The Cryosphere Discuss., 8, C2509–C2519, 2014
www.the-cryosphere-discuss.net/8/C2509/2014/

The Cryosphere
Discussions
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Interactive comment on "Warming permafrost and active layer variability at Cime Bianche, Western Alps" *by* P. Pogliotti et al.

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Received and published: 28 November 2014

General comments of Referee#2

Both Data and Methods and Results are presented in an understandable way. The measured data are interesting and highly relevant for the research community - and relevant for the readers of The Cryosphere. However, and here is my main objections; several points from the interpretations and conclusions presented are well known from the existing literature. The presented analysis are quite simple and do not reach the level of most recent knowledge/understanding. The potential for new insight is large but not fully utilized.

Dear Referee#2,
we are grateful for the positive consideration about the manuscript and the analysis. We carefully considered your main concern testing some of your suggestions and deepening the analysis on GST and snow cover: two figures (fig.2 and fig.5) and one paragraph (sec. 2.4) were thus added to the revised manuscript. In the following we discuss your comments and how the manuscript was modified to take into account your suggestions. We are confident to have fully answered all questions and incorporated most of your recommendations in the revised paper.
All the modifications are reported in red in the revised manuscript. In the following, your comments are in bold while our responses are in italics.
Best regards,
Paolo Pogliotti and co-authors.

Major comments

1. The authors present an extensive list of existing and relevant literature in their introduction and try to put their work in a theoretical context. However, the Introduction in its present form is too long and unfocused. It is not obvious from the first paragraphs what their main research question(s) is. Why is it interesting? The introduction should summarize the relevant literature so that the reader will understand why you were interested in the question(s) you asked. In my point of view two to four paragraphs should be enough.

We agree with referee#2 that introduction was too long and unfocused. Thus we deeply reworked the introduction highlighting the main research questions and removing not relevant parts. Modifications on the revised manuscript: from P3.L3 to P4.L20.

2. The site in general appears to be reasonably homogeneous, but (A) it should be both interesting and possible to do more in-depth analyses on how the timing and the duration of the snow cover influence your ground surface temperature data. In addition the influences of other variables than snow cover on ground surface and ground temperature variability should be analyzed. (B) I suggest that the authors make more sophisticated analyses to get new insights into e.g. how the spatial and temporal variability of ground surface temperatures are in relation to several of the climatic parameters they have measured at the site - such as air temperature, solar radiation and precipitation (in- fluencing e.g. soil humidity) when the ground is not snow covered. (C) And how does the inter-annual variability of both air temperature and snow cover influencing your observed warming trends?

2A. We want to thank referee#2 for this constructive comment: we added a new paragraph (2.4) and 2 new figures (Figures 2 and 5) to answer to the question: "how timing and the duration of the snow cover influence your ground surface temperature data". In particular we looked at the influence on MAGST of snow cover duration and air temperature when ground is snow-free. For doing this analysis, we had to work only on snow-covered *nodes applying the method of Schmid et. al, 2012 to determine snow cover duration. Modifications on the revised manuscript: from P8.L16-23, from P11.L17 to P12.L2, P16.L1-4, P21.L16-17, Figure 2, Figure 5.*

2B. We acknowledge that an in-depth analysis of how spatial and temporal variability of ground temperature relate to meteorological drivers would be relevant. We partially answered to this suggestion by analyzing the impact of mean air temperature during snow free days on MAGST (see figure 5 panel B in the revised manuscript). Unfortunately spatially distributed measures of the other suggested climatic parameters (i.e solar radiation and precipitation) were not available. Thus we limited our analysis on the effect of air temperature. Moreover a specific in depth analysis of the impact

climatic variables on GST data was beyond the main scope of this paper.

2C. We thank the referee#2 for this question pinpointing that we did not clarify enough that warming trends detected are not affected by inter-annual variability because a seasonal detrending was applied before SS and MK. In order to better clarify this point we added a sentence in the discussion. Modifications on the revised manuscript: P18.L17-20.

3. The English need some smoothing and corrections.

We sent the manuscript to a mother tongue for checking the English. We are confident that the revised manuscript is improved.

Specific comments

4. Title The (sub)region/term "Western Alps" may not be clear for all readers from e.g. America. I suggest writing "western European Alps".

The title has been changed in accordance with this suggestion.

5. Abstract L2, P4034, L2: Suggest to include "Italian" or "the Italian side of the Western Alps", e.g. write: ". . .permafrost at the Italian monitoring site Cime Bianche (3100 m a.s.l.),Western Alps."

The sentence has been changed in accordance with this suggestion. Modifications on the revised manuscript: P2.L3-L4.

6. P4034, L3: ground temperature observations. Modifications on the revised manuscript: P2.L4

We corrected the typo.

7. P4034, L5-8: You write: "The analysis aims to quantify. . .(iii) the warming trend of deep permafrost temperatures". Since this paper is the first synthesis on the state and recent evolution of permafrost at Cime Bianche, "warming" or "cooling" of the permafrost is a result itself. In L16-17 you conclude that: "The analysis of deep temperature time series reveals that permafrost is warming". . . Thus suggest that you in L8-9 replace "(iii) the warming trend of deep permafrost temperatures" with "(iii) recent (or present?) temperature trends in deep permafrost".

The sentence has been changed in accordance with this suggestion. Modifications on the revised manuscript: P2.L7

8. P4034, L11-13: Is the accuracy in your measurements so high that the use of one centimeter can be justified here? As far as I can see the spacing of the thermistors around the ALT in DP is two meters! The use of a simple interpolation between two thermistors having two meter spacing for the determination of ALT based on the 0- isotherm. introduce a quite large uncertainty.

We agree with referee#2 comment. See the answer to comment 10 for our detailed

answer. Modifications on the revised manuscript: P2.L12.

9. (A) Data and Methods P4039, L5-6: Did you do any calibration of the thermistor chains before installation? As far as I know such thermistors have $\pm 0.1^{\circ}$C accuracy without calibration. Some additional information about this would be useful here. (B) You also mention sensor noise at P4041, L12-13. The whole setup with datalogger and possible external noise sources etc. should be included when talking about absolute accuracy.

9A. The calibration of the thermistor chains was performed by the manufacturer few days before the installation. About the accuracy, the values cited are those we have on the technical documentation received by the manufacturer. Modifications on the revised manuscript: P6.L6-7.

9B. About sensor noise this is present on deep sensors where temperature changes are very small. The amplitude of such noise is $\pm 0.01^{\circ}$C that is the sensor resolution. We think that this is mainly an electrical noise related to the resolution of differential channels of datalogger (campblell CR800) used to read voltage values. This noise is smoothed with a running mean and we think that it does not affect our results. Modifications on the revised manuscript: P8.L7-9.

10. P4040, L20-26: Please give some additional information about accuracy of the ALT determination used here (cf. my point above (P4034, L11-13)), in the light of especially the spacing of your thermistors and their accuracy.

We thank the referee for this comment. We analyzed the uncertainty of ALT estimation by propagating the error associated to temperature measurements. This analysis was

run on daily mean temperature data. For each day we first identified the two nodes encompassing the ALT. We then generated 500 random temperatures for these two nodes and linearly interpolated between them to calculate ALT. Standard deviation of the resulting 500 ALT values was used as an the actual ALT uncertainty for that particular day. The same was done for all days. Noise was generated according to three different amplitudes, ± 0.01*,* ± 0.02 *and* $\pm 0.05^{\circ}$*C which are respectively the resolution, the relative accuracy and the absolute accuracy of the sensors. As expected the uncertainty increases with the increases of noise. The results are reported in the figure 1 (end of this document). Considering the worst case, that is assuming a noise of* $\pm 0.05^{\circ}$*, the uncertainty of the ALT estimation is* $7.5 \pm 1\,\mathrm{cm}$*. As a consequence, in the revised manuscript, we rounded to the first decimal all the ALT values and added a sentence. Modifications on the revised manuscript: P7.L21-23.*

11. P4046, L18-21: did you compare this tendency with climatic data, e.g. air temperature and snow cover? Variable snow cover may be responsible for some of the inter-annual variability observed in the upper permafrost layers, but also for the observed warming trend at greater depths.

We did not compare temperature trend with short term variations of air temperature and snow data. We are aware that the observed warming trend could be the results of both long term warming and short term inter-annual variability. But given that i) due to a lack of long term observations, it is not possible to disentangle the contribution of both long and short term components ii) the impact of climate data was not the main focus of this paper, we preferred to focus on the quantification of the warming trend and the estimation of related uncertainties. Moreover, as previously underlined in the answer number 2C, the seasonal detrending applied before the trend calculation has the specific purpose of removing these short-term fluctuations .

12. P4046, L27-28: during some warmer years or under/after a longer warm period the warming rate will always be higher near the upper permafrost layers than in the deeper part. Thus, please rewrite this sentence.

We thank the referee for this comment. We rewrote this sentence. Modifications on the revised manuscript: P13.L26-27

13. P4047, L5-6: I cannot see that your results supports that permafrost is "degrading". The term "Permafrost degradation" is normally expressed as a thickening of the active layer, a lowering of the permafrost table, a rising of the permafrost base, or a reduction in the areal extent or the complete disappearance of permafrost". Thus I suggest to better write: ". . .that permafrost at Cime Bianche is warming and that significant positive warming rates are reported at all depths".

We thank the referee for this comment. We rewrote this sentence. Modifications on the revised manuscript: P14.L2-4.

14. P4054, L26 to P4055 L8: this is also reported from several other studies, which are included by the authors in their reference list (see my general comments).

Looking at alpine literature, this is one of the first studies systematically analyzing and quantifying the small-scale spatial and inter-annual variability of both GST and ALT. The estimation of uncertainties is rarely reported in permafrost studies and we think it is fundamental for comparisons between sites. Moreover Cime Bianche site provides new data from the southern side of the Alps, a poorly represented region at European Alps level.

15. P4055 L11-12: I cannot see that your results supports that permafrost is "degrading" (see my previous point. And "observed warming rate exponentially decreasing with depth" is the normal case, and should not be a surprise.

We thank the referee for this comment. We rewrote this sentence. Modifications on the revised manuscript: P21.L26-27

16. Table 2, P4066: ZAA with two decimals – can it be justified? Here the spacing of the thermistors is one meter (see my previous points).

The procedure of uncertainty estimation used for ALT has been replicated for ZAA (see reply to comment 10 for details). The results obtained for ZAA are very similar to those obtained for ALT. As a consequence, in the revised manuscript, all the ZAA values have been rounded to the first decimal. Modifications on the revised manuscript: P31.Tab.2

17. Figure 5, P4071: Especially the minimum profiles are not very smooth. How do you explain this, in the light of the apparently high accuracy of your measurements? Is it real and may be due to lithological contrasts, or variable ice-and water content and/or latent heat effects? Or is it due to non-calibrated thermistors (cf. my point at P4039)?

Since thermistors were calibrated few days before field installation, we are quite confident that instrumental issues are not responsible for unsmoothed temperature profiles. Weathering or bedrock fracturations and variable ice-water contents may both determine the observed behavior. Data in our possess does not allow to elucidate the reason of this pattern. However this pattern is not particularly uncommon in mountain permafrost (see PERMOS, 2013").

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C2509/2014/tcd-8-C2509-2014-supplement.pdf

Interactive comment on The Cryosphere Discuss., 8, 4033, 2014.

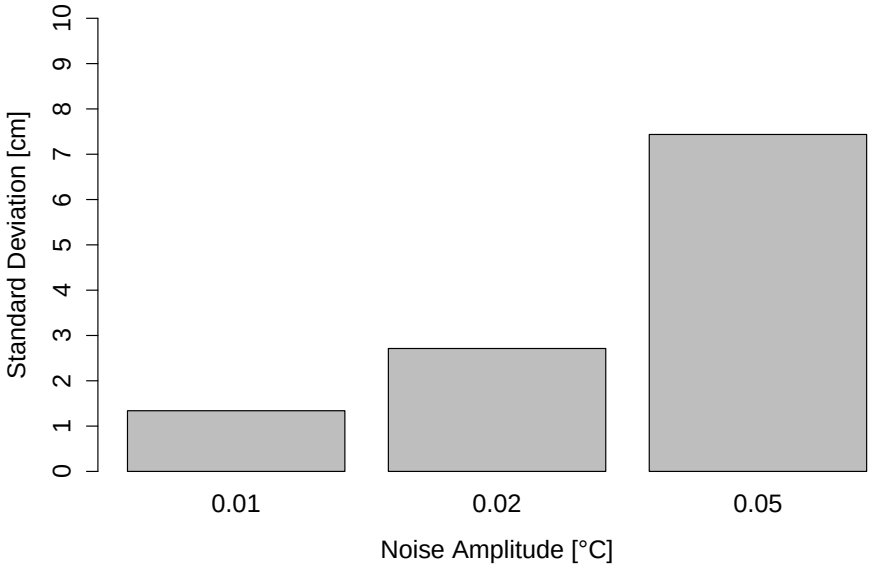

Fig. 1. Uncertainty of ALT estimation as a function of simulated sensor noise.