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Climate of the Past Discussions

Interactive comment on "Cryogenic cave carbonates in the Dolomites (Northern Italy): insights into Younger Dryas cooling and seasonal precipitation" *by* Gabriella Koltai et al.

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From carefully reading this very interesting study, it appears that the results provide important insights on the temporal changes in the amount and timing of snow fall at the study site during autumn and early winter during the YD. Koltai et al. estimate rather warm January temperatures of around -13.7°C for the YD and only a quite moderate change in seasonality of up to 5.7K relative to the AL. Based on these results, the authors "challenge the commonly held view of extreme YD seasonality" (e.g. line 15).

While the inference of changes in snow cover over time are important by itself, I do not agree that the results can challenge seasonality changes from other studies: The

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inference of snow-rich conditions and that such a snow cover insulates the cave cannot be used with much confidence to estimate the full severity of winter temperatures and hence can neither reconstruct nor challenge seasonality changes in a general way.

In the best case, the results may be valid for the local cave or regional climate setting. However, the authors do not provide evidence for why the results from a cave record at a high elevation from the Southern Alps can generally challenge commonly held views on extreme YD seasonality in other regions and, i.e., not north of the Alps across the Euro-Atlantic region.

As discussed by the authors, the timing and amount of snow cover over the cave has a major control on subsurface temperature changes. An early and/or thick snow cover will protect the cave from the most severe winter cooling like in January. It is therefore quite likely that the cave record fails to estimate the full winter cooling which would define the amplitude of seasonality change (this might also apply to other studies affected by ice or snow cover insulation). This would imply that the authors do not reconstruct the full seasonality. Hence, they cannot challenge seasonality results from other regions. It is unclear to me how assumptions in the thermal modelling can account for the combination of two unknowns: an unknown winter severity in the YD together with an unknown snow thickness. I think this should be clarified in the text.

Line 37: “Siberian-like” would imply extreme seasonality which is typical for continental climates. Such a climate is reasonable for the YD north of the Alps as the major heat source is shut off with an ice-covered North Atlantic Ocean. This does not need to apply to the Southern Alps, though.

Line 39: Remove “however” as the sentence before is about winter and this sentence is about summer.

Line 39-40: Replace “shutdown” with slowdown - the study assumes an AMOC slowdown of around 36% relative to the Alleröd. The 4.3 to 0.3 summer cooling refers to chironomid-based estimates and hence a notable summer cooling – the mild YD summers are based on plant indicator species and the climate model which suggest no overall summer cooling.

Line 49: The -10 K change in the northern Alps would be an indication that your record from the Southern Alps does not reflect the same changes – hence you cannot challenge large seasonality changes in general. If anything, your study reflects local or even only cave ambient temperatures/snow cover changes and additional information is required to claim these muted changes would generally apply to south of the Alps. Spagnolo & Ribolini (2019, see section 4.3) estimate a seasonality of 21 degrees for the maritime Alps at the ELA during the YD compared to 14.2 degrees today.

Lines 62-63: While it is true that Hepp et al. 2019 claim the opposite, several comments by E. Schefuss, B. Zollitschka as well as D. Sachse and myself (see the online discussion: https://cp.copernicus.org/articles/15/713/2019/cp-15-713-2019-discussion.html) raise serious questions regarding the validity of that study. The extensively studied Meerfelder Maar nearby does not agree at all with the interpretation using a much more reliable chronology. I think this disagreement needs to be at least mentioned here (e.g. relative to Brauer et al., 1999; Bakke et al., 2009).

Line 87: Again, there is no reason why the local cave record should "argue against strong winter cooling during the early YD" at other places and perhaps even not at the site itself.

345-350: This might be true for the local setting in the cave insulated by a snow cover but might fail to reconstruct the full MAAT cooling which happened in air temperatures above the snow cover.

Bakke J., Lie Ø., Heegaard E., Dokken T., Haug G. H., Birks H. H., Dulski P. and Nilsen T. (2009): Rapid oceanic and atmospheric changes during the Younger Dryas cold period. Nat Geosci 2, 202–205.

Brauer A., Endres C., Gunter C., Litt T., Stebich M. and Negendank J. (1999): High resolution sediment and vegetation responses to Younger Dryas climate change in varved lake sediments from Meerfelder Maar, Germany. QSR 18, 321–329.

Spagnolo, M and A. Ribolini (2019): Glacier extent and climate in the Maritime Alps during the Younger Dryas. Palaeogeography, Palaeoclimatology, Palaeoecology, 536: 109400, https://doi.org/10.1016/j.palaeo.2019.109400

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