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Discussions

Interactive comment on "First Investigation of Perennial Ice in Winter Wonderland Cave, Uinta Mountains, Utah, USA" *by* Jeffrey S. Munroe

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Over the past decade, investigations of perennial cave ice deposits took a more central stage, as the continuous rise in temperatures (and associated climatic changes) threaten these rather understudied components of the cryosphere. In this context, understanding the genesis and behavior of cave ice deposits could lead to the development of novel proxies of past climate variability that could add unique insights in past climate variability. Munroe attempts to do so by applying a range of investigation tools to several small perennial ice deposits in a cave in the Rocky Mountains, Utah and presenting and discussing the preliminary results. While the paleoclimatic potential of the investigated cave ice deposits is rather small, the results could advance our general knowledge of ice cave processes. While I had numerous comments, I never-

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Discussion paper

theless think that the manuscript could be published, given that 1) the paleoclimatic reconstruction is diminished in importance (see the comments on chronology) and 2) processes in the cave are emphasized as a tool for subsequent studies.

General comments One of the main problems of the paper is the chronology. It is not clear at all what was actually dated. My understanding is that the top 15 cm of ice were dated. Because of the uneven ablation of the cave surface this approach is extremely problematic when it is intended to be used for climate reconstructions. If I understood correctly, the surface of the ice is extremely uneven and samples were collected from different depths, measured ("estimated visually" – a rather unusual choice) against the nearest ridge. Sublimation and/or melting was definitely acting with different intensities over the surface of the ice, bot today and in the past, and as such, younger packrat droppings could have been incorporated in older ice and or older ones reworked and deposited in new ice. Based on these considerations, I think the only message that can be obtained from the 14C dating is that ice was present in the cave between the youngest and oldest dates. Anyway, sampling locations need to be marked on the cave map. A detailed sketch of the ice surface with the position of the dated samples against morphology (and stratigraphy, if available) will help us understand what was being done.

I am surprised that no attempt was made to collect and use bulk electromagnetic wave propagation velocities – these could have been used to peek into the composition of the ice (a method used by Hausmann & Behm, 2011, which is cited by the author). Is the data available and usable? Even if not "fantastic", it could further help subsequent studies (elsewhere). The point I want to make (here and through the review) is to have as much as possible methods descriptions and data available, rather than only "publishable" ones.

Specific comments The final paragraph of the introduction reads like being taken straight from a research proposal. Perhaps it should be rephrased to sound more article-like. Field site: it would benefit the readers to add one line about the characteristic of the limestone (rather than the name of the formation which would be in negligible interest). E.g., primary/secondary porosity and thickness of caprock are important to understand how water reaches the cave and/or how heat is being transferred to the cave. Also, for the understanding of ice chemistry, the general lithology of the rock should be presented.

Line 89: at what height above the ice were the loggers suspended and at what distance from the rock walls? These are important considerations for the understanding of cave meteorology and factors leading to ablation/formation of ice, as the both presence of ice and of air currents induce strong vertical thermal gradients.

Line 100: please detail the "variety of gain settings" that were used during the GPR data acquiring (later in the manuscript, values are mentioned) and discuss the choice of one over the other. This is important if this study is to be useful for other researchers.

Line 115-126: see my general comment. What calibration curve was used? The most recent is Raimer et al., 2020.

Line 129-132: please describe the stratigraphy of the exposure and the number of samples collected per layer as the results are later presented using the layering. Did you consider layering during sampling or cut across strata (as suggested by the 2 cm spacing)? As water formed by the freezing of water, fractionation would have resulted in different stable isotope values within the same layer of ice.

Line 134-136: please detail the location of the additional samples for stable isotope analyses.

Line 159: see my comment on the height of data loggers above cave ice surface.

Line 167: using freezing degree days is rather uncommon, some readers might think that the number of days was calculated, rather than the sum of degrees below 0 °C. Please define it in the text. Perhaps the fdd should be calculated for the cave data, as well.

Line 212-217: these should be moved under “methods”

Line 219-220: move to methods.

Line 220: “depleted” against. . .? Generally speaking, values cannot be depleted. A sample can be depleted in the heavy (light) isotope, resulting in a low (high) delta value.

Line 224-229: I do not understand the reason for removing “outliers”, the wide range of values is not an issue. This is a very unusual approach; I would consider all results, especially as the omitted results are almost similar to the ones used (e.g., fig. 7)

Line 245: relative to. . .?

Line 254-255: move to methods, and detail

Line 259: Ni – possible contamination of the upper layers?

Line 274-276: I do not understand the rationale behind the explanation for the chimney. If outside air gets below the internal one, cold air will flow inside the cave, regardless of the presence or absence of a chimney (perhaps references in lines 2776-277 should be updated). Form the data in gig. 2, I do not see the need for a secondary entrance. The morphology of the cave and the data clearly indicates a “cold air trap”, with dynamic cooling as cold air flows inside the cave and slow warming, perhaps triggered by geothermal heat (and additional heat brought to the cave by dripwater). This does not exclude the presence of a chimney, but if it was not observed during the visits, perhaps the simplest explanation would be sufficient (Occam’s razor).

Line 284-294: elegant

Line 292: did you notice airflow? Perhaps it is just warming propagating outwards though conduction from the warmer inner parts of the cave.

Line 295: what you mean by “back”? The longitudinal profile indicates flow from the entrance towards the “back” of the cave.

Line 299: again, was there a chimney/conduit observed? If not, water will just drip through the limestone's fissures.

Line 302: no need to flood the epikarst (it would be problematic at that altitude); in limestone, water will always find a way towards lower altitudes/caves.

Line 303: it would be useful to discuss the "ridges and troughs" on the surface of ice in detail. Inflow of warm water would definitely result in melting of ice, leading to the formation of surface micro topography. Further, the same water could bring in young packrat droppings and redeposit them in older ice, exposed during melting. This is evident in figure 10, where ridges are drowned in newly formed ice.

Line 304: not seeing a secondary entrance, only hypothesize one, I would consider the cave a "static cave with congelation ice" (Luetscher and Jeannin, 2004a)

Line 318: Is there possibly ice present under the breakdowns (in the "ice free" sections)? Perhaps a continuous layer of ice extends from the entrance through the cave and the breakdowns cover it in places. It would be otherwise difficult to understand the lack of ice in parts of the cave.

Line 324-326: the low reflectivity could indicate a thick and homogenous layer of clear ice, formed by the slow freezing of water in a through between ridges (so-called "lake ice"). And indeed, this ice would be free of cryogenic calcite and other sediments, that would settle at the bottom of the lake water during freezing. Here the usage of use bulk electromagnetic wave propagation velocities could help. Line 334: a photo and stratigraphic sketch would help understand the structure of the ice deposit and the stable isotope values

Line 342: depleted in heavy isotopes

Line 353 and subsequent paragraph: "freezing slopes" have been described as being generally below 7.2 (Jouzel and Souchez, 1982, Souchez and Jouzel, 1984, Souzhez et al., 2000, Persoiu et al., 2011). Layers 4, 5, 6, 10, 12 and 13 (but not 9 and 10, which have slopes below 7) likely formed as thin layers of water froze on top of the existing ice block (slope>7). This could be checked by plotting d-exces vs. d2H (Souzchez et al., 2000) for every layer. Lack of correlation between the two parameters would indicate kinetic conditions and thus open-system freezing (thin layers of water freezing on top of existing ice). Alternatively, all layers could have formed as "lake ice" and subsequently part of them melted away thus resulting in the loss of the alignment along a line with a slope below 7.

Line 363: technically, it was the isotopologues that were fractionated. . .

Line 364-366: because during freezing of a pool of water the samples align one a straight line in a dH-δ18O diagram (with r2>0.9), potential loss of top and/or bottom samples would not affect to much the slope of the line.

Line 371-372: regardless of type of freezing, fractionation occurs. It is the type of freezing and fractionation that matters. In layers of ice formed by the freezing of thin films of water, all water freezes "at once" and as such the stable isotope composition of the resulting ice is similar to that of the water. In the case of freezing of a pool of water, fractionation and continuous incorporation of heavy isotopes in ice would result in stable isotope trend from top to the bottom. If the entire layer is sampled, the stable isotope composition of ice is similar to that of the parent water; but if samples are collected at various depths, their stable isotope composition would differ (higher values at top, lower at the bottom). Further, partial melting would result in loss of usually top layers, enriched in heavy isotopes, so that the stable isotope values of the ice would have little resemblance of that of original water. Elegantly, and nicely employed here, intersecting the LMWL with the slope of samples from every such layer would result in the original water before freezing (regardless of ice loss, freezing intensity, fractionation factors etc). This intersection should be applied to layers with low values of the slope (2, 7, 8, 9, 11 and bottom) and one single data point per layer should be than plotted in fig. 8 (assuming thus that each of these layers formed from one single pool of water – as for example, the one in Fig. 10d). However, this would alter the discussion in lines

374-384.

Line 380: I believe it is assumed here that the delta values reflect long-term annual means, in which case the difference is indeed large, pointing towards extreme climate shifts. Alternatively, it could be that they reflect different recharge patterns (e.g., winter vs. summer recharge).

Lines 379-384: in the absence of age control, I would refrain form discussing LGM-old ice. Further, it would be difficult to explain the survival of ice in a small cave, with such a dynamic ice accumulation/ablation processes.

Lines 396-404: this could be somewhat shortened to a line, not to break the stable isotope discussion.

Lines 404-412: perhaps a lengthier discussion of stable isotope variability in winter vs. summer could be included, with data from other stations nearby. Two data points are not enough to sustain the subsequent modeling. Further, if OIPC is used for summer, it should also be used for winter (especially as the OIPC-derived data for winter is depleted in 18O by about 3 ‰ compared to measured values). So, either used OIPC only, or data from stations, but not a combination of the two. I understand that this would affect the modeling in lines 412-420, but it is more correct.

Lines 422-423: I still cannot see the reconstructed values.

Lines 422-434: see my comment above and perhaps redo the calculations (using one value for the layers formed as "lake ice")

Lines 435-442: I find the discussion in this paragraph difficult to sustain by the data. Given the complex morphology of the ice surface and the lack of stratigraphic sketches, it is difficult to follow.

Lines 444-456: in the absence of chronology, this section is very speculative and could be safely left out.

“Glaciochemistry interpretation” should be moved before paleoclimate and “indication of recent change” after the climate data. However, in the absence of chronology, the entire discussion of the potential sources of variability in the chemistry of ice is speculative. As above, it could be reduced to a few lines, detailing the layer-by-layer variability, rather than potential temporal variability. For instance, are there differences between the two types of ice (lake vs. floor) in terms of chemistry? This could be more helpful for subsequent studies of cave ice deposits with better age control.

Line 469: see my comments on chronology. Ages should not be employed when discussing the layers, as 1) it was only the surface that was dated and 2) complex surface morphology, with deep ridges.

“Limitations and direction for future research” and “conclusions” These two chapters should me merged and emphasize should be put on the potential usage of data obtained from this case in the general understanding of cave ice processes.

The first panel in figure 2 is slightly misleading du to the inverted scale, please use a normal one (and perhaps write the “warmer outside, warmer in the cave” text with the same color as that of the line.

Souchez, R., Jouzel, J., Lorrain, R., Sleewaegen, S., Stiévenard, M., and Verbeke, V.: A kinetic isotope effect during ice formation by water freezing, Geophysical Research Letters, 27, 1923-1926, 10.1029/2000GL006103, 2000.

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