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Climate of the Past Discussions
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Interactive comment on "Stable isotopes in cave ice suggest summer temperatures in East-Central Europe are linked to AMO variability" *by* Carmen-Andreea Bădălută et al.

Anonymous Referee #1

Received and published: 20 February 2020

Dear Editor, Dear Authors,

I was very excited to read the study on the interpretation of the stable isotopic data of the cave ice profile of this Romanian cave. From time-to-time, there are reports from the ice cave community that cave ice offers a great potential in paleoclimate research, but much tangible evidence has not yet been presented. This study has the potential to prove the much-mentioned 'great potential' however some revision is needed. The proposed link between the fluctuations of the d18O signal of precipitation (and its signature preserved in cave ice) at the multidecadal scale and the AMO is interesting and, according to my understanding, plausible. My major concern about the manuscript is

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Discussion paper

the presentation and treatment of the geochronological data. The current age-depth model can be doubted and the table presenting the radiocarbon ages also needs corrections. I sum up my major comments in four points below and add some additional minor comments in a list. Hope it can help the Authors to improve their age-depth model. The results are interesting and deserve publication so I encourage the authors to revise their manuscript.

I note that I did not correct the English as I am not a native English speaker.

Major points:

1) The preselection of the samples entered or excluded from age-depth modelling needs additional consideration. It is written in Table 1 that the sample collected at 63 cm was a fragment of a large wood. It was recently discussed that there is a notable risk related to the C-14 dating of large wood fragment because they can origin from reworked trunks so their radiocarbon age could predate the actual age of the hosting ice layer (Kern 2018). The C-14 age of sample at 63 cm shows the most pronounced old bias, so I think it is an example of the above discussed situation and there is a good reason to omit the obtained C-14 age from age-depth modelling.

Kern, Z. (2018) Dating cave ice deposits. In: Perşoiu, A. and Lauritzen, SE (eds) Ice Caves, Elsevier pp.109-122.

2) Comparing the new C-14 ages to an independent set of C-14 ages (Maggi et al., 2009) is principally a good idea. However, as fa as I know, the ice core of Maggi et al., 2009 was extracted in May 2004. I am pretty sure that the equal depth values below the ice surface in 2004 and in 2014 do not represent the same stratigraphic situation. There are two factors which should be considered: - Basal melting has been documented practically at each major cave ice block where it was monitored. Assuming a similar basal melting rate as reported from the nearby Scarisoara Ice Cave (1.54 cm/yr, Perşoiu 2005) $\sim$15.4 cm should be subtracted from the depth belonging to the C-14 ages of the samples collected in 2004 before the two set of ages are compared. - Neg-

ative mass balance at the ice surface over the past decades has been reported practically from all cave ice deposits in Europe. For instance, in the previously mentioned Scarisoara Ice Cave the complete loss of 20 century ice accumulation was assumed (Perşoiu et al., 2017). I think it is very likely that some degree of surface melting took place also in the Focul Viu Ice Cave during the past decades. The difference between the 2004 and 2014 ice levels should be quantified and it should be used to establish a corrected depth scale before the two set of ages are compared.

Perşoiu, A. (2005) Evidence of basal melting of the ice block from Scărişoara Ice Cave, in (Mavlyudov, B.R.,Ed.) Glacier Caves and Glacial Karst in High Mountains and Polar Regions, p. 109-112.

Perşoiu A, Onac BP, Wynn JG, Blaauw M, Ionita M, Hansson M. 2017. Holocene winter climate variability in Central and Eastern Europe. Scientific Reports 7(1):1196

3) As an additional consequence of the above mentioned surface melting (i.e. negative mass balance at the ice surface) the collection year should not assign to the top of the core during the age-depth modelling.

4) Finally a comment on the presentation and discussion of the model ages: Keeping in mind that the uncertainty of model ages ranges from $\sim$10 to $\sim$40 yrs (according to fig6) reporting dates with annual precision (e.g., P4 L14 "...the maximum age of the ice is ...1099 cal BP...", P3L17 "ice accumulation rate between AD851 and 947") is misleading. Please round the modeled dates to the nearest decade and refrain presenting the model dates with annual precision.

Minor comments:

Page2 Line7 Colucci et al., 2016 and Colucci & Guglielmin 2019 could be cited also to support this statement.

Colucci, R. R., Fontana, D., Forte, E., Potleca, M., Guglielmin, M. 2016 Response of ice caves to weather extremes in the southeastern Alps, Europe. Geomorphology 261,

1-11. doi:10.1016/j.geomorph.2016.02.017

Colucci, R.R., Guglielmin, M. 2019 Climate change and rapid ice melt: Suggestions from abrupt permafrost degradation and ice melting in an alpine ice cave. Progress in Physical Geography 43: 561-573 https://doi.org/10.1177/0309133319846056

Page2 Line8 The statement “In such caves, ice is deposited as layers of frozen water. . .” needs revision. This is not the only origin of ice in temperate climatic region. Mavlyudov 2018 can be recommended as a recent review on ice genesis and types of ice caves.

Mavlyudov, B 2018 Ice genesis and types of ice caves. In: Perşoiu, A. and Lauritzen, SE (eds) Ice Caves, Elsevier, pp.34-68.

P3L13 “mL” instead of “ml”

P3L15-18 This part is neither drilling nor stable isotope analysis. You should move this paragraph to another place or change the title of the subsection, please.

P3 L31-32 probably better to say that small carbon yield instead of small sample mass was the problem for AMS analysis

P5 L9 I think the citation in this line should be Nagavciuc et al, 2019b

P5 L39 The current statement is triviality. The AMO index is indeed strongly associated with the prevailing SST since it is defined based on the SST pattern. The sentence needs revision.

P5 L42 Probably “the” instead of “de”

P6 L9-10 Please consider replacing “at our site location” with the excat site information e.g.”in cave ice deposit in the FV ice cave” to avoid any potential confusion.

P7 L5 probably “maxima” instead of “maxim”

P7 L7-9 Cited references need some revision here. Moberg et al 2005 is not a sum-

mer temperature reconstruction; Seim et al., 2012 is again not a pertinent reference because there is no summer temperature reconstruction in the study. Finally, I've comment on the citation of Dragusin et al., 2014. The sentence lists warm decades, however the temporal resolution of speleothem record of Dragusin et al. 2014 is insufficient for the past 1000yrs to be able to see decadal warm peaks.

P7L13 "with" instead of "woth"

There are some easily correctable small mistakes or strange points in the reference list: -There are few places where the text is in red for a few characters (P10L6, P11L20) -P11L7 a space is missing between "and" and "predicting"

Figures and Tables

The figures are clear and illustrate well the paper however if Authors accept my suggestions on age-depth modelling then Fig2 should be modified. In the caption of Fig7 (P19 L5) probably "Northern Hemisphere" should be written instead of "Nordic Hemisphere"

Table1: First and foremost, the typo in the title should be corrected ("Table" instead of "Tabel").

-9th column: Please follow recommendations of Millard 2014 reporting calibrated ages and present all calibrated intervals with associated probability.

-10th column: The current header sounds strange. Please revise. In addition, the ±sign suggests that accompanied uncertainties are also presented in the cells but it is not mentioned neither in the title nor in the header. Finally, the header suggests mean model ages are presented. Why did you used mean age? According to my experience using median ages, which belong to the highest probability, is more usual.

Millard, A. R. (2014). Conventions for reporting radiocarbon determinations. Radiocarbon, 56(2), 555-559.

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