

## ***Interactive comment on “NALPS19: Sub-orbital scale climate variability recorded in Northern Alpine speleothems during the last glacial period” by Gina E. Moseley et al.***

**Anonymous Referee #1**

Received and published: 27 May 2019

### **1- SUMMARY AND GENERAL COMMENTS:**

The study by G. Moseley and collaborators presents 11 new radiometrically-dated speleothem records from five caves located on the northern rim of the Alps. Those records are used to propose a revision of the composite NALPS calcite  $\delta^{18}\text{O}$  curve published in Boch et al. (2011) covering the time interval  $\sim 120$ -60 ka. Using those new speleothem records and comparing them to Asian speleothem  $\delta^{18}\text{O}$  records and Greenland NGRIP ice core  $\delta^{18}\text{O}$  records, the authors investigate the coherence and possible lead and lags in the timing of the abrupt stadial-interstadial transitions. To do so, they use a recent statistical tool to objectively define the onset and the end of

C1

the abrupt changes in the different records. They evidence a good agreement within the dating uncertainties between the NALPS calcite  $\delta^{18}\text{O}$ , the radiometrically-dated Asian speleothem composite  $\delta^{18}\text{O}$  and the NGRIP ice  $\delta^{18}\text{O}$  record when the later is displayed on the GICC05modelext chronology (Wolff et al. 2010). The authors discuss the relative influence of the different control factors on the NALPS calcite  $\delta^{18}\text{O}$  record. Finally, the new NALPS composite  $\delta^{18}\text{O}$  curve provides further evidence that the sub-millennial-scale climate variability identified in the NGRIP ice  $\delta^{18}\text{O}$  record is also visible in continental records from the Northern Alps. Taking advantage of their precisely-dated record, the authors propose a possible mechanism for triggering this sub-millennial-scale variability, involving ice sheet melting impacting the intensity of the Atlantic Meridional Overturning Circulation (AMOC).

This paper presents substantial new material and it will be of great interest for the speleothem community and to the extended paleoclimate community. It is thus well within the scope of Climate of the Past. However, many aspects of the paper related both to the form and the content need improvements and clarifications. Hence, I recommend that major revisions are being done before it is considered for publication.

Before I describe my comments in the following, I would like to mention that my limited expertise on the technicalities for dating speleothem records prevents me from providing a careful evaluation of the methodology described here. I thus hope that this important aspect of the submitted work will be carefully addressed by expert(s) in the field.

My first general comment is that I find that the manuscript is unbalanced as some sections of the paper should be shortened while others would deserve to be expanded. In particular:

- Section 2.1 presenting the cave sites and speleothems could be shortened. Instead of a long and tedious text to read, it would be helpful to have an associated table in the main manuscript that summarizes at least some of the information provided in the

C2

text e.g. cave name, coordinates, elevation, air temperature and precipitation rate, associated sample acronyms, length of the samples. . .).

- The discussion is also difficult to follow in Section 4.3, and the take-home messages hard to identify. The authors are investigating and discussing the roles of the different potential control factors on the calcite  $\delta^{18}\text{O}$  records from the different caves. However, I feel that we are left without clear conclusions or discussion of the implications when no firm conclusion can be drawn. This section needs to be written in a more concise and structured way (the authors should consider breaking the text into sub-sections), with a better highlight of the take-home messages. When reorganising the discussion, the authors could have in mind the following key questions to structure the text: (1) what is investigated and on what scientific ground? (2) what is observed? Is it significant or not? (3) What are the implications and how to go further?

- I find the Section 4.1 on the coherence and updates to NALPS19 versus NALPS unsatisfying. I believe that more specific justifications for selecting one speleothem rather than another to build the new composite calcite  $\delta^{18}\text{O}$  record are missing. For instance, it would be useful to provide a quantitative comparison (in a table?) for at least one or two periods (if not all) where there is overlap between “old” and “new speleothems” to better illustrate that the new ones are better dated and hence, more appropriate than the ones already published to constitute the new composite curve.

My second general comment is that I find that many formulated statements, whether it is in the abstract or in the main manuscript, are too vague and/or miss some short background information. It renders the text sometimes hard to follow, especially for non-specialist readers. For instance, in multiple places, the authors state the good agreement of the different chronologies from the speleothem and ice core record within the dating uncertainties, without ever explicitly attached to their statement quantitative estimates of what those uncertainties are (pluri-decadal-scale? centennial-scale?). Another example is the lack of a short sentence providing basic information regarding the different ice core timescales discussed in the text. In the section 2 of my review,

C3

I point out specific places in the manuscript that require revisions. But the authors should go through the manuscript with this comment in mind and revise accordingly when appropriate. I detail below some specific comments and technical corrections that should be considered by the authors when preparing the revised version.

## 2- SPECIFIC COMMENTS:

- While it is relatively long, I find that several statements in the abstract are too vague and should be reformulated to be more specific:

Line 14: “...with highly similar shifts”: this is vague, spell out clearly that you are referring to abrupt changes observed in the water isotopic profiles from Greenland ice core. I think also that one should be careful with the use of “highly similar”, they are not the same proxy. If such comparison is kept, it should be specified in which sense they are highly similar.

Line 18: It is necessary to specify in which term(s) the major transitional events between stadials and interstadials agree i.e. timing of the transitions and/or amplitude of the transitions? In the same sentence, it is necessary to provide also a quantitative average estimate of the uncertainties that are referred to here.

Line 19: “...a good agreement between the NALPS19 speleothem  $\delta^{18}\text{O}$  record, the GICC05modelext NGRIP ice-core  $\delta^{18}\text{O}$  record and...” First, my comment is the same as previously, it is important to make it clear in which term the good agreement is. Second, “GICC05modelext NGRIP ice core  $\delta^{18}\text{O}$  record” should be reformulated. It needs to be clearer here that GICC05modelext refers to an ice core age model (it might not be necessarily obvious to all CP readers). It could be reformulated such as “the NGRIP ice core  $\delta^{18}\text{O}$  record displayed on the GICC05modelext age scale”.

Line 21: “...too young” and “...a longer duration”. By how much? Please be quantitative here and provide at least an order of magnitude.

- For clarity purposes, I think it is important that throughout the manuscript, the au-

C4

thors specify “calcite  $\delta^{18}\text{O}$ ” when mentioning the  $\delta^{18}\text{O}$  records from the different speleothems and “ice  $\delta^{18}\text{O}$ ” and referring to the  $\delta^{18}\text{O}$  from ice cores. They do it in places, but I think this should appear systematically to avoid any confusion.

- P2, line 40: While for further details, the reader can certainly be referred to the Erhardt et al. (2019), a few sentences need to be added to describe the added value of performing such analysis and the general principle and method used for the ramp-fitting of the transitions.

- From P5, line 25: “Results” section:

SI Table 1 could appear in the main manuscript and information could be removed from the section. I found the information provided in the text very technical and from a non-speleothem expert view, I feel that this should better be the supplementary material. Instead the results section could be focused on the description of the different records and a detailed comparison of the timing inferred for the transitions in the paleoclimatic records using the statistical tool of Erhardt et al. (2019).

The authors discuss the relationship between calcite  $\delta^{18}\text{O}$  and calcite  $\delta^{13}\text{C}$  and perform Hendy test. Again, from a non-expert point of view, I would find it very useful to have a few of sentences explaining why they are performing such exercise, what they expect to be able to decipher from such investigation and finally what are the implications of the results of their test.

- P8, line 13: sentence starting with “Furthermore, despite...”. Is there any explanation why the St Beatus records would record a signal that is different from the Gassel samples? It would be useful to provide more information on this.

- P9, line 5: The paragraph regarding the durations of GS-22 and the precursor event is difficult to follow. It would be very helpful if the authors could provide a table that summarises the different existing and new estimates of the durations of GS-22, GS-21.2 and GI-21.2 in the discussed paleoclimatic records. Implications from their new

C5

NALPS composite curve should be expressed more explicitly.

- The numbering of the different sections needs to be revised. There should not be a sub Section 1.1 if there is no Section 1.2 within the introduction section. I have a similar comment with the sub section 3.2.1, 3.3.1 and 3.3.4.

### 3- FIGURES

- Figure 3.c. A sentence to explain the shift in calcite  $\delta^{18}\text{O}$  values between the Asian monsoon composite records and the original data from which it was constructed need to be added.

- Figure 3. In the last sentence of the caption, “NGRIP nomenclature” should be replaced by “the latest INTIMATE event stratigraphy scheme”.

- Figure 4. This figure needs to be reworked to improve its readability. A y axis scale is missing. Transitions should be numbered following the INTIMATE event stratigraphy scheme and it should probably also show the reference curves in the background the reference curves onto which they have performed the analysis. Also, it needs to be clarified what are the three panels in (c), which speleothem records have been used to perform the transition analyses. Again, this would be straightforward if the original curves were shown underneath or in parallel.

- Figure 6. This figure is hard to read. Efforts must be made to improve its clarity. For instance, a triangle symbol should not be used to represent different parameters e.g. in (a), the catchment elevation relative to longitude and in the other panels some stadial  $\delta^{18}\text{O}$  values. For panels (e) and (g), “specific time periods” is vague, they should be specified. As far as I understand the caption for panel (f) is incorrect as only the mean  $\delta^{18}\text{O}$  values for the speleothems covering some selected GI and GS are being shown relative to the catchment elevation and not all. Finally, the expression “Colours are the same as in (a)” doesn’t need to appear after the description of every panel. The authors could simply write the colour code at the start of the caption stating that it is

C6

the same on all the panels of the figure.

- Figure 7. The authors should be explicit on which type of  $\delta^{18}\text{O}$  values they are showing on the title of the axis e.g. ice  $\delta^{18}\text{O}$  for (a), calcite  $\delta^{18}\text{O}$  for (b) benthic  $\delta^{13}\text{C}$  and planktic  $\delta^{18}\text{O}$ .

- Figure S7. More information must be given to understand clearly what is represented: Titles for the two y axes should be provided as well as a description in the caption of the different curves that are represented e.g.  $\delta^{18}\text{O}$  data, uncertainty ranges, probability density plots about the onset, mid-point and end of transition etc.

#### 4- STYLISTIC, TYPOGRAPHICAL COMMENTS AND MINOR COMMENTS

P1, line 16: a space is missing between “using,” and “eleven “.

P1, line 21: Since it is not mentioned previously, it is important here to specify that AICC2012 refers to an ice core chronology i.e. “NGRIP ice  $\delta^{18}\text{O}$  when displayed on the AICC2012 ice core chronology “, or alternatively, the acronym can be spelt out.

P1, line 28: “precursor” instead of “pre-cursor”.

P1, line 29: to write “GS-24.2 COOLING event”.

P1, line 29: “...occurred shortly”. Please be more specific so we have an idea from the abstract if you are talking about a few decades, or a few centuries, etc.

P1, line 35: write “orbital-“.

P2, line 2: “...have been shown to be synchronous within dating uncertainties”, please provide a reference to support this statement.

P2, line 7: There is no need for higher resolved ice  $\delta^{18}\text{O}$  profile to identify the decadal- and centennial-scale variability, it was already visible from the  $\delta^{18}\text{O}$  profile published in NGRIP project members 2004. Only that no one had provided a specific description before the study by Capron et al. (2010). Hence, I think that the sentence should be

C7

rephrased.

P2, line 9: write “centennial-“.

P2, line 14: “GICC05...” Add information regarding the time interval covered by each of the timescales.

P2, line 32: the authors are correct about the age differences between the different chronologies and they should provide a quantitative estimate of them (at least an order of magnitude).

P2, line 35: add a space between (ka) and (Boch et al. 2011).

P2, line 42: “a good agreement”. Please be quantitative here regarding the agreement.

P3, line 3: why 1.1 Regional climate while there is no 1.2 and it follows the long introduction that doesn't have a sub-section heading.

P3, lines 17, 20 and 21: Northern Alps and Southern Alps.

P3, line 23: The formulation is awkward and should be rephrased with a more direct style.

P3, line 32: space is missing between (2015) and (though.

P5, line 26: for clarity purposes, please write instead “samples from Baschg Cave” and similarly in the titles of sub-sections 3.2, 3.3, 3.4 and 3.5.

P8, line 29: here and throughout the manuscript: Erhardt et al. 2019 (not 2018).

P9, line 2: In the paper by Columbu et al. (2017), a well-dated Sardinian speleothem covering GI-25b and GI-25a is presented. The timing of the abrupt transitions is also discussed and compared relative to the timing of the same events when displayed on the different Greenland ice core timescales. This study also provides evidences that there is a good agreement between the transition timing in the speleothem record and when considering GICC05modelext timescale, but that when considering

C8

the AICC2012 chronology, ages are younger by several millennia. The authors should mention this study in their manuscript.

P9, line 3: The sentence should be completed: "...too young by about XX yrs".

P9, line 7: The formulation of the sentence starting by "This demonstrated..." is awkward. It needs to be reformulated.

P9, line 13: I don't find the information in brackets necessary, it can probably be removed.

P9, line 22: I find the title of the section 4.3 quite vague and not really appropriate. The authors should try and be more specific.

P12, line 11: centennial-

P12, line 14: space between (Fig. 7) and (Capron...).

P12, lines 13 and 27 and P13, lines 17, 18, 20 and 22: The use of the word "termination" should be avoided in this context and replaced by e.g. stadial-interstadial transition. Indeed, as the authors know the word "termination" is classically used in paleoclimatology to refer to glacial-interglacial transitions and I think for clarity purposes, it is preferable to avoid introducing this term in a different context and to refer to a different climatic event.

P12, line 19: "Changes IN Ca<sup>2+</sup>" rather than "Changes TO Ca<sup>2+</sup>". Also, I don't find the reference to Rasmussen et al. (2014) appropriate in this context. Instead I would suggest referring to the studies by Ruth et al. (2007). U. Ruth et al., *Geophys. Res. Lett.* 34, L03706, 10.1029/2006GL027876 (2007).

P12, line 25: "the NGRIP nomenclature" should be replaced by "the INTIMATE event stratigraphy scheme".

P13, line 13: space between (Wang et al. 2004) and (Fig. 7).

C9

#### REFERENCES:

Boch et al. *Clim. Past*, 7, 1247-1259, <https://doi.org/10.5194/cp-7-1247-2011>, 2011.

Capron et al. *Clim. Past*, 6, 345-365, <https://doi.org/10.5194/cp-6-345-2010>, 2010.

Columbu et al. *QSR*, 169, 391-397, 2017.

Erhardt et al. *Clim. Past*, 15, 811-825, <https://doi.org/10.5194/cp-15-811-2019>, 2019.

Ruth et al., *Geophys. Res. Lett.* 34, L03706, 10.1029/2006GL027876, 2007.

Wolff et al. *QSR*, 29, 2828-2838, 2010.

---

Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2019-44>, 2019.