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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 Bădăluţă et al.

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Dear editor and reviewers,

On behalf of the authors, we thank you for the time spent on reading our article and for the constructive comments. In the attached document we present our point-by-point responses to the comments and the subsequent modifications of the text.

Response to reviewer #1 Comment 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: Persoiu, A. and Lauritzen, SE (eds) Ice Caves, Elsevier pp.109-122. Response 1: Indeed, dating cave ice deposits is extremely challenging, due to possible incorporation of young organic material in old ice (during melting) or viceversa, redeposition of old organic material in young ice. As correctly pointed out by the reviewer, this was the reason behind rejecting the sample at -63 cm.

Comment 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, Persoiu 2005) âĹij15.4 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. Negative 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 (Persoiu 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. Response 2: We thank the reviewer for this comment; we did ponder ourselves for long on the age of ice. We will discuss basal and surface melting separately. First, the depths along both cores ("Maggi core" and ours) were measured from the 2004 and 2016 ice surfaces, respectively (one of us - AP - was involved in both drilling efforts). We mention this, because the position of a certain horizon inside a cave glacier can be measured both against the ice surface at the time of drilling or against a fixed point on the cave's walls/ceiling (a situation not encountered in polar and most mountain glaciers). As such, if a horizon at a certain depth is measured several years later, its depth below the ice level will be affected by changes at the surface only (melting or accumulation), whereas the same depth below the cave's ceiling will be affected by changes at the surface and also by the general lowering of the ice block due to basal melting (lateral flow, although present for example in ScăriÈŹoara Ice Cave (Romania) and Dobsinska Ice Cave (Slovakia), has a very limited role on lowering the ice surface against a fixed point). Now, the ages reported by Valter et al (2009) were measured against the 2004 surface, whereas ours were measured against the 2016 surface. The surface of the ice block was affected by melting and accumulation processes between these two dates, but we have no means to quantify them (see also the response below). We can only make assumptions on how much ice melted or how much accumulated. Our data from ScăriÈŹoara (as pointed put by the reviewer) shows that all the ice that accumulated since ∼1860 AD melted away, most of this melting occurring between AD 1947 (when monthly measurements in the cave started) and ∼AD 1982 (this was due to changes in the cave's morphology, with ice levels being ± constant since). Contrary, tritium data from the nearby BorÈŻig Ice Cave (Kern et al., 2009) indicates no important melting since at least AD 1950. Summing up this information, we conclude: 1. Basal melting resulted in the lowering of horizons dated by Maggi et al (reported in 2009, drilled in 2004) compared to the cave ceiling, but did not result in changes between the depths measured by them and the surface of ice 2. Depth along the core drilled in 2016 (this study) were measured against the 2016 ice surface, and very likely this was not the same as the 2004 surface. Consequently, we agree that numerous assumptions would be required to combine the two chronologies. Based on our knowledge of cave ice processes and dynamics and assuming "normal" melting conditions between

2004 and 2016, we estimate that the depths reported by Maggi et al in 2009 should now be some 10-20 cm lower (for example, the depth of sample SO5-13 reported by Maggi et al at 213 cm below the 2004 surface would now be at ∼193 cm below the 2016 surface, exactly matching our depth-age model. The same would also be true for Maggi's sample SO9-5. The other samples by Maggi et al (wood samples) are already outside our depth-age model. Now, at this point we are facing two possibilities: 1) incorporate Maggi's ages in our depth-age model and thus improving it by increasing the number anchoring points and 2) use Maggi's ages to independently support or model ("all depth-age models are wrong. . ."). We tested the first possibility and the change in the modeled ages was between 0 and 30 years, thus within the limits of the dating uncertainty. Nevertheless, we did lose the independent verification of our model and therefore we decided to keep the depth-age model as is, except modification of the topmost age (see p. 3 below).

Comment 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. Response 3: We agree, up to a point. While we do not have detailed monitoring data from the cave, observations made during (almost) annual trips show that in some years melting affects more ice that has accumulated in the previous winters, while in others, less. Accordingly, it is difficult impossible to precisely establish the age of the upper (most recent) layers of ice (well, technically, before melting starts during the summer of a given year, the age of the uppermost layer of ice is exactly that of the year, being formed in the previous winter). Monthly measurements in the nearby ScăriÈŹoara Ice Cave (with pauses, since 1947) have shown that annual melting could result in the loss of up to 20-25 years worth of ice accumulation. Our observations have shown that the two caves (∼10 km apart, same elevation, same morphology of the cave and ice block) behave similarly in terms of response to external climate (qualitatively, but perhaps not quantitatively). Consequently, while assigning a modern age for the topmost layer of ice is wrong, it would be equally wrong to assign any other age in the absence of precise age determination. For simplicity (Occam's razor) we keep a modern age of this layer. However, the top age for the depth 0 cm is now modeled as a uniform distribution between 1991 and 2016 AD, allowing for a 25 years' time interval. This range is similar to that of the radiocarbon-based ages and it also reflects the uncertainty related to the melting – see above.

Comment 4) Finally a comment on the presentation and discussion of the model ages: Keeping in mind that the uncertainty of model ages ranges from âĹij10 to âĹij40 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. Response 4: We rounded all ages throughout the text to the nearest decade (and in some case, half-decade).

Comment: Page2 Line7 Colucci et al., 2016 and Colucci & Guglielmin 2019 could be cited also to support this statement. Response: The statement was actually discussing the influence of the AMO on climate in Europe, not specifically on cave ice. We have nevertheless added a sentence to the introduction " Studies of ice caves in southern Europe have also highlighted the sensitivity of cave glaciers to summer climatic conditions (Colucci and Guglielmin, 2019; Colucci et al., 2019".

Comment: 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. Response: Of course. We have changed the text and now it reads (see also the response to the first comment by rev 2): "In such caves, ice forms either by freezing of water or direct snow deposition in the entrance shafts (e.g., Mavlyudov, 2018). [. . .] Regardless of the deposition style, the ice records the original stable isotope composition of precipitation that further reflects changes in air temperature and thus is an important archive of past temperature and moisture source variability".

Comment: P3L13 "mL" instead of "ml". Response: Corrected.

Comment: 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. Response: We added a new paragraph entitled "3.3 Climate data" and moved the text of climate-related analytical methods there.

Comment: P3 L31-32 probably better to say that small carbon yield instead of small sample mass was the problem for AMS analysis. Response: Indeed, carbon yield sounds more proper here. Corrected.

Comment: P5 L9 I think the citation in this line should be Nagavciuc et al, 2019b Response: Yes. Corrected.

Comment: 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. Response: Yes. We have deleted the redundant part of the sentence and modified the text: "Further, we have computed the correlation map between the summer mean air temperature at SB station (with the longest instrumental record) and the summer SST as indicator of AMO variability (Sutton and Dong, 2012)."

Comment: P5 L42 Probably "the" instead of "de". Response: Corrected

Comment: 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. Response: Indeed. The text now reads "high d18O values in the FV ice core".

Comment: P7 L5 probably "maxima" instead of "maxim". Response: Corrected.

Comment: P7 L7-9 Cited references need some revision here. Moberg et al 2005 is not a summer 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. Response: Finding seasonally distinct climate proxies is always difficult and we thank for the suggestions of the reviewer. First, we have removed the reference to DrăguÈŹin et al. (2014), due to the very low temporal resolution of the record during the investigated period. Further, we have used the Moberg et al (2005) data as it summarizes both high resolution records biased towards the growing season (summer), and low-resolution ones that include several summer records (Moberg et al., 2005). However, not being specifically a summer temperature record, we have decided – as suggested by the reviewer – to eliminate it. Seim et al (2012) – while not specifically a temperature record, it is nevertheless the result of the combination of summer precipitation and temperature climatic conditions, i.e., summer droughts (Seim et al., 2012). Our present and previous work (Nagavciuc et al., 2019a, cited in the main text) shows that both Western Romania (where Focul Viu Ice Cave is located) and Albania (from where the Seim et al data originated) are influenced to the same extent by the AMO and summer droughts and thus we have used this record in our analysis. Figure 7 has been modified accordingly, by removing the DrăguÈŹin et al (2014) and Moberg et al (2005) records, but we have kept the Seim et al (2012) record and changed the caption to specify that it is a summer drought record.

Comment: P7L13 "with" instead of "woth". Response: Done.

Comment: 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". Response: Corrected.

Comment: The figures are clear and illustrate well the paper however if Authors accept my suggestions on age-depth modelling then Fig2 should be modified. Response: Please see our first comments on depth-age modeling.

Comment: In the caption of Fig7 (P19 L5) probably "Northern Hemisphere" should be

written instead of "Nordic Hemisphere". Response: Done.

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

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

Comment: -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. Response: Indeed, for interpretation of separate probability age distributions the median would be more suitable. However, the ages modelled by P_Sequence have all normal distribution. In fact, this assumption is made in the underlying statistical algorithm (Bonk Ramsey 2008, Appendix, p. A.2.3). As such, the modeled ages are calculated as means and standard deviations. We clarified it in the table header.

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