"Investigating cold based summit glaciers through direct access to basal ice: A case study constraining the maximum age of Chli Titlis glacier, Switzerland" by Pascal Bohleber et al.

- Response to reviews -

Please note:

- ***Author's responses to the referee's comments are in blue***
- ***Changes in the corresponding revised manuscript are highlighted in red***
- ***All line numbers in "Changes to manuscript" refer to the new revised version***
- ***All new references can be found in the new manuscript***

Response to referee #3, B. Hubbard

General comments

The paper reports ice temperatures, stable-isotope concentrations and radiocarbon dates from ice located near the base of Chli Titlis, Switzerland. The manuscript reports that the basal ice is cold (and infers that this has been the case for a considerable period of time) and that it has a maximum age of _5000 years BP. I find the manuscript to be of interest, but also that it still needs fairly substantial revision and improvement in a few areas. My main concerns relate to two aspects of the manuscript.

We thank the referee for his valuable comments and suggestions, which we believe have helped us significantly to improve the revised version of the manuscript. The work has been designed as a pilot study focused on investigating the unchanged existence of the lowermost layers and their potential for drawing conclusions regarding the maximum age of ice at the site. The main concept was to assess the feasibility of the approach before being applied to more locations in the future. Since the authors lack a background in geology we particularly appreciate the suggestion to include a description of the ice facies. We have added this aspect to the revised manuscript and will also include the ice facies investigation in our set of methods for future work at Chli Titlis and other sites.
A more detailed response to the referee's comments in presented below.

1. The measured ice temperatures at the location are too close to the glacier's surface and/or the access tunnel to be able to ignore these external influences. Seasonal variations in surface temperature typically penetrate 10 to 15 m into the ice. Further, temperature within the tunnel is artificially influenced and, to some extent, controlled (as clear from p. 7 paragraph 1). The reported temperatures are

also not substantially below freezing, so error 'estimated at 0.2 degrees C' really needs a more formal quantitative analysis and accompanying statement. I recommend plotting the thermistor time series to reflect ice cooling back down (presumably exponentially) to host temperature following borehole-wall warming by the steam drilling. I believe this aspect of the manuscript needs greater focus and for all the relevant information to be brought into one early section. For example, published temperatures are reported towards the base of page 9 that should really form part of this background material. Temperature control in the tunnel is also mentioned elsewhere later in the manuscript.

We fully agree with the referee that both the seasonal variations and the artificial cooling need to be considered in interpreting the measured temperatures. In fact we tried to emphasize this important point already in the original manuscript. Regarding the thermic disturbance by stream drilling and subsequent equilibration, we have ensured to wait (40-60 mins) long enough for temperature fluctuations to be well below the measurement accuracy. Regarding the latter we refer to the study by Hoelze et al. (2011) that employed identical sensors.
From the referee's comment we understand that there is the need to present this information in a more concise way and early in the manuscript. We have modified the text and added to section 3.1 accordingly. We are continuing the discussion with the referee regarding thermal conditions by responding to other comments made by the referee below.

Changes to manuscript: New paragraph starting out section 3.1 to summarize the settings of the ice tunnel relevant to temperature.

2. The manuscript reports on the temperature of basal ice, but presents no formal analysis of the nature that ice. Such an analysis would be useful both for the reader to understand the nature of the environment, and because certain physical basal-ice facies are indicative of certain basal processes and conditions. I recommend the revised manuscript include a formal analysis of the types of ice present at the sample locations (see review by Hubbard and others (2009) in Quaternary Science Reviews and references therein).

We thank the referee for bringing this to our attention. We consulted the suggested literature and now present a full description of the visual stratigraphy at the three sampling sites in the ice tunnel. We also include a summarizing table with an ice type classification, adopting the scheme of Hubbard et al. (2009). We believe that by this means, the similarity between profiles 1 and 2, but also their difference regarding the basal ice are clarified. This also concerns differences with respect to profile 3 in the far end of the tunnel.

Changes to manuscript: Added to section 3.4 "Visual stratigraphy and physical ice properties". Added Table 2 including an ice facies description.

Specific comments (page/line):

1/1 “with great success” adds little and is a value judgement; I recommend deletion.

Changed accordingly.

1/3 “. . . low altitudes may also contain old ice if locally frozen. . .”

Changed accordingly.

1/5 “However, with recent warming and consequent glacier mass loss,. . .”

Changed accordingly.

1/6 Delete “, however,” and “Since sampling and dating the lowermost ice usually requires. . .”

Changed accordingly. We also generally tried to improve the second part of the abstract and reworded accordingly.

1/8 “We combine standard glaciological tools. . .” (and such ‘tools’ should just be specified as the term is rather too open)

Changed accordingly.

1/9 “. . . physical properties and radiocarbon dating.”

We now use the term "state-of-the-art micro-radiocarbon analysis" in order to distinguish it from conventional radiocarbon dating (the application to glacier ice being a challenge not least due to the low carbon concentrations).

1/11 this “pioneering exploration” needs to be specified for the definite article (“the”) replaced. (Note: I have not gone through the rest of the manuscript in the same detail; the grammar can still be improved)

We now use the indefinite article. Thank you for your help in improving the language. We have tried to also improve the grammar of the rest of the manuscript.

2/15 These statements make clear the need for a formal analysis of the ice types present and sampled for this study.

As stated above, we have followed the referee's suggestion and now include information on the ice types (cf. new Table 2).

2/23 What is “glaciological surveying”?

We clarified this by giving examples (mass balance measurements, ground-penetrating radar) to the tools used in the cited study.

Changes to manuscript: P2, L26. Please note that we have rearranged this part of the Introduction in an effort to improve clarity.

3/9 “. . . generally seem to be low” ideally needs some specification and quantification.

We agree that it would be preferable to include a quantitative statement here. However, we can only refer to what is reported in the cited study by Haeberli et al. (2004), which does not provide more detail in this respect.

3/18 - 21 I recommend combining this material with temperature data from the existing literature in order to present as complete and accurate a situation as possible relating to the thermal history of this site. That this ice is, and has been, cold is central to the manuscript’s message.

Thank you. We follow this suggestion and have integrated the text in the revised section 3.1.

Changes to manuscript: Moved to new paragraph in revised section 3.1.

5/8-14 I think the manuscript would benefit from a more formal statement of isotopic error here. Currently, some delta D values are issued with caution because of ‘large uncertainty’. I would prefer to see formal error bars added to each data point.

After considering this remark and also the comments made by the two other referees we have decided to include a detailed plot showing the co-isotopic data of profile 2, for which reliable delta D measurements were available. We decided not to consider the delta D data of profile 1 further, in view of the large uncertainties involved. However, we now clarify this and also report the respective measurement uncertainties.

Changes to manuscript: P6, L5-7, added text. Added Figure 4.

7/25 Tes, there appears to be a pattern here that broadly matches one(s) recorded elsewhere, but such a comparison should include all other profiles (including ice coring literature from Alpine glaciers at least) so the reader is convinced that this particular pattern is significantly over-represented. Also, if it is real, the explanation is a little truncated. Could it be related to the formation of clear facies basal ice by deformation induced preferential expulsion of light isotopes?

We appreciate this comment and take this as encouragement for further investigation into the origin of the basal isotope anomaly (which is in fact ongoing work). We will especially also consider the hint to deformation-induced preferential expulsion of light isotopes. The detailed explanation of the isotope anomaly was not part of the study presented here, however, and is certainly an intricate matter that deserves a separate investigation. An overview of the present state-of-the-art regarding the isotope anomaly is presented in Wagenbach et al. (2012) to which we have little to add at this stage. We have tried to point out that, within the present work, we are merely using the anomaly as a marker for the basal ice, previously described by the earlier study of Lorrain and Haeberli (1990).
We have added an additional statement to clarify that the anomaly is not regarded as being a climatic signal of atmospheric origin.

Changes to manuscript: P9, L8-9.

7/33-35 I'd like to see this co-isotopic plot (including error bars). Which of the less certain delta D values were used and what is their associated error. If the data are not of sufficient quality to 'interpret in more detail' then they may not be of sufficient quality to present at all; at present, the reader cannot judge this.

Thank you for this suggestion. As mentioned above after careful consideration we decided to i) clarify the associated errors, ii) not consider the delta D values of profile 1 further due to high measurement uncertainty and iii) show the co-isotopic data of profile 2 in a new Figure (Figure 4). The latter provides additional overlap with the previously reported co-isotopic analysis by Lorrain and Haeberli (1990), which we now include in the discussion.

Changes to manuscript:

- P6, L5-7. Added text regarding measurement uncertainties.
- Added Figure 4 with co-isotopic data.

8/5-8 Is it possible to illustrate these crystal size differences and the sub-grain boundary and elongation conditions mentioned? The elongated crystals sound like 'interfacial' facies ice, agreeing with the congelation origin advanced in the manuscript. The text states 'grain size' – which is presumably 'ice crystal size'.

We agree that the elongated crystals and almost bubble-free conditions at the base of profile 1 point towards congelation ice (and state this accordingly). At the same time we are not entirely sure how to best illustrate the crystal size differences other than reporting them in the text.

Changes to manuscript: P10, L8. We have also clarified the meaning of "grain size" being equivalent to "ice crystal size".

9/Fig. 3 These images are not very clear and seem to give no indication of scale

We had to reduce the size of the images in order to keep the file size manageable. We have increased the image quality and also include an indication of scale.

Changes to manuscript: Revised Figure 5, improved image quality and included scale.

9/2-4 This reference to possible warm temperatures in the past seems at odds with the general thermal interpretation of the site as cold. Perhaps some text could be spent on rationalizing these seemingly contrasting thermal conditions.

We agree about this apparent contrast and in fact our main intention was to discuss in this paragraph that the cold-based conditions are not immediately intuitive. Connecting atmospheric temperature to the thermal conditions at the base of the glacier is not straightforward of course and would require a detailed investigation of the surface energy balance. The latter is highly complex, not least due to the anthropogenic technical measures (ski area). We already discuss the role of recent negative mass balance and surface covers above the cave changing radiative fluxes, but also snow accumulation (wind drift) and percolation of meltwater and rain. We have taken the referee's comment as a suggestion to add some more details, including the apparent challenges involved in surface energy balance conditions. A thorough calculation of past and present changes in energy fluxes governing the thermal regime is not yet feasible within the scope of this manuscript, but will be subject to future investigations starting with the installation of a monitoring network – which hopefully will be funded.

Changes to manuscript: P12, L12-14. Explicit reference to surface energy balance and technical measures adding to the complexity of the thermal conditions at Chli Titlis

10/6 'karst'

Changed accordingly.

10/9-10 More here of relevance to the thermal conditions

Please see our comments made above and below.

10/17 True, but to focus on the future does not address the issue raised by this paragraph – that there may be issues complicating the temperatures reported. . . Surface temperature changes should be accounted for in any interpretation of point temperatures recorded within a thickness of _ 10m. Same for the tunnel, although

this zone of influence is likely smaller because the temperature changes in the tunnel will presumably be muted. Are there records of external temperature at the surface (or nearby, to which a lapse rate can be added) and in the tunnel?

To continue the above discussion, we agree that with an ice thickness of less than 10 m, it cannot be ruled out that the base is affected by seasonal temperature variations. We now mention this circumstance more explicitly. There is a weather station operated by MeteoSwiss at the telecomunication tower on Titlis glacier, close to the ice cave. Over the last decade, the data shows a typical seasonality ranging between 5 and -15°C (monthly data) and an annual mean temperature around -3 to -4°C (e.g. -3.5, -2.9 and -3.7 for 2014,15,16, respectively). Unfortunately, to our knowledge no temperature logging is available for the tunnel.
That said, the englacial temperature profile is determined by additional factors (which we are sure the referee knows), especially regarding conditions at the surface (e.g. meltwater, snow cover,...) which today are highly disturbed by anthropogenic technical measures (surface covers, reworking of the snow surface, etc.). Multi-annual logging of englacial temperature by installing a thermistor chain in a borehole would be needed in this context. Although this clearly went beyond this initial study at Chli Titlis, we have already incorporated this aspect in our follow-up studies at other sites in the Eastern Alps- featuring both borehole temperature logging and automated weather station data. We also take this as encouragement for future work at Chli Titlis.
In conclusion to this discussion, and to reiterate our previous statement, we fully agree that under these circumstances finding sub-zero ice temperatures is not trivial and has to be considered in view of the current technical measures by the ski resort. We appreciate the discussion and have tried to elaborate on these important issues even more in the respective paragraph.

Changes to manuscript:

- P11, L12-13. Explicit reference to influence of seasonal temperature variability.
- P12, L18. Mention reworking of snow surface for ski area maintenance.
- P13, L8-9. Suggest future logging of englacial temperature at the site.

11/Fig. 4 Some of the structures here do seem to indicate ice deformation. How does this relate to the interpretation of generally undeformed ice in this location? Maybe these features are not deformation structures, but some analysis and interpretation might help address this possible issue.

We are not entirely sure which of the structures the referee refers to but are trying to give an answer as we understand the situation. Except for the basal layer, we interpret the visual layering in the ice as originating from the surface, e.g. dust or soil material being deposited and accumulated on the glacier and subsequently incorporated into the ice body. We do not think that deformation

is entirely absent (cf. also section 4.3). However, we observe no evidence of turbulent ice flow or macroscopic layer folding. In addition, some localized basal melting may have occurred, and could have contributed to the observed lateral differences in age structure between profiles 1 and 2. We have added these considerations to the revised text.

Changes to manuscript: P15, L1ff. Added to discussion of deformation and vertical age gradient.