The Cryosphere Discuss., 4, C1035–C1039, 2010
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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.

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Received and published: 3 November 2010

General assessment

Obviously, perennial ice bodies exist in caves even situated in regions with rather temperate climates. Exploring to what extent such frozen water archives might hold past environmental and climate information is an open question, yet. In this context, the present paper dealing mainly with glaciochemistry (as following the title) would address an interesting issue. Especially since it reports an extensive set of major as well as minor trace elements comprising a data set which is almost absent in the cave ice literature. However, I cannot recommend publication of the MS in its present form

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mainly due to the following deficits: 1. the purpose and objective of the study are not stated 2. the information on the setting of the investigated ice (cave) is largely unsatisfactory 3. the presentation of the ice data does not allow the reader to assess their significance 4. there are various misconceptions in the data interpretation and, most important 5. the presented material does not really support the overall results and conclusions, respectively

Rationale

To point 1): Information on the chemical (and isotope properties) may either be used to infer processes which are related to the formation and dynamics of such ice bodies or to assess the environmental and climate significance of the archived signals. However, the introduction does not provide the purpose (or any main objective) of the study. Instead the authors give here the a priori notation that: They (ice deposits)... "offer a unique alternative to decipher comparable palaeoenvironmental information (e.g. air pollution history) like Alpine glaciers" - which is by no means warranted at this stage.

To point 2): The reader is lost since almost no setting of the cave environment relevant for the impurity sources and the expected governing ice formation processes are given (except, some scattered indications coming lately at the end of the paper). At least basic information on the sampled ice block (including a glaciological and stratigraphical characterisation) is essential here. For example, I wonder how (wind blown) snow and soil dust being trapped in the ice covered cave section might have influenced the results.

To point 3): Supporting data which are needed to characterize the sampled ice core sequence remained largely inconclusive or blurred: (a) given radiocarbon date but not addressed at all, (b) lack of a statement, at least, on the assumed, characteristic time resolution of the depth profiles (estimated from the two tritium tie points and outstanding stable water isotope cycles) (c) deceiving discussion of the δD- δ18O co- variability

The reliability of the minor trace element data is unclear: (a) What are the contribution

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of blanks, associated uncertainties and detection limits related to the whole analytical procedure (not just the ICP-MS one). In this context, the authors should be alarmed by the relatively high Zn level (sensitive to contamination), the generally high variability and the frequent disproportion among crustal elements (except Ca, Mg). Moreover, the operational defined restriction to the "non-filterable" fraction (at neutral conditions) is not addressed.

The comparison with other trace element data (precipitation, Alpine firn) is largely incomplete and not representative (refer among others to: Barbante et al. J. Anal. At. Spectrom., 1999, 14, 1433–1438; P. Gabrielli et al. Atmos. Chem. Phys. Discuss., 6, 8781–8815, 2006 and various precipitation networks). Moreover, the formal PCA results appears to be strongly biased by the outstanding "dust horizons" at around 1.2-1.7m and the overall trends within the PC2 (Zn, Cu, Cr) group.

To point 4: There is no observational evidence presented warranting the statements that, among others: (a) changes of non local bedrock species are related to atmospheric ones, (b) long term Zn and Cu agree with respective deposition trends (c) the level of various trace species are consistent in with those observed at other sites.

Suggestion for major revisions

Presently the work does not constitute a self-contained contribution in its field. Unless no substantial and detailed discussion material would be added, I recommend to produce a concise "Short Communication" which focuses on the trace element results (the only new ones) including a thorough comparison with the literature. Where needed, this version may be supported by the relevant findings from the (ice) isotope data.

Specific remarks related to data presentation

1. Reduce the given numbers to their significant digits, particularly in Table 1. In view of the strongly skewed data distribution the arithmetic mean and SD presented in Table

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1 are not appropriate entries. Add some information from descriptive data statistics (e.g median, quantiles, min./max, etc.).

2. indicate meaning of grey areas in figure 1

3. Revise figure 2 as providing a more useful picture on the cave topography and the immediate environment of the sampled ice block (among others, indicate the vertical scale and address the displayed contour lines in the present sketch)

4. Skip the redundant figures where δ18O as well as δD depth (time) series are shown. As both data sets refer basically to meteoric water they are always closely correlated, thus almost no additional (visual) information is provided by figures displaying the variability of both isotopes.

5. Figure 5 illustrating the formal 14C date calibration is obsolete here.

6. The same holds true for figure 6 which information is completely contained in the δ D versus δ18O regression parameters. If really needed, the co-isotope information might be illustrated instead, by the D-excess depth profile plotted along with one of the isotope species shown in figure 9 (preferentially plotted here as histogram with the blurred smoothing line skipped unless specified).

7. In illustrating the depth variability of selected trace elements, the logarithmic concentration scale deployed in figure 9 is not useful. Display the data on a linear scale e. g. normalized to the overall mean (again, preferentially as histograms).

8. There is an obvious imbalance between the amount of external (supplementary) data shown in figures to the ice related ones. Thus, strictly reduce these figures to those really needed (a good deal of relevant findings displayed in the external material may be certainly replaced by a concise sentence).

9. In a relative sense, the description of the instrumental analytical procedures are too detailed and not needed in that extent here: 3H, δ18O, δD on water and 14C on wood samples are standard anyhow deserving not much more to report than respective

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instrumental detection limits and/or precisions.

10. The presented Supplementary Material made up by an uncommented exel file (even lacking units) is anything else than a useful addendum.

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