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The Cryosphere Discussions

# *Interactive comment on* "Linkage of cave-ice changes to weather patterns inside and outside the cave Eisriesenwelt (Tennengebirge, Austria)" *by* W. Schöner et al.

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Many thanks for your comprehensive review to which we are responding to as follows:

General: The paper investigates determinants of ice growth and loss in alpine caves, an important problem for management of commercial caves and in protecting fragile ice formations from damage by visitors or broader climate changes. The approach initially adopted is quite formal in presenting ice surface- boundary layer and energy budget for an ice mass balance. The presentation implies that a complete characterisation of fluxes and net melt/accumulation is both feasible and planned. Unfortunately, many of the formulae for terms in the energy budget are not accurately developed, or



omitted (e.g. advective energy/mass transfer by water flow!). Some formulae might be justified if they were used to make rapid sensitivity analyses. For ex-ample a quick assessment of down welling longwave radiation (modified from eq 5) will show that it has negligible influence on melting. The absence of any references in this section implies that this section is either self-evident (in which case it is not needed) or original (which it is not). One is left concluding that the main objective of the section is to establish credibility by conspicuous wielding of equations. The instrumentation has obviously worked well over quite a sustained period, though there is no commentary on the practical aspects of such successful deployment. Unfortunately, the instrumentation does not appear to be compatible with the preceding theory on energy budgeting. The boundary layer gradient approach developed is not pursued, nor would it be compatible with the non-equilibrium boundary layer described, so that virtually none of the budget terms outlined in theory can be estimated. The reader is left wondering why the elaborate boundary layer theory is needed to allow an essentially qualitative analysis of the data. A more appropriate approach for these problems is the advective penetration model advocated by Wigley and Brown (1976).

RESPONSE: The reviewer clearly touches the general problem for studies such as the study performed by us – to find a balanced reporting between underlying theory in order to enable interpretation and to leave theoretical considerations because they are self-evident. It was certainly not the aim of the study to provide a full characterisation of energy fluxes and mass balance components, as stated in the introduction. In order to avoid misleading expectations and to clearly state the aims we reworked the introduction chapter. Additionally, we now can refer to the work of Obleitner and Spötl (2010) which, as part of the AUSTRO*ICE*CAVE*2100 project, provided results on all components of the mass- and energy balance for another site of Eisriesenwelt. This paper was not ready at the initial state of submission of our paper and therefore could not be included. From the work of Obleitner and Spötl (2010) several queries of the reviewer can be answered. We are still sure that the theoretical descriptions are of major importance for understanding of the interpretation of results and conclusions of the paper and are therefore kept it, however in a tightened form. We reworked the theoretical chapter according to the remarks of the reviewer aiming to keep it at a minimum stage. Additionally, we reworked the paper for better linking the theory chapter to the measurements and findings/conclusions. Instrumentation worked well after some problems at the beginning. We now introduced an extra chapter on measurements and added a paragraph on the constrains of instrumentation and problems/successful sensor implementation etc.. Though the sensors did not allow full development of energy balance terms they however allowed rough assessment of the main driving variables for some components of the energy balance (temperature gradient, simplified vapour pressure gradient, wind speed).

The measurements are interesting and well worth reporting. But the authors face a common problem in adequately presenting long high frequency time series; many of their graphs are ineffective in communicating the key attributes that are bring interpreted. The high frequency data are almost impossible to read. For example in Figure 7 the temperature data should be low pass filtered and resolved to temperature differences These differences need to be carefully plotted again wind expressed as approximate volumetric fluxes (+ve in, -ve out). The ice survey elevation data make the net melt energy inscrutable. The change in thickness would give this and allow comparison to similarly lumped energy flux terms. The ice data seem to suggest overwhelming year-to-year differences that demand immediate explanation, otherwise subsequent generalised analysis makes little sense. If tourist operations are a significant influence on air movement, then surely this needs substantial data and some attempt to segregate the data into door-open and doorclosed sets. The discussion of cryogenic carbonates sees like an afterthought. If the ice stratigraphy is important it needs full and early disclosure and description. The real message in the ice description is that there is a mineralised (liquid) water source that is being frozen and partially sublimated. The discussion of this poorly described phenomenon does not seem to draw systematically on the physics. The conclusion that the “ice mass changes are in fairly good agreement with the energy fluxes” is not supported as there is no quantitative state-



ment of these terms. The interesting diurnal temperature records (Figure 8) present substantial anomalies, but the authors fail to clearly highlight the paradox and do not provide any useful analysis. Any cave climate study lacking a vertical profile of the cave (i.e. providing the fundamental advectional setting) is unlikely to make much headway.

RESPONSE: The quality of Figures in particular Fig 7 (now Fig.8) was significantly improved and the key attributes are better highlighted. Computation of volumetric fluxes was not applied because of the high uncertainty of this quantity which would be derive from unknown cross section, low wind speeds (close to the detection limit of sensors), singular point information of wind speed. Ice survey data were transferred to thickness change data. Year to year variability is high as there is certainly significant influence from cave management (water input in order to clean the ice etc.). The door at the entrance is open between 1.12. and 1.5. (the reason why we focus on this period in Fig 7). Physics of cryogenic carbonate was not subject of our paper (we referenced Spötl et al. 2008 in order to exclude what is already published). We mentioned the cryogenic carbonates as their appearance in Eisriesenwelt clearly supports the role of sublimation (see also detailed comment to this subject later). Our conclusion that the “ice mass changes are in fairly good agreement with the energy fluxes” were reworked. In our study ice mass changes can be associated to single components of energy balance but the exact validation of ice mass change from energy flux is not possible. Diurnal temperature fluctuations paradox was reported as an interesting finding. We do not have plausible explanations now but we think that the paradox is well worth reporting. As this paradox was unexpected result we do not have network of sensors in order to go into details of forcing. There is certainly the influence from the second entrance for the site Odinsaal (which could explain the higher temperatures for this site in late summer) and we would modify our measurements in a follow-up study aiming to explain this paradox. A generalised cross section of the cave was provided as new Figure 2.

Overall, the paper’s strength is in providing a sustained data set describing cave temperatures and wind. Yet the boundary layer (energy budget) theory is not appropriately developed for the setting, nor usefully applied to the data set. More diligent data processing as a foundation for a systematic qualitative characterisation of the primary processes and the likely signatures could be much more useful. The analysis of the data does not get beyond a qualitative interpretation of poorly presented information which is unfortunate.

RESPONSE: We now refer on a simplified theoretical background for interpretation of data series. In particular the linkage between the underlying theory and the results gained from measurements was significantly improved. Additional information on energy fluxes were derived from ice surface height changes.

Detailed comments Section 1. So the purpose of the paper is.....? The introduction suggests that ice stratigraphy can be interpreted through inverse modelling which requires a robust and unique means of deriving ice thickness (and composition?) from climatologically pertinent variables. In retrospect, there is little support for this grand scheme.

RESPONSE: See also response to general comments. We reworked the introduction chapter in order to clarify that the aims of the study are not ice stratigraphy and inverse modelling.

Section 2 Methodological concepts and data: this section has no references implying either it is self-evident or original. Sources for the many equations and claims should be provided.

RESPONSE: References were provided.

Equation 3. Meltwater advection appears to be significant (there is no meteorological precedent for rapid accumulation of ice nor freezing of mineralised water) and advection is not in the equation.

RESPONSE: We introduced a term for seepage water in both mass and energy balance equation. This is in agreement with other studies (Obleitner and Spötl, 2010 or



Luetscher et al., 2003).

Eq 5. Why are air and rock radiative terms additive? This seems like a very naïve expression of down welling longwave radiation to a surface. Best bet is to measure it directly, especially in a complex geometry. The net longwave can be approximated from T4 which for any likely T can be shown to be ->0, so can be ignored . Selecting a suitable emissivity is not easy, so most people close their eyes and assume it is constant.

RESPONSE: We reworked the equation for longwave radiation. However, net longwave cannot be ignored as shown by Obleitner and Spötl (2010), but is largest input of energy in summer (when turbulent fluxes are ca.0). We improved explanations to the radiative terms.

Equation 7 and following: the turbulent exchange term still needs to be determined. The expression “is well explained” is not clear nor justified quantitatively because the boundary layer is not unbounded (there is a roof and walls) and gradients are not homogeneous (edge effects and adiabatic effects may occur), a fundamental assumption in gradient energy budget methods. Qualitatively, the influence of temperature and vapour pressure gradients is adequately presented. It is difficult to believe that stability pervades the system throughout the year. Cold air over warm (0C) ice will be unstable; a particularly likely condition in winter near a lower entrance.

RESPONSE: We simplified our concept for estimation of turbulent fluxes making detailed discussion on surface near conditions (roughness, stability, etc which could not be measured or parameterized) unnecessary.

Equation 8. The ground heat flux is important as a substantive term. It can be modelled fairly accurately based on surface temperature measurements and assuming reasonable simple boundary and initial conditions. If it is to be excluded, it requires a quantitative demonstration of its insignificance in the overall balance (EQ2).

RESPONSE: The role of ground heat flux was discussed in more detail now. However, the ground heat flux is not essential in order to understand ice height changes observed for Eisriesenwelt.

Equation 9 a should be a (subscript). Note “a” has been previously defined as a melt term, so should not be used ambiguously. g=is not defined

RESPONSE: Was changed and g was defined.

Equation 11 and ff. The expression is a bit sanguine (non critical). It is not really stratification that drives the chimney effect winds it is hydrostatic imbalance in coupled columns. Job one is defining the approximate column geometry. Taking the hydrostatic case and extending it into wind dynamics is a much more challenging problem. The subsequent exclusion of water vapour from consideration may be a mistake as it implies RH0. RH100% is a more reasonable assumption if it is to be considered constant.

RESPONSE: We rephrased the description on forces for air ventilation in the cave. Column geometry is now described by new Figure 2. We, however, excluded dynamical forcing of air ventilation as on the one hand side the measurements clearly suggest hydrostatic behaviour and on the other hand side dynamical forcing is hard to quantify. This is clearly explained in the text now.

P1715 line 20 ff. The discussion here confuses mechanical advection (external pressure patterns) and external-internal pressure fluctuations with density contrasts (Chimney effect winds).

RESPONSE: The paragraph was reworked in order to avoid misinterpretation and confusion. See also statement above.

Section 2 measurements. It is difficult to link the measurement regime with the formal theory previously outlined. Sometimes, there is a lack of clarity in the language used. More important, it is not clear how the gradient approach is being applied from apparently single, edge-influenced (i.e. not amenable to boundary layer representa-



tion) measurement of wind speed, ice and air temperature and (apparently unsatisfactory) relative humidity. A critical consideration of the energy balance equation shows that only M is fully characterised by the stake and distance measurements (though for some reason never quantified in the theory; M=Lf(zi-V/A), perhaps). The radiation terms lack rock and representative air temperatures, the sensible and latent heat terms lack diffusivities and representative air temperature and relative humidity. In addition, the ice surface temperature is a very poor datum for gradient methods. There is no ground heat flux term modelled or measured. Does it matter? And advection by liquid transport is not included (and it is clearly important if incoherent). It is not clear how the discussion of pressure-temperature relations is applied. None of the analysis uses energy flux density or water equivalent melt rate. It is largely an analysis of the form of the primary data series. I suggest that the formal theory presented is not being used critically in attaining a practical energy budget and complete monitoring programme. It would be more straightforward for the reader to use the theory less rigorously to provide a qualitative basis for interpretation of the measurements of ice growth and meteorological variables. The problem with the setting is that the energy budget is strongly influenced by air and water advection that is incompletely characterised. Wigley and Brown (1976) provide a much more salient discussion of chimney effect winds and penetration distance into a cave.

RESPONSE: See previous response above. We now used the theoretical equations of turbulent fluxes in a less rigorous form in order to derive robust conclusions on the meteorological forcing for ice height changes without the need to go into details of boundary layer conditions (surface roughness, stability etc.). Additionally, we used ice height changes to compute underlying energy terms. The concept is now much more consistent.

Results: The most obvious problem to address is that the winter of 2006-7 is apparently quite different to the winter of 2007-8 and 2008-9. Similarly the 2008 build up is not replicated. The temperature analysis does not immediately explain these inconsistencies.

RESPONSE: The US-height measurements show a clear and replicated annual cycle of ice changes for 2007/08 and 2008/09 with exception of the built-up in autumn which is, most probably, generated from artificial water intake from show cave activities. Therefore the temperatures cannot explain this "inconsistency"

Contrary to the claim (p1719 line 10-12), the wind in the cave has a strong correlation to outside winds, suggesting physical forcing rather than simple chimney effects. The more detailed data (fig 7) can not be assessed by the reader (as described in 1720 17-19). To make the claim better substantiated, the data might be low pass filtered to an appropriate frequency, and internal-external temperature differences plotted. The wind speed should be expressed as velocity vectors (velocity x direction). A shorter clearer time period might be used, and a plot of wind velocity against temperature difference provided.

RESPONSE: We assume this is misinterpretation of Figure 7. There is no outside wind shown, but wind inside the cave which is highly correlated. However, there is strong correlation in temperature behaviour between outside and inside which supports the density contrast/hydrostatic forcing of the air ventilation in the cave. See also later comment!

1720 line 29 No reference to figure 9. Figure 10: the diurnal cycle is actually not easy to resolve on these graphs. Do you mean figure 8?. Rather than claiming the diurnal variation is due to door openings, present data to support the claim. This control makes interpreting figure 7 very difficult. How is a threshold external temperature excluded from consideration?

RESPONSE: Reference was added. The diurnal cycle is well reflected in Figure 8 (Fig.9 in the revised version of paper). Reference to Fig. 10 was wrong. In fact forcing of the diurnal cycle remains unclear and would need further measurements. Figure 7 (Fig 8 in revised version) covers winter period mainly when the cave entrance was



continuously open. The diurnal cycle was observed in summer only. Explanation by the influence from door openings on the diurnal cycle is vague. We rephrased the text.

1721 line 10...is adiabatic warming likely? See Wigley and Brown for discussion of penetration distances and flow reversals. Provide a reference for assuming 100% saturation (1721 15-21). Winter winds are warmed and are unlikely to be exactly at 100% RH and so induce sublimation loss. Summer cold air drainage can initially precipitate hoar frost. (See W&B for discussion)

RESPONSE: The paragraph was rephrased. If adiabatic warming is likely is not known and was therefore excluded. We added information on invading winter air masses which could reduce rel. humidity below 100% inside the cave for the entrance near parts, thus even increasing the vapour pressure gradient for sublimation. Reference is provided.

1722 6-16. The data indicate the direction and magnitude of a vapour pressure gradient. More precision will not resolve the problem of determining sublimation/ evaporation. The situation is very difficult to model or monitor. In effect, the finesounding theory (e.g. eq 7) is actually not really applicable.

RESPONSE: Yes and no. It is true that major problem for computation of turbulence fluxes derives from unknown information on eddy diffusivity, but on the other hand quantities of energy balance terms are all rather small. So to measure it really would need higher precision sensors. Equation (7) was introduced in the paper to clearly show the relationship, what is measured and what is missing in order to resolve the balance terms.

1722 17ff. The argument seems to get derailed here by discussing previous work under results. I assume that "carbon" is actually meant to be "carbonate". So the point is that sublimation is demonstrated which implies an upward vapour pressure gradient when the temperature is below zero Celsius. It should be possible to quantitatively identify periods of sublimation loss and gain and evaporation-condensation. Condensate and hoar water should not contain carbonate, so you have a testable hypothesis that the bulk water contributing to ice formation is groundwater. Your closing remarks seem incompatible with sublimation loss dominance required to produce carbonate cryobanding. (There is a fairly useful literature on this phenomenon. see Karel Žák, Bogdan P. Onac and Aurel PerÂÿsoiu 2008 Cryogenic carbonates in cave environments: A review. Quaternary International Volume 187, Issue 1, 15 August 2008, Pages 84-96 Archives of Climate and Environmental Change in Karst )

RESPONSE: Carbon is meant to be carbonate. The upward pressure gradient and therefore periods of sublimation is demonstrated by Figure 10 (now 11). As pressure gradients were computed under the assumption of saturation actual vapour pressure of atmosphere is even lower in cases when relative humidity is below 100% because of warmed-up dryer air invading from outside the cave. Only in cases of invading cold air from outside in winter significant wind speeds were measured and thus sublimation plays a role. Cases of deposition or condensation were negligible for mass balance because of the close to 0 wind speeds in those cases with vapour pressure gradients towards the surface. We feel that our point concerning cryogenic carbonates was misleading. In fact not the initial formation of cryogenic carbonates suggest the role of sublimation but the concentration of cryogenic carbonates by sublimation to distinct brown layers (in the ice profile as to be seen at site Eiswall in Eisriesenwelt as described by Spötl (2008) for Eisriesenwelt) supports our results. We rephrased these statements on cryogenic carbonates.

1723 12-15 The longwave might become a net contributor, but what is the source temperature from an atmosphere? The cave wall temperature might give an approximation. See discussion of eq 5 above which can now be applied to discover that the resulting melt is 10EE-10 - 10EE11mm/day. In other words, applying the theory can usefully dispense this discussion. It is not clear how the "ice temperature" was measured. Encapsulated Hobo recorders are not suitable nor are probes because they do not provide "surface" temperature. A remote thermal infrared thermometer might be



better, but difficult to calibrate adequately. Instead an ice temperature profile can be used to extrapolate to an estimated surface temperature with the added advantage that a suitably sensitive unit could indicate the presence of liquid water (depending on the mineral composition of the ice and water.)

RESPONSE: Information on longwave radiation is now included from the work of Obleitner and Spötl (2010), which showed that longwave rad. is the main contributor of energy balance. The loggers for ice temperatures were not encapsulated. But the reviewer is right, it is not really surface temperature what we measured. However the HOBOS gave ice temperatures 1-2cm below the surface (and were adjusted during each field visits). We now described this in the measurement section of the paper. The "close to surface" ice surface temperatures gave surprisingly good results and fit well with observations on ice height changes.

1726 10 "The ice mass changes are in fairly good agreement with energy fluxes" There was never any systematic presentation nor analysis of this. Given the heterogeneity of the ice change and the limited climate data, it is not going to be easy to obtain a reasonable resolution. As a start, the net ice changes over each observation period can be converted to a melt energy value. Similar integrations can be made for vapour and temperature x windspeed to get a surrogate measure of sensible and latent heat fluxes. Segregate into warm and cold ice conditions. These can be compared to one another. The advection of water was not discussed nor measured and may prove to be larger than any of these terms unfortunately.

RESPONSE: This is right, our statement is not appropriate to the results shown. What we showed, and what was a clear result from the measurements, is that ice loss in winter/early spring clearly comes from sublimation. Even though it is only in the order of approx.. 2cm it is a significant result of US-ice changes and computed vapour pressure gradients. Similar the ice loss in summer can be explained clearly from melt (indicated by ice surface temperatures). Additionally, we now computed energy fluxes from height changes as suggested. We also rely now on the results of the work of Obleitner and

Spötl (2010) which provided several results which fits well to our findings.

1727 18ff. The summer air temperature data are indeed interesting and deserve greater consideration. The near entrance temperatures are higher than the internal temperatures which is not possible using a single conduit penetration model. The implication is that there is a secondary flow system influencing temperatures. Vertical section cave maps may reveal this if exploration is complete including the roof. The other feature is that there may be a correlation with external air temperature lagged by one day. This is not impossible, but my first question would be on the logger clock synchronisation. (I say this having done it myself!)

RESPONSE: It is true that the cave is not a single conduit. There is a least one additional flow system certainly influencing the inner AWS site (Odinsaal), to be shown in new Figure 2. Investigating/understanding this feature would need other instrumentation as used in our study. Logger synchronisation should be ok as loggers were replaced several times and the periodicity was observed for all logger configurations.

Table 1: Luftfeuchte=relative humidity. The sensor model should be provided, not just the manufacturer. The table could be enhanced by adding the approximate precision. (Assuming calibration has taken care of accuracy adequately)

RESPONSE: Luftfeuchte was replaced by rel. humidity. Sensor models were provided in case they are existing. Some sensors don't have further specifications.

Figure 1. Not sure what the grey shades and lines indicate on the plan. Vertical profile of the cave is more important than the plan for meteorological interpretation.

RESPONSE: This is the cave plan available. The bold line indicates the guiding track for visitors. Dark grey shades are paths under or over the ice covered part of the cave. This is explained in the Figure caption now. A new Figure with schematic sketch of cross section is now included.

Figure 4. Not sure what the two vertical scales refer to. Not clear how a max, min



and average are computed. The step-character suggests a finite resolution, but this should disappear in averaging many such discrete values. The figure caption and label indicate that this is "change" (i.e. z in the respective interval). . but the graph looks like it is actually zt-z0, the elevation relative to an arbitrary datum (time zero?). The rate of change is probably more pertinent to the energy budget approach z/t), Clarify. Dates are hard to read and different in the two graphs.

RESPONSE: Comment was to Fig 5 and not 4. Max, min and mean is explained now (are highest, lowest and mean value of hourly readings and was thought to give some information on data quality). Graph was changed to the scale of Fig 4.

Figure 5. See figure 4. These are elevations not "changes" I think. It is not clear which axis refers to which line. The lower right hand axis seems to be a different scale.

RESPONSE: Axis of Fig. 4 were changed to thickness change.

Figure 6b. Wind speed is not really expected to correlate between outside and inside. Within the cave wind speed may correlate, but is contingent on cross sectional area. Discharge would be a more appropriate measure of advectional forcing of the energy budget.

RESPONSE: We agree that "discharge" would be intersting to have but would need cross sectional area, which is not available. Additionally, computation of discharge for site Posselthalle (which is rather large hall) would need more than one velocity measurement in order to provide representative discharge data. Additionally, for interpretation of turbulent fluxes wind speed is more appropriate.

Figure 7. Is difficult to decipher. I suggest making the time axis readable and simpler (label each month which is about the readability in subsequent figures as well). The wind velocity and direction should be combined to show inward and outward velocity (the product of the two graphs) or flow (x respective area).

RESPONSE: time axis were changed and quality of Figure were significantly improved.

We prefer not to combine wind speed and wind direction in order to enable clear interpretation of wind speed which is of higher interest as wind direction.

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Interactive comment on The Cryosphere Discuss., 4, 1709, 2010.