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

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–> general comments:

This article particularly focuses on the introduction and description of the calcFLOW-method. Numbers and results, which are mentioned in the article, are explicitly mentioned to support a better understanding of the description and to give an example for the application of the method. The focus is not to present new results on the on the airflow regime of Schellenberger Ice cave. According to the data available for the study site we are not able to describe the airflow regime in all details so far, but this does not reduce the validity of the method in general. In general, I am convinced that the quality of the results is mainly limited by the quality of the data (e. g. time resolution) and not by principal shortcomings of the method. For the example of Schellenberger ice cave we have problems with the not optimal positioning of some loggers and the sampling rates, but these are also common problems for others. But we also have to emphasize here that the air temperature measurements at the study site were not installed for this purpose, as they have been started already in 2007 for a first basic study on the air temperature in Schellenberger ice cave. The unfavourable sampling rate is a result of the runtime of the batteries and the limit of the storage medium and thus constrained by circumstances. With the help of our method we received valuable hints on how to improve the logger setup and this will most probably also be the case for studies in other caves. One of the advantage of the calcFLOW-method is that it is applicable whether you use only a little or a big amount of loggers.

I have to admit that the time lags that are needed to determine wind speeds are the parameters of our model that are determined weekest. This is mainly due to the insufficient observation geometry and sampling rate. With better temporal and spatial resolution of temperature observations it should even be possible to include them into the least squares adjustment in an iterative way (experiments in this direction with the existing data set did not lead to convergence). Concerning the validation: We do not have any long-term airflow measurements for the study site and will not have in the near future, so for this article we have to live without.

The presented quality measures (formal and a posteriori errors) and the provided tests of significance of parameters are standard methods to assess the applicability of a model and the model fit in least squares procedures. We have to learn about the cross validation methods mentioned by the reviewer and see, if they may be of additional value.

–> specific comments:

According the detailed questions I would like to focus at this point of time of the most important. In the specific comments the reviewer states correctly that we published in

Meyer (2014) that there may be an connection of Schellenberger Ice Cave with a bigger cave system below. At the lowest point of the ice cave (Fuggerhalle) there existed a passage to a small underlying room that was free of ice. This passage was artificially kept open for a while, but now it is mostly closed by ice for many years.The fact that the recorded air temperatures at Fuggerhalle during the open period of the cave were warmer than the temperatures closer to the entrance initiated the development the calcFLOW-method. It was suspected that Fuggerhalle is warmed from below (i.e. by dynamic ventilation). Our study revealed that Fuggerhalle fits well into what our model predicts and it is warm because it is the farthest point from the entrance (where the cold air enters). While we cannot exclude it, at the moment we do not have any hints for a warming by dynamic ventilation. Because of the fact that we have so far no airflow measurements in the cave, we were looking for another possibility to prove or disprove this theory on the base of the given data. We think that this strengthens the possible applications of the method rather than limiting them.

Regarding the question of the open phase: We have to admit that the formulation of the sentence is indeed wrong. Of course, the open phase is not limited to negative temperatures. Moreover it is limited to external temperatures below the cave air temperatures, whose are in many cases negative.

Another question was: Is the applied method a 2 step or a 3 step approach? This side note of the reviewer actually is a very interesting question. In a mathematically strict sense the modeling of the air flow has to be done in one step and this should become feasible when the observation data allows. The separation into two steps (first the time lags by cross-correlation, second bias and scaling factors by least squares adjustment) is a necessity due to the poor observation geometry and sampling rate. Actually step one fails quite often while the least squares adjustment is robust. All results presented in the paper were derived in a two step approach. Note that it is always possible to set some of the parameters of a model to zero and just evaluate a subset of parameters (as done for some of the figures). In the special case of our model we found out that the correlation between bias and scale factor is very low (as mentioned in subsection 3.2) and therefore a 3 step approach will not lead to different results (this was proven by experiment). This fact on the first glance is quite astonishing, because normally one would expect bias and scale factor to be highly correlated. The low correlation is due to the fact that we first subtract a mean and only scale the remaining variations. In this way the mean of the observed temperatures is not scaled and thus the two parameters may be estimated separately.

How were the provided air speeds calculated? They were calculated exactly as the reviewer did in his example. We chose a period of relatively stable conditions and devided the median of the determined time lags by the distance between the loggers. Due to the poor determination of the time lag between loggers 1 and 3 these numbers are just a rough guess to check, if the obtained results are plausible. And this is also the reason why we did not elaborate on them. We will add some explanation and we have to provide some reference to measured airflow speeds during the open period of static ice caves from other study sites (references are welcome).

What is meant by the note that the determination of time lags may be validated by the form of the correlation function? This was a rather unscientific attempt to find something usable for validation. A distinct pattern in the temperature curves will lead to a peak of maximum correlation that has steep flanks and is easily distinguishable from side peaks. A time lag derived from a cross-correlation function showing a distinct peak with steep flanks is more reliable than a time lag derived from a not very pronounced peak with flat flanks. Maybe there exists some statistical method that exploits these features. We will have to study some literature on that and we will also follow the hints on cross-validation that were provided by the reviewer.

Some additional remarks: It will be straight forward to follow the reviewers advice and include a dedicated section on study site and logger setup. A vertical section of the cave would indeed be very helpful for illustration and for interpretation of the results. The weak point of the article is section 3.1, where we study the temporal variability of the determined parameters. There we encounter all kind of effects that most probably are due to the bad data coverage and cannot be explained satisfyingly. We could think about canceling this section, because it adds nothing from a methodical point of view. Instead we could choose one or two example periods and limit our evaluation to them. Moreover we should avoid any speculation and reduce our conclusions to what we actually learned: Logger 1 misses the main inflow of cold air, logger 4 records the warmed outflowing air, the temporal sampling of our data is to poor to derive the airflow speed of the inflowing cold air between loggers 1 and 3, and the spatial distribution of the loggers is to poor to really separate different airflows. Concerning section 3.2 I do not see any chance to validate our results by external data. But I do not think the provided internal validation is lacking.

I agree with the reviewers remark that reality is much more complicated than our model. Our evaluation is based on the assumption of constant airflow speeds between loggers. But airflow speed depends also on passage size and therefore will never be constant. A volume model of the cave would help, but portable laser scanners just start to become available and this kind of model will not be available for most study sites. A much denser network of loggers would also help. For a rather small cave like Schellenberger ice Cave some hundred loggers may suffice to get a reasonable picture for a model of the energy flux, but for evaluation with the method presented, even the simultaneous and automated evaluation of many thousands of loggers would be easily feasible. We do not have that many loggers and this is also the reality for many researchers. Considering the very limited number of loggers and the very simple model setup the fit achieved and illustrated in Figures 5 and 6 is astonishing and in my eyes justifies publication, even if our conclusions on the air flow regime of Schellenberger ice cave are still very limited.

Regarding the application of the method I would like to give some additional ideas, which I will describe more precisely in the final review of the paper. As already said, our paper is in general a methodological paper, so we do not attend to present more detailed results on the study site Schellenberger ice cave. The advantage of our method is that, when you install a dense measurement network for example in one passage of the cave, you get information, in which part of the passage (respectively height) you have the inflowing air respectively the outflowing air and the travel time of the inflowing air. With this information you can estimate or calculate approximately the airflow speed depending on the profile of the passage, which could be calibrated with additional loggers. Moreover you get the temperature bias with our method and with this information you can start to calculate an energy balance of the cave. Of course, we are now at the beginning of the development of the method, but we thing that there numerous other applications, which we can deduce from the basic results we receive now. For example, in Schellenberger ice cave, we would like to know more about the energy balance of the cave, but we only have some loggers for air temperature and short-term measurements of ice temperature etc. So quite a lot of information is missing. For this reason we started also to think about how the can calculate /deduce /estimate the missing information for a basic calculation of the cave's energy balance from our given data. On this point we would like to focus more in the future. I agree with the reviewer that ice and rock temperature will provide the possibility to calculate and observe the energy flux between the different media.

We would like to emphasize that possible applications of our method can not be reduced only to the time lag as one result of our model and the possibility to estimate airflow speed, the two other parameters bias and damping are equally interesting for other applications like the energy balance of the ice cave.

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