by Savard et al. (2012). This practical step consists in calculating the cellulose-lignin DELTA$\delta$18O values (subtracting the lignin $\delta$18O values from the cellulose values). This isotopic difference was calculated for the 1890 to 1905 period during which the subfossil and living series are overlapping, and for two contrasted climatic periods of the subfossil record: 1145-1160 and 1745-1760.

These DELTA$\delta$18Olignin-cellulose values in living trees vary between 12.8 and 15.4‰ (Figure B, D), matching the variability found for subfossil stems (1890-1905; D) and others that grew up under different climatic conditions (1145-1160 (B) and 1745-1760 (C)). This approach allowed assessing the integrity of the isotopic results over the last millennium.

Concerning the age of trees and the potential juvenile effect, we invite the referee to



read the answer to comment 2.2.2 by N. Loader, which explains why we consider that the studied black spruce trees do not show juvenile effect in $\delta$18O series: "We know that a juvenile effect can exist with $\delta^2$H (or $\delta$18O) 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 $\delta$18O 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."

[3.3.2] Were some trees included in more than one cohort?

Not at all, every tree is used only once. Please see the now added supplementary material (Table I and II), where the lifespan of trees and tree rings sampled for constructing each cohort are all identified.

[3.3.3] Also, it is clear from the calibration/verification exercise that the strength of the Tmax signal differs between the periods (e.g. RE and CE values). I think that the impact of this manuscript would improve a lot if the data and methods were addressed a bit more thoroughly.

We have added explanations about why the signal differs between the periods on lines 282-286: "However, it appears that the calibration and verification coefficients are significantly changing depending on the period selected for the statistical analysis. This observation implies that the correlation between Tmax and $\delta$18O series is not stable over the last century even if the correlation stays significant (Naulier et al. in press)."

Presentation quality

[3.4.1] The presentation of the data is OK, but slightly more information on the trees used in the study would be beneficial, as well as the methods used.

Please see the reply to comment [3.3.3].

[3.4.2] However, I feel that too much text is spent on trying to link the i-STREC to

pervious reconstructions and find potential forcings of the observed long-term variability. I would have liked to see a more detailed discussion about the potential impacts of using isotopes from lakeshore trees as temperature indicators (where it has been suggested that fluctuating lake levels can affect the temperature sensitivity), or if the isotope values may be affected by being submerged.

Please see reply to comment of R. Way (1.1) where he suggests that the paragraph comparing i-STREC to other reconstructions need to be longer. According to comments from reviewer Way and knowing that the comparison with other series is used to invoke climate-driving mechanisms, we are led to think that the length of the paragraph is appropriate.

The impact of lake level variations on $\delta$18O series of riparian trees has already been assessed in Naulier et al., in press. In this article, we have compared $\delta$18O series of riparian trees sampled at L20 with $\delta$18O series of trees sampled at a nearby mesic site (HM1). We have found that series from HM1 and L20 were strongly correlated (r=0.88) and showed similar variations. This observation indicates that level fluctuations of the lake do not affect the $\delta$18O series of riparian trees at the study site.

[3.4.3] Also, I would have liked to see the corresponding TRW chronology (based on the same "cohort" method) from this particular lake to compare with the isotope chronology. It is clear that they differ and the discussion of this could be more informative.

Agreed. However, obtaining and comparing the $\delta$18O series with a TRW series obtained using the same cohort approach is a colossal task that could be the subject of another article. We would like to remind the referee that our main objective here was to reconstruct the last millennium in northeastern Canada by using isotopes, not ring width series as already used by Gennaretti et al. (2014). In addition, the TRW information corresponding to trees used for i-STREC are all reported in the supplementary material of Gennaretti et al. (2014). Concerning the comparison of TRW and $\delta$18O series for our study site, we did it and we discussed the differences between them in



a long paragraph of the discussion, underlining that even if the signals extracted from the two series sometimes diverge due to differences between processes influencing isotopic assimilation and ring-width growth, the two signals give complementary information of the past climate in northeastern Canada (lines 402 to 441). Finally, we think that applying the cohort approach to TRW series is not adequate as climatic reconstruction using this proxy is not limited in terms of number of samples or analytical costs, two constraints that unfortunately exist when working with isotopic series.

Access review, peer review, and interactive public discussion (CPD)

[3.5] Does the paper address relevant scientific questions within the scope of CP? Yes.

[3.6] Does the paper present novel concepts, ideas, tools, or data? Yes, but could be improved with some critical discussion of the data and methods.

We have added new points about pros and cons of the method on lines 257-266: "Although the cohort sampling method has shown many positive points, it is nevertheless important to highlight some concerns about this procedure. Indeed, the sampling strategy produces a $\delta$18O series smoothed with a centered 9-years filter. This smooting leads in some cases to series requiring more care than non-smoothed series before they can be interpreted or used. For instance, the calibration of our smoothed $\delta$18O series required a centered 9-years filtering of the climatic series. Consequently, correlations between isotopic and climatic series are generally overestimated. It is important to highlight that even if the correlations are improved by smoothing due to the sampling method; they nevertheless represent a solid and real link, and do not create an artefact (see also the discussion on the calibration/verification method of next section, Naulier et al., 2014)."

[3.7] Are substantial conclusions reached? Yes, although a slightly more critical discussion would be beneficial, and may change the conclusions. Also, I'm not fully convinced by the presented figures that solar forcing is the most important forcing.

In the paper, we showed that several solar minima (more than the half) coincided with periods of minimal temperature in northeastern Canada. We are aware that the solar forcing is not the only forcing that played along to regulate climate in the study region. However, at this stage, the solar radiations appear as being the main climatic forcing that may have dominated during the inferred periods of minimal temperature. All the other forcings that we have examined did not show matching periods with temperature extremes (NAO, AMO, El-Nino, volcanic eruptions...). In simple terms, here we present solar forcing to control temperature lows in northeastern Canada as a possible interpretation, which in the case of the period of low T of the 19th century is combined with volcanic activities. Clearly, we do not discard the possibility that other forcings have exerted key roles during the last millennium.

[3.8] Are the scientific methods and assumptions valid and clearly outlined? OK.

[3.10] Are the results sufficient to support the interpretations and conclusions? Would likely change somewhat after revision.

[3.11] Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? I'm not fully convinced by the cohort method because of the limited overlap between them, however, this may be clearer if also the time spans of the samples are shown (since I guess that one tree can contribute to several cohorts?).

Please see replies to comments [2.8] by N. Loader and [3.3.2] for these points. Figure 2 illustrates the time-span covered by every investigated tree. Of course the life span of trees was longer than the part sampled for the reconstruction, because the method requires using 5 stems overlapping in time, i.e. removing rings that are not necessary for producing the millennial isotopic series. The reader is referred to the supplementary material (Table I and II) where all years covered by dated rings in subfossil stems are reported.

[3.11.2] Also, I feel that the discussion of the influence of the AMO (and lack of NAO)



is a bit speculative.

The discussion about the possible AMO control is based on other papers addressing that question for northern Canada. However, we are aware that based on the knowledge of the AMO state during the MWA, its potential impact on summer temperature in the study region is only an assumption. According to comment 1.7 by R. Way, we now discuss the potential influence of the Labrador current on temperature of the L20 region over the last millennium (lines 328-336 of the manuscript): "In contrast, the AMO influences spring and summer temperatures (Fortin and Lamoureux, 2009) and is partly responsible for the recent sea surface temperature warming of northeastern Canada (Ding et al., 2014). However, the state of the AMO at the beginning of the millennium being unknown, it is difficult to assess its influence on climate during the MWA. Recently, Sicre et al. (2014) have demonstrated that during the MWA, the strong Northern Annular Mode (NAM) was concomitant with a strong ice-loaded Labrador Current (LC). This combination could be responsible for a decrease of fresh air from Arctic to eastern Canada and consequently, for an increased temperature along the continent during a part of the medieval period."

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

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

[3.14] Does the abstract provide a concise and complete summary? OK (but why the Medieval Warm Anomaly??? MWP or MCA).

We are using MWA all along the text as used by several authors (e.g., Trouet et al., 2009; Mann et al., 2009). But is it clear that MWP is also a largely accepted appellation in the literature.

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

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

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

[3.18] Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See previous comments

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

[3.20] Is the amount and quality of supplementary material appropriate? NA.

âĂČ References

Gennaretti, F., Arseneault, D., Nicault, A., Perreault, L., and Bégin, Y.: Volcano-induced regime shifts in millennial tree-ring chronologies from North-eastern North America, Proceedings of the National Academy of Sciences of the United States of America, 111(28), 10077-10082, 2014. Naulier, M., Savard, M. M., Bégin, C., Marion, J., Arseneault, D., and Bégin, Y.: Carbon and oxygen isotopes of lakeshore black spruce trees in north-eastern Canada as proxies for climatic reconstruction, Chemical Geology, 374, 37-43, 2014. Naulier, M., Savard, M.M., Bégin, C., Marion, J., Nicault, A. and Bégin, Y. Temporal instability of isotopes-climate statistical relationships- A study of black spruce trees in northeastern Canada, Dendrochronologia, in press. Mann, M. E., Zhang, Z., Rutherford, S., Bradley, R. S., Hughes, M. K., Shindell, D., Ammann, C., Faluvegi, G., and Ni, F.: Global signatures and dynamical origins of the Little Ice Age and Medieval Climate Anomaly, Science, 326, 1256-1260, 2009. Savard, M. M., Bégin, C., Marion, J., Arseneault, D., and Bégin, Y.: Evaluating the integrity of C and O isotopes in sub-fossil wood from boreal lakes, Palaeogeography, Palaeoclimatology, Palaeoecology, 348–349, 21-31, http://dx.doi.org/10.1016/j.palaeo.2012.06.003, 2012. Trouet, V., Esper, J., Graham, N. E., Baker, A., Scourse, J. D., and Frank, D. C.: Persistent positive North Atlantic Oscillation mode dominated the medieval climate anomaly. science, 324(5923), 78-80, 2009.

Please also note the supplement to this comment:



http://www.clim-past-discuss.net/11/C389/2015/cpd-11-C389-2015-supplement.pdf

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

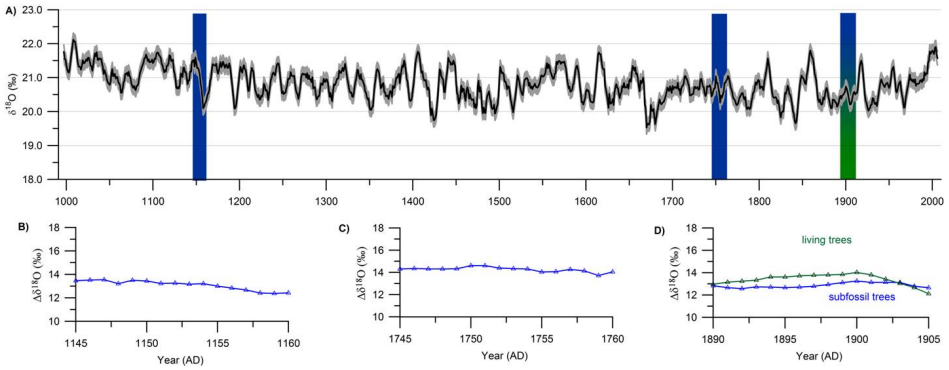


**Fig. 1.** Figure IV. (A) Millennial $\delta$18O series with box corresponding to the period where D$\delta$18O values have been calculated. The calculated D$\delta$18O values for the (B) 1145-1160, (C) 1745-1760, and (D) 1890-1905