The Cryosphere Discuss., 9, C2266–C2273, 2015
www.the-cryosphere-discuss.net/9/C2266/2015/

The Cryosphere Discussions

TCD

9, C2266–C2273, 2015

Interactive comment on "Analyzing airflow in static ice caves by using the calcFLOW method" *by* C. Meyer et al.

Anonymous Referee #1

Received and published: 16 November 2015

Interactive comment "Analyzing airflow in static ice caves by using the calcFLOW method" by C. Meyer et al. (MS No.: tc-2015-145)

General comments: The manuscript introduces a method to quantify air flow in caves, which is based on statistically analyzing measurements of air temperature in Schellenberger ice cave. The approach is based on employing a regression method fitting time lags, damping and scaling parameters to pairwise temperature records representing thermal disturbances traveling through different cave sections forced by certain outside conditions. The basic idea to interpret phase shifts in temperature signals as measures of flow speed is not new. But it is interesting and valuable to put it forward in a formal context now, because air temperature measurements are nowadays increasingly available from many caves. However, a thorough exploration of their scientific

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

potential is largely pending. This is particularly true regarding cave circulation, which so far can hardly be tackled by direct measurements due to inevitable logistic and instrumental constraints in cave environments. This work will therefore receive principal attention by the cave community as well as by the snow/ice community because the method is applied for an ice cave hosting substantial subsurface ice bodies. The environmental conditions and processes that determine the existence of such subsurface ice masses are rather unexplored in which context a better knowledge of the air circulation is crucial, too. The background and the basic method are adequately documented, as well as the description of the used data. However, the detailed documentation of the application of the proposed method to measured data and the interpretation of the presented results need some reconsideration. Basic constraints of the approach were properly noted like restriction to particular types of ice caves and circulation pattern therein, assumption of stationary flow conditions, insufficient time resolution of the used data or uncertainties whether sensors catches the same branch of air flow. Their potential impact on the results is overall properly addressed as well. Unfortunately, the finally given numbers on the calculated travel time of air masses in the cave can hardly be reproduced, which is somewhat disappointing in view of the title. Moreover, the provided "internal" validation of those figures appears rather insufficient and the reader is thus left with substantial doubt in what extent the method can be considered as a reliable tool to quantitatively determine air velocity in (ice) caves. This had to be better proven before applications elsewhere as are announced by the authors on the other hand. The concluding remarks on the flow conditions in the investigated cave remain quite general and do not yet foster a better and process oriented understanding of air circulation in Schellenberger Eishöhle compared to what was published by the authors earlier and without use of the method (Meyer et al. 2014). The structure of the paper may be improved e.g. by merging basic information on the topography and data in the beginning instead of distributing that over the remaining paper. Content of section Results and Discussions may be better separated.

Specific comments: p5293, references: I think that a somewhat extended overview on the state of art of related work is appropriate. I here just point to e.g. Smithson 1991, or more recent work by e.g. deFreitas and Littlejohn 1987, Pflitsch and Piasecki 2003, Morard et al. 2010, Schöner et al. 2011 etc.

p5293 l27 to p5294 l5: sentences referring to Racovitza, 1975 are partly redundant and unclear, consider revision

p5294: because of their importance for this study, the nature of static ice caves may already here be described in more detail as well as the definition of open periods. I see that such is provided in section 2 (model), but suggest to merge the piece wise distributed information in a section "sites and data" which could also be the place to readily introduce the reader to the obviously important topographical features of the considered cave environment (which are currently addressed in Section 3).

p5295 l4-5: reconsider the wording related to "gravitational" as is used in those two consecutive sentences. Once you state that there is no gravitational transport possible and in the next sentence you refer to gravitational layering.

p5295 l19: "specific colder air" may be reformulated

p5295 l17: this sentence suggests that the open phase is essentially limited to negative external temperatures, which according to my understanding is not true in general.

p5296 l1-2: reconsider the sentence "... the gravitational layering ... is replaced by a positive correlation between air temperature an distance...", whose current formulation is not logic. The message of this sentence is reasonable, but unfortunately e.g. the mentioned "inversion of the temperature gradient" can not be reproduced by e.g. Fig. 7 in Meyer et al. (2014), nor the later mentioned restore of the gravitational layering as soon as outside temperatures rise above those inside the cave. Similar regards (p5296 l9 (.."expected temperature bias with inverted sign"). Watching Fig. 5-7 in Meyer et al. (2014) leaves the impression that things are overall more complex than stated here. For example Wasserstelle appears consistently warmer than Mörkdome which contradicts

TCD
9, C2266–C2273, 2015

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

plain "gravitational layering" as is proposed here (assuming that Wasserstelle being located lower than Mörkdome as far as can be judged from Fig. 3 and Fig. 4 in Meyer et al. 2014). I generally miss mentioning potential dynamic effects, Wasserstelle being located in a kind of narrow passage compared to Angermeyer Halle and Mörkdome representing dome like topographical settings. In my view this must have implications on the flow regime and is also relevant regarding assumptions and interpretation of your analyses (velocity constant?), Please clarify. Another issue which is not mentioned at all concerns the indications that the cave is most probably not "closed" below Mörkdome, but has connections with a larger cave system (remarks in Meyer et al. 2014). This would mean that Schellenberger ice cave can not be truly considered as a closed system but experiences some dynamic effects which tentatively will have effects on the ventilation regime and hence on the observed temperatures, too.

p5298 l17: add here that delta(t) is the measure to calculate the ultimately interesting speed of the air flow between the logger positions

p5297 equ.4: may be I am wrong with respect to the second term, which I expected to refer to TB(t) and where I can't see the point of a derivative with respect to b*, which according to p5296 l24 and equ. 3 should be constant. Please clarify.

p5298 equ. 5 and l4: shouldn't "s" be denoted "ŝ" (optimized parameter)?

p5299: consider shifting the somewhat lonely Fig. 1 and 2 into Sect 3 (application to data, keyword sensitivity studies?)

Fig. 3: This map is rather bad. If really no better one is available to show the regional topographical features (which are of interest here), at least please mark the position of Schellenberger ice cave more clearly.

Fig. 4: the indicated scale 1:200 is correct / useful ? Please insert a length scale which allows to judge the distances between e.g. loggers. For readers not being familiar with Schellenberger ice cave (as I am) a sketch demonstrating the vertical structure of

the cave would be helpful. Searching for some related information on this needs quite some time as is mostly hidden in "grey" literature.

Fig. 5 and Fig. 6: You may consider omitting one of them as both essentially provide the same information. Moreover, they suggest that the three coefficients are determined by a consecutively performed three step procedure, which contradicts what is stated in context of equ. 4 and equ. 5 (p5297, p5301). I basically understand your approach as a two step procedure (first determining delta(t) bycross-correlation and secondly determining bias and scaling simultaneously applying regression analysis. Please resolve possible misunderstandings and improve text and legends correspondingly.

Legends Fig. 5 / Fig. 6: what means " ...extended period displayed for loggers..." ?

Âňp5301 l25 - p5303 l9 i.e. discussion of Fig. 5 and 6: this is within "application to data" which corresponds to a "results" section and is currently difficult to follow due to a mixture of description of cave features, instruments and interpretations in terms of (reasonable but largely unproven) comments on cave processes (belonging to section discussion). A less disturbed description of the results themselves would be desirable here, focused on time lags and velocities. Some important information is quite hidden in legends. Mention also more clearly that these figures refer to the calibration of the method. Provide more accurate information on the "good fit" mentioned in p5303 l9 and point out that this may not be considered as a measure of the skill to forecast data outside the calibration period. The fact that based on your data the method does not work to determine air velocity between T1 and T3 may be marked here as well, which is crucial regarding the overall aim and the title of the paper.

Sect. 3.1 needs major revision: First, because one expected a validation of the method here, which is mentioned (p5305 l18) but is not really addressed although being essential due to the methodical nature of the paper. The heading as well as p5303 l13 indicate that the aim of this section is to check the temporal variability of calculated speed of cave flow. Essential uncertainty arises because it is not clear how the results presented in Fig. 8-10 are produced. This is in part due to imprecise notation e.g. what means "best correlation" mentioned in Fig. 8? I guess this refers to coefficients being determined for (temporally shifted?) calibration periods. Please clarify and describe in detail the individual calculation steps yielding the shown time series of "best correlation" and "time shifts". As to the presented results (Fig. 8, 9 and 10), I am wondering about any purpose of showing and discussing results related to time shifts =0 (or negative), which merely indicate that the method is not valid to calculate flow speed under the given circumstances. Is it thus a valid conclusion that air flow is not constant, is it worthwhile to consider corresponding correlation time series? Please help understanding why correlation is insignificant below 0.97 (r or r2 ?), compare to a corresponding statement later (p5305 l9). If meaningful at all, the right hand panels of the plots should better reveal the significant periods and corresponding description and discussion may be better focused on that. Thereby and referring to Fig. 8 &11, please also elucidate why right during methodically insignificant periods (centered e.g. around 01 Feb) significant time shifts (and thus velocities) shall reliably be calculated and may serve for estimation of flow speed. Why not during 30 Jan, when equally pronounced time shifts are obvious? Section 3.2 (Validation) needs essential revision, too: validation in a true sense is not possible due to the lack of corresponding data, as you state and remains a serious methodical shortcoming of this work. Instead you propose "internal validation" based on the "shape of the correlation function" introduced in Sec. 3.1 and error analysis comparing modeled and observed temperatures. Concerning the first, it can just be assumed that you mean cross validation, which to a certain extend is acceptable but needed to be documented more clearly and in more detail. Please then state corresponding essentials e.g. the choice of training and testing periods, did you apply e.g. a leave-one /or block out method? How then is such analysis connected to the time series presented in e.g. Fig. 5, to which you refer here again? Clarification of this issue is important to judge the remaining parts of this and the next section.

Section 3.3 (Discussion): p5308 l20: is there an argument how rock or ice temperatures could be used in context of calculating velocity of air flow in caves?

TCD

9, C2266–C2273, 2015

Interactive Comment

p5309 l1: you here present two single numbers of calculated velocities, which presumably are for many readers most interesting in the overall context of the paper. Unfortunately, however, it is not possible to reproduce them based on what has been presented and discussed before. I am thus wondering for which section of the cave they are valid and how you can state them as "realistic results" without any reference in terms of independent measurements, thereby even differentiating for inflow and outflow conditions. According to Fig. 1, 2, 5 and 6 the method could not detect a significant time shift between loggers 1 and 3, how then could you calculate inflow velocities (which according to the various comments occur right within this cave section)? In the next sentence you mention that insufficient sampling rates limit the analysis, which on the other hand shall not limit the validity of the results and still enable to characterize patterns of air movement in the cave (no section-wise distinction of time lags is given and flow direction can not at all be determined). I may mention at this point that I do not doubt the conceptual view as is put forward in Meyer 2014 et al.. I assume that being well based on valuable observations, but we here needed specific data to support the mentioned statements incl. the summarizing one at p5310 l7. I therefore propose to present in this section how the mentioned numbers concerning flow speed were actually determined. Refer to e.g. Fig. 5 or 6 and thereby also to (better documented) distances between the different loggers (see comment above). As far as possible, put them in extended context to uncertainties including those imposed by the temperature measurements themselves (which is not at all addressed so far). In the discussion, please be more specific how exactly this application of the method contributes to more detailed knowledge about the cave circulation. I am also wondering in this context, why you did not consider temperature measurements outside the cave, which according to Meyer et al. 2014 should be available (Geiereck). Concerning the estimates of flow speed one might finally ask about the benefit compared to derive them from plainly reading out phase shifts and corresponding distances from one station to the other (based on Fig. 5/6 and distances in Fig. 3)? Thus, a zoom into Fig. 7 easily reveals a time lag T1-T4 in the order of 300-350min compared to 300 in your Fig. 8 (lower panel) which via measuring an approx. distance of 115m may be converted to ca. 2m/min as a measure of effective flow speed (which is comparable to one of your given numbers). One may expand that to get some section wise overview telling that there was no significant phase shifts between T1-T2 i.e. that time resolution is insufficient to resolve air flow in this section, ca. 130min across a distance of 65m between T1 and T3 (yielding 0.5m/min).

Abstract and conclusions may be adjusted concerning the proposed revisions (validation and results mainly).

Interactive comment on The Cryosphere Discuss., 9, 5291, 2015.

TCD
9, C2266–C2273, 2015