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Interactive Comment

Interactive comment on "Stable isotope records for the last 10 000 years from Okshola cave (Fauske, northern Norway), and regional comparisons" *by* H. Linge et al.

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This paper is a worthwhile attempt to synthesize stable isotope data from nine Holocene stalagmites from caves in Scandinavia and I applaud the authors for presenting also some discomforting aspects of the data openly.

The paper is well written but some aspects require further attention:

P.1766, l. 6. The statement that speleothem deposition in caves of high latitudes/altitudes is only possible during periods devoid of glaciers and permafrost is not entirely true. The authors are reminded of Castleguard Cave, and our own work in Spannagel Cave in the Austrian Alps has also shown that this statement should be re-considered. Spannagel Cave, by the way, is geologically and hydrologically comparable some of the Norwegian marble caves.

p. 1768, l.27 and 28. Would not trust a temperature precision of 0.00X degrees C.

p.1773, l.23. The authors obtained 14 U/Th dates of a hitherto unpublished stalagmite (Oks82), but decided to dismiss seven of these dates. Looking at the proposed age model (Fig. 2) I have some serious reservations about these data. This data set, even if using 232Th-corrected data, involves large uncertainties and I am surprised that the authors still went ahead and used these data. The authors state (p. 1777, l.1) that Oks82 was active when sampling, but their depth-age model (Fig.2) stops at about 5ka. Personally, I would consider the age uncertainties as too large to be useful for a paleoclimatological study.

p.1775, l.25 etc. The stable isotope data of the stalagmites strongly imply kinetic fractionation effects, but the authors – quite surprisingly – declare that the cause of these effects are "not obvious“ and chose to treat the speleothems as deposited in "quasi-isotopic equilibrium“. This is clearly a weak point in the paper. Both of their arguments are not convincing: even in cold caves evaporation can play a role, in particular if the cave is/was ventilated. "High humidity“ (l. 26) is a vague expression; we found gypsum forming at ca. 97 percent relative humidity at 1.8 degrees C. The second argument - low pCO2 values in the drip water (are there actual data from these sites?) suggesting slow degassing – should also not be generalized. E.g., a drip site beneath a small doline with rather thick soil (or even peat) can show a different pCO2 and, alternatively, very slow drip rates can also give rise to prolonged degassing. What would be very useful in thus regard is a detailed comparison of modern calcite data (from actively growing stals or glass-plate precipitates) and their drip water. Table 2 only provides paired calcite-water isotope data for one of the five sites (Okshola) and their the measured modern calcite d18O value is between 0.5 and more than 1 permil higher than the calculated one based on equilibrium considerations (Friedman & O′Neil 1977 vs. Kim & O′Neil 1997). As it stands, the reader has to decide herself/himself if she/he

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believes in "quasi-isotopic equilibrium“ or not, which has serious consequences for the subsequent climate interpretation. p.1783, l.25. The authors explain the non-uniform start and end points of individual stalagmites by a "lesser robust "water seepage-calcite precipitation“ system“. Can the authors try to explain this in a bit more detail? How can such a system "break down“ (l. 25/26)? Are they invoking prior calcite ppt?

I fully concur with the authors that the next steps must be the improvement of the current age models and of the modern climate-proxy "calibration“ (p.1784, l. 26 and p. 1784, l. 22).

Interactive comment on Clim. Past Discuss., 5, 1763, 2009.