Clim. Past Discuss., 10, C1796–C1800, 2014
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Climate of the Past Discussions

Interactive comment on "Technical Note: How accurate can stalagmite formation temperatures be determined using vapour bubble radius measurements in fluid inclusions?" *by* F. Spadin et al.

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Received and published: 29 October 2014

The paper presents an interesting and, in my opinion, technically sound approach to further improvement of the microthermometry on the low-temperature fluid inclusions and, by extension, of paleotemperature reconstructions based on speleothems. Having said that, I should also say that I have a number of technical questions related to this manuscript. Specific comments are keyed into the annotated text. Here I will only address what I perceive as major issues.

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Interactive Discussion

Discussion Paper

* Technical aspects

The paper is conceived as a technical note, purportedly addressing the accuracy of fluid inclusion-based measurement of formation temperatures of stalagmites. The paper, however, suffers from a somewhat loose usage of the relevant terms, such as accuracy, precision, reproducibility, error, etc. I have an impression that in the text these terms are sometimes assigned (implicitly) meanings that are different from the generally accepted ones. As one example, accuracy and precision are used interchangeably in the Discussion and Conclusions (p. 3703, l. 25; p. 3704, l. 5), which is not appropriate.

Quantitative measures are mostly undefined in the text, and it is not clear what metrics are used (variance, standard deviation, standard error?) and at what level of confidence.

My general recommendation is to use consistantly the accepted concepts and terminology and to always explicitly state the metric used and (if applicable) the confidence level of the quantitative estimates.

* Assumptions leading to accuracy estimation

According to definition of the accuracy, in order to assess it, the measured values need to be compared with the true values (a.k.a. the accepted reference values). In other words, if true value is not known, accuracy cannot be ascertained.

For this specific paper, in order to claim accuracy of the temperatures obtained using the method proposed by the authors, these temperatures must be compared with the "true" cave temperatures that existed during the growth of the part of a stalagmite that was studied. In the paper this is done through a series of assumptions, each of which is associated with uncertainty. One problem is that most of the assumptions are implicit (not adequately described and discussed), so that the associate uncertainty remains "hidden".

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Assumption 1: The modern-day temperature in the cave, obtained through 1 year-long monitoring, is assumed to correspond to the mean annual temperature (MAT) on the surface. This assumption is supported by references to general publications on cave climatology (McDermott, 2004; Fairchild et al., 2006). Although in many cases the cave T, indeed, corresponds to MAT, it is not ALWAYS the case, and significant discrepancies between the two values have been reported. This assumption must be supported by site-specific data (compare measured cave T's with independently derived MAT's from the area).

Assumption 2: Historic MAT's for the last 350 years near the Milandre Cave area can be used as a benchmark for comparing the speleothem-derived temperatures. This assumption relies critically on the Assumption 1. But there are several additional problems here.

Firstly, what is assumed by the authors to be the MAT from "the vicinity of Milandre cave" (p. 3699, l. 18; p. 3700, l. 14 and l. 24 for example) has little to do with the local area of the study. The temperature reconstructions extracted by the authors from Luterbacher et al. (2004) are averages for an area of ca. 15 million square km, stretching from Iceland to Syria and from northern Sweden to southern Spain. Portraying mean temperatures obtained from such a vast territory as representing mean annual temperature "in the vicinity of the Milandre Cave" is clearly inappropriate, if not misleading. As a minimum, the authors must present a convincing arguments as to why they believe the MAT averaged over the Europe can be attributed to one specific location in the Europe (with very small assumed uncertainty of tenths of a degree).

Secondly, the original paper of Luterbacher et al. (2004) reported the temperature anomalies (i.e., relative values). The latter were converted by the authors into absolute temperatures. Methodology of the conversion is not presented in the paper, so there is no way of assessing the reliability of the derived temperatures.

Assumption 3. Calcite containing studied fluid inclusions grew over the last 350 years;

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therefore it can be compared with the historical MAT's for this time period (discussed above). This assumption is poorly supported in the paper.

Out of the two studied stalagmites, one (M1) has no associated geochronological data. Its growth rate is simply assumed to be similar to the second stalagmite, M2, on the basis of similar growth conditions (drip rate, drip height, T, etc.). In my opinion, this similarity does not represent a sufficiently robust basis for the assumption.

For this second stalagmite, M2, the growth model was purportedly established (Schassmann, 2010; this is a Master Thesis, which means it is difficult to access). The growth model is not presented in the paper. All we are told is that the M2 had an average growth rate of approximately 0.02 mm per year and that the growth model "relies on U-Th dating in the lower part of the stalagmite and assumes a constant growth rate in the upper part" (p. 3695, l. 5-7). A number of questions arise here, the most relevant ones being: can the growth rates established in one part of the stalagmite be simply propagated throughout the stalagmite, and how reliable is the age estimates derived through extrapolation of the growth rates? (One must recall that at the assumed growth rate the stalagmite M2 must have been growing for 13.5 thousand years, and M1 – for 18.5 thousand years).

Summary: the growth model purportedly exists for one stalagmite, but it is not available to a reader. The model is based on the U-Th dates from the lower (older) part of the stalagmite. Arguments why the determined growth rates must be constant (which means the age of the outer layers of the stalagmite can be determined by extrapolation) are not presented. The same growth rates are assumed to be valid for the second stalagmite, but basis of this assumption is poor (the presumed similarity of growth conditions). Under such circumstances the opinion of the authors about the growth period of the studied part of the stalagmites (assumed to be 0 to 350 years, as far as I understand; cf. p. 3700, l. 23-24) seems to be highly uncertain.

To summarize this discussion, it is my opinion that the uncertainty with establishing

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the “true” cave temperature to which the fluid inclusion measurements must be benchmarked in order to assess the accuracy of measurements is so great, that any meaningful quantitative assessment of the accuracy is simply not possible. On another hand, the precision of the method can be assessed, although the authors must present the statistical analysis of their measurements more carefully so as not to over-report the results.

I would like to encourage the authors to take into consideration these comments and re-submit their paper which, I believe, gas a great potential.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/10/C1796/2014/cpd-10-C1796-2014-supplement.pdf

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Interactive comment on Clim. Past Discuss., 10, 3689, 2014.