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The Cryosphere Discussions

Interactive comment on "Evidence of accelerated englacial warming in the Monte Rosa area, Switzerland/Italy" *by* M. Hoelzle et al.

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General remarks#

The englacial temperature of cold firn areas constitutes one of the most obvious, characteristic parameter of immediate climate relevance which, moreover, is directly gathered in the field. Dealing with its spatio-temporal distribution in the high Alpine Monte Rosa region the authors present a wealth of new data, which they put in context to former loggings. Apart from backing up the respective data base made freely available by the authors and from providing a well written, comprehensive state of the art (including respective bibliography) this conclusive work clearly merits publication in The Cryosphere. I feel, however, that the present version may be considerably improved by some, mostly not very substantial, revisions aimed at making the paper easier to read

and thus to attract a broader audience within the glaciology community.

Specific recommendations

1) Reading in the abstract: "The observed increase since 2000 is far beyond a modelled firn temperature increase based on the IPCC climate scenario from 2001", let me expect to find this issue elaborated in the paper. Since this is not really the case (just this sentence appears already in the Conclusions with a respective quotation of Suter, (2002) I suggest de-emphasizing this statement in the abstract. But I would prefer instead seeing this important aspect addressed more extensively in the Discussion (currently being relatively concise) e.g. by showing somehow the respective modelling result of Suter(2002) in the context with the data displayed in Fig. 10.

2) It would be helpful getting, beyond the given instrumental accuracy, a very brief statement on the expected (typical) uncertainty range of the reported temperature data, especially since different procedures are deployed over an extensive period.

3) The expression "firn facies" is repeatedly used throughout the paper though it is not always clear to what the authors are exactly referring to. Since not all readers might be familiar with this terminus I recommend introducing it at page 2279, right where the various firn zones are characterized.

4) Subjectively, I feel that the muddled data code and its application (referring somehow to boreholes, profiles, logging dates, drilling dates, etc.) contribute a good deal to my confusion and make it hard to follow the text without jumping back and forth to table 1 and respective maps. I can imagine that the authors are not happy changing their code in a more intuitive one (allowing for disentangling spatial and temporal as well as drilling and logging settings) if it is already deployed in their open access data base or elsewhere. Nevertheless, the dilemma needs to be overcome, at least by, by following measures: - give a brief advice somewhere how to read the code getting univocal information - dissect all three tables by drilling area or add a respective row - change figure legend or captions accordingly (as profile position and year can be

directly identified, see minor comment No 15) - avoid mentioning a profile in the text by stating its code only

5) There is a quantitative imbalance between the introductive, setting the stage and methodological chapters and the result and discussion parts being both relatively concise. Especially the text of -3 Results- is not intuitively structured (also lacking comments on the formal observations and motivation of figure presentations). It essentially repeats the information carried by the figures and tables in the order of their appearance, nevertheless, going through this text I quickly lost track. Although a detail, I missed here a comment on the role of the different borehole positions within the Colle Gnifetti north-west flank versus the saddle area (which is associated with a change in the surface energy balance), same hold true within Seserjoch area. For a potential extension of -4 Discussion- see No 6 below.

6) The finding displayed in Fig. 10 on the decadal scale change in the englacial temperature is perhaps the most essential one. It would deserve, therefore, some more critical discussion including e.g. following issues: - the atmospheric temperature level is significantly higher than the englacial one, I would have expected it vice versa. Perhaps it would be more straightforward dealing with temperature anomalies making the altitude correction obsolete (in this case one may use as well the more representative data compiled for the Colle Gnifetti grid in the HISTALP data base) - the englacial temperature is expected to lag the surface temperature signal by some years or so, this fact should be mentioned, at least. - the way the result are displayed leaves unclear if the borehole loggings refer to different positions or a common one. Thus, I wonder if the temperature effect might be biased by a spatial effect or not? - Perhaps, the extraordinary year 2003 might be mentioned (not standing out in the atmospheric record but expected to have considerably added latent heat to the firn body)

Minor suggestions and comments

1) In the Abstract it is stated: "Air temperature records from the Jungfraujoch high-

altitude station (MeteoSwiss-Station) from 1980 to 2008 show a mean annual increase of 0.05 °C per year, indicating that the amount of infiltrating and refreezing of melt water at Colle Gnifetti has increased since 2000." I recommend re-wording here, since this statement is weak, lacks causality and belongs not to the fundamental outcomes of this paper.

2) p 2279, line16: The expression "in the uppermost firn layer" - is unclear, I guess the authors mean confined to the uppermost annual layer

3) p 2280, line 26: Replace trace element analyses from ice cores by glacio-chemistry in ice cores, etc.

4) p 228, line 2: It is not made clear why a high accumulation rate should be disturbing in this context. Probably it is meant, that a high accumulation rate comes along with a high balance velocity, however, this would be already included in the before mentioned ice flow disturbance. Perhaps, the time coverage of the borehole thermometry is the author's concern?

5) p228, line 7: In view of firn/ice transition depth the reference Schotterer et al 1981 is not up-to-date enough. Depending on accumulation rate, respective depths range from 27m to 43m.

6) p228, line21: The sentence is misleading in saying , " we bring all existing ...data together".

7) p2283, line14: Better to use mechanical instead of electrical for the drill characterisation, change the respective row in table1 accordingly.

8) p2284, equation 1: The denominator should be 4πKt instead of 4πK.

9) p 2286, sentence starting in line 8: The mentioned "a little bit further down the saddle" is clearly associated with much less wind actions accordingly decreasing the (latent) heat flux out of firn especially during potential melt conditions. Thus, aspect or altitude (temperature) appears to be less important for that sudden change.

10) p2287, line15: the given 0.05°C warming should be higher by one order of magnitude or instead related it to the annual rate.

11) p2289, sentence starting in line3: It is not straightforward that polar firn areas subject to a comparable (mean annual) temperature as Colle Gnifetti would be comparable as well in their thermal regime and sensitivity to warming trends. This is simply because the seasonal and diurnal insolation cycles are largely different between these locations. For example the Academii Nauk ice cap at 80°noth clearly belongs to the cold infiltration zone, though MAAT is -15,7° C (thus somewhat lower than at Colle Gnifetti) while, as expected, the firn temperature is significantly higher reaching -10°C at 10m depth (Fritzsche et al. 2005, Annals of Glaciology, 42, No 1, 361-366)

12) Table 1 and table 2 listing temperature logging site contains the deep borehole B95-1 but not the down stream B95-2 one, though temperature loggings are performed here as well.

13) Fig.1 is somewhat confusing: There are more sites indicated than addressed in the paper, so distinguish visually the available from those reported in this paper, or instead skip the sites not addressed here and differentiate the old and novel ones, only. The only Grenzgletscher site reported here should be indicated as well.

14) The captions of figures 2 and 3 are not clear. Indicate there the meaning of the double notations at single borehole positions.

15) Again the caption of figure 4 is not appropriate for immediately conceiving what is shown. I guess that the Colle Gnifetti site is common to all profiles, but I cannot attribute the measuring year to the profiles without consulting table 1 or being advised on the profile code.

16) It is not obvious to me what the essential difference between fig. 4 and fig. 5 might be. Strictly speaking, what is the reason distributing the old-new Colle Gnifetti comparisons on the two figures as it was done?

Interactive comment on The Cryosphere Discuss., 4, 2277, 2010.