The Cryosphere Discuss., 4, C1239–C1244, 2010
www.the-cryosphere-discuss.net/4/C1239/2010/

The Cryosphere
Discussions

Interactive comment on "Glaciochemical investigations on the subterranean ice deposit of Vukušić Ice Cave, Velebit Mountain, Croatia" *by* Z. Kern et al.

M. Citterio (Referee)

mcit@geus.dk

Received and published: 2 December 2010

Overview

This manuscript presents a wealth of analytical data from an ice core drilled in a cave ice deposit, including chemistry, stable O and H isotopes, tritium activity and radiocarbon ages, and attempts to: 1) relate these data to the corresponding regional values for the surface environment; 2) argue that the selected cave ice deposit is suitable for paleoenvironmental studies.

General comments

Despite the short length of the core (2.4 m), the ice core dataset is among the most comprehensive to date within the field of cave glaciology, which has just begun to develop in the last few years. This, together with achieving points 1 and 2 above, would have made for a very relevant and important contribution. However, the interpretations and conclusions are both unsupported by data and flawed by some misconceptions (see "detailed comments" below).

I think the selection of the study site was inappropriate, as very little seems to be known about this cave and its ice deposit from previous investigations, and very little is provided by this manuscript apart from the ice core dataset alone. No background knowledge on the cave climate, on the processes and seasonality of ice accumulation and ablation, on the stratigraphy of the ice deposit and on the fluctuations of the ice mass over time is available. Unfortunately this also reduces the usefulness of the ice core dataset. Finally some very important ions have not been measured, or at least they are not in the present manuscript and supplement data.

My advice is for this manuscript to be either rejected, or withdrawn pending the collection of the missing basic information on the cave and the ice deposit that would enable a proper interpretation of the ice core record and a much stronger future manuscript.

Detailed comments

p.1562 l.25: Citterio et al 2007 has no information of the Appenines, only on the Alps, please correct.

p.1563 l.3-7: These sentences can be misleading to a majority of TC readers who may not be familiar with cave climate. Air temperature in caves does not track the surface climate in a simple way. The most obvious effect is the delay and damping of the surface temperature signal due to the thermal inertia of the rockmass. However what is most important here is that the underground air circulation (the very reason perennial cave ice can exist at much lower elevations than the regional glacier equilibrium line) also makes cave ice respond in a different way to surface climate, so making comparison of cave ice and surface glaciers is not straightforward. Typical cave ice deposits are found either at the bottom of single-entrance descending caves ("cold traps") or close to the lower entrance of multi-entrance caves or cave systems. In both settings, the hypogean air circulation makes for colder cave temperatures then the mean annual air temperature at the same elevation outside the cave. For cold traps in particular, the cave temperature is mostly dependent on winter surface ice temperatures and is largely unaffected by summer air temperatures, which instead affect glacier mass balance the most.

p.1563 l. 17- p. 1564 l.16: In this section there are two fundamental misconceptions that are reiterated and come into play later and undermine most of the discussion and conclusion sections: 1) while cave ice may be impermeable to surface meltwater (and even this could not be entirely true, as I have observed many times that hydraulically connected water films do form at crystal boundaries, at least in coarse grained cave lake ice) the water that reaches inside a cave and freezes can not be assumed to have the same composition of the meteoric water. This is one reason cold snow and firn from high altitude Alpine glaciers are so much more desirable. The manuscript doesn't take this problem into any consideration. There is no mention of what kind and how extensive the soil cover is in the cave surroundings, no information on groundwater, on the amount and seasonality of infiltration and dripping in the cave, and so on. Indeed, the high Ca and Mg reported later on demonstrate just how large the contamination from the carbonatic environment can be. Many other ions discussed throughout the manuscript may be either enriched or depleted by a soil horizon above the cave.

2) the other reason why this cave ice can not be assumed to be of similar use (and by no means can be interpreted along the same lines) as cold Alpine firn is that there seem to be no knowledge of the stratigraphy of the ice deposit. Firn layers high in the accumulation zone of a glacier may be expected to be reasonably continuous laterally, while cave ice of the type described here is typically very discontinuous laterally, because it forms from the freezing of thin water films flowing along the steepest gradient

on the previous year's ice and typically resulting in lanses and "onion like" structures. The only type of cave ice that could be suitable is lake ice, where the original layer surface is known to have been flat and level. Here no information of the ice structure and stratigraphy is provided, not even generic descriptive terms. This makes very problematic to attach any stratigraphic or temporal meaning to depth below the surface, and consequently to interpret the ice core data in any meaningful way.

p.1563 l.9: this means that there is no knowledge of the seasonality of water infiltration and of water freezing and ice melting.

l. 10-13 the description does not match the field survey in Fig. 2, as in the figure it is clear that the ice in 1962 was not in contact with the rock on one side (and it wasn't in 2008 as well). The only reason for this can be active air circulation, as this gap between rock and ice is otherwise in the lowest part of the cave and most removed from the surface, which should be the coldest part and the favourite place for water to flow and freeze. Therefore I do not believe this to be a static cave as stated in the manuscript.

l. 14-15: here is another misconception: even assuming the 1962 survey of the ice surface was really this accurate (also, in which month was the 1962 survey carried out?), there is no information of the seasonal variability of the ice surface, so the -20 cm 1962-2007 and further -6 cm 2008-late autumn 2008 are very impossible to interpret. The deposit may have fluctuated by much more than those 20 cm since 1962 and there would still be no stratigraphic evidence that the ice exposed in 2008 at a lower level then the 1962 surface is indeed older. It may well have melted one meter below the 1962 level and then accumulated again, with no way to tell until some stratigraphic work is done. As a bare minimum, is there any visual stratigraphy info from the ice core? When several such fluctuations could have occurred, the tiny evidences of the tritium activity are too little to support any further conclusion

p. 1565: the cleaning of the core should preferably be performed in the lab, unless transporting the ice frozen is unfeasible. Here it should have been cleaned again once arrived in the lab to provide the best assurance.

p. 1566-1568: shorten these

p. 1567: it's a pity that some major ions have not been measured: at least Na+, K+, Cl-, NH4+, SO4– should have been included to allow better interpretation of the trace elements. E.g. are the enriched U samples associated to higher clay silicates or to organic matter? K may have helped, as the association with Al values suggests contrasting evidences here (could it be that the filtering of the water altered some results?). Na+, Cl- and SO4– would have allowed better analysis of the marine signal, and so on. If these data are available but were not included here, please consider including them in any future re-elaboration of this manuscript.

p. 1570 l. 8-16: This is all very much speculative and not really supported by data, furthermore any consideration based on the 6 cm/yr (mm/yr?) is very shaky as this loss rate is based on nothing more than the two points in time 1962 and 2007, and everything may have happened in between (see my other comment above).

l. 22-25: this is in direct contradiction with the introduction where it is claimed that mixing is not an issue as it does not occur in ice from these ice deposits.

l. 26-28: unfortunately these radiocarbon dates are pretty inconclusive

p. 1572 l. 14: what elevation is Ledena Pit?

p. 1573 l. 15-16: Ca and Mg are not trace elements in the carbonatic environment, and the reference is both useless and misplaced.

p. 1574 l.6: yes, the very high Ca (and Mg) contents are indeed a clear signal from the local carbonatic bedrock, and both the finding of elevated values in cave ice and the very same interpretation suggested here have previously been published in Citterio et al. 2004b, so please fix this entire sentence.

p. 1574 l. 8- : these affinity associations would require much better discussion. I understand these seem to be the empirical evidences, but somewhat unusual findings such as Ti, B and U behaving as a chalcophile elements deserve some discussion. Also, even though I believe most carbonate-hosted sulphide mineralizations in the region are Triassic, small concentrations of sulphides can normally be associated with most carbonates, so Fe, Cu and Zn and other metals may also have a very local natural origin. Pb would also have been a useful measurement but I don't find it in the supplement data.

p.1575 : as stated above most of these conclusions are not adequately supported by data and proper interpretation.

Tab. 1 : Please note that Colle Gnifetti core is much higher elevation so chemical content is also different.

Fig. 2: please add some detail topography of the cave surroundings

Fig. 5 is not necessary

Fig. 7 can be both ovearlaid on a single plot. These are remarkable oscillations, it may deserve closer look.

Interactive comment on The Cryosphere Discuss., 4, 1561, 2010.