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Interactive comment on "Thermal characteristics of permafrost in the steep alpine rock walls of the Aiguille du Midi (Mont Blanc Massif, 3842 m a.s.l.)" *by* F. Magnin et al.

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Author's response to reviewers' reports

Paper title: Thermal characteristics of permafrost in the steep alpine rock walls of the Aiguille du Midi (Mont Blanc Massif, 3842 m a.s.l.) Authors: F. Magnin, P. Deline, L. Ravanel, J. Noetzli, P. Pogliotti

Dear Handling Editor: Dr. Tingjun Zhang, dear reviewers: Dr. Andreas Hasler and

anonymous,

All the authors deeply thank you for having thoroughly read our submitted paper, and for the encouraging comments and relevant remarks that you provided.

All these remarks and suggestions have been considered to produce the revised version of our paper and we think that the manuscript has been significantly improved. We hope that it now satisfies the standards for a publication in The Cryosphere.

Hereafter you will find the detailed answers to your comments, with comments from referees, authors' response and changes in the manuscript (written in red). These changes have been reported in a supplementary revised manuscript in which they are highlighted in yellow. We replied to every comment and question, but some of them do not have any sense any more in the revised version. The text has been subject to major revisions:

1. The abstract now states major observations that confirm previous studies and extent existing knowledge. 2. The introduction is widened with background components, with the scientific goals, previously placed in the site description (former sect. 2.2), and with specific research questions. 3. The site description section is restricted to a single section (former sub-sect. 2.2. moved in the introduction of the revised version), but is widened with complementary measurements not used in this study (formerly in method sect. 3.2). 4. In the method sections, titles are adapted; sub-section 3.2 is reduced and only presents complementary measurements used in this study. Table 1 now contains information on the snow thickness at the logger locations. 5. The design of Table 2 is modified (coloured) to improve visibility. 6. In section 5 (rock surface temperature) and section 6 (borehole records), the data description is now separated from its interpretation. In section 5, sub-sections 5.1 and 5.2 are merged in a single sub-section (5.1. Surface Offset patterns). Section 5.3 becomes sub-section 5.2, and 5.3 is built with the discussion lines of the section 5 from the submitted version. In section 6, sub-section 6.3 is totally rewritten with the discussion lines of the sub-

sections 6.1 and 6.2 of the submitted version. Figure 8 and the related discussion of the previous sub-section (6.3. Heat flux end bedrock structure) is removed, but is summarized as an outlook in the new perspective section. Table 3 is completed with air temperature data. 7. The section 7 (conclusions) is reworked in accordance with the re-organisation of the introduction and discussion. 8. An additional section, (8. Further developments), outlines our research perspectives with the presented dataset.

These modifications, aim at satisfying the requirements expressed by reviewers, and we hope that they will allow the paper to be accepted for publication.

Best regards.

Dr. Florence Magnin, on behalf of all the authors.

Response to A. Hasler, Reviewer #2 Reviewer #2: This paper presents an outstanding data set of rock surface and borehole temperatures in extreme topography and analyses them with respect to their topographic and structural setting. The presented study supports and extends existing knowledge on the thermal characteristics of potential rock fall detachment zones in high-alpine permafrost. I agree with Referee #1 that it is highly relevant to The Cryosphere. However, after reading, I had several open questions and was a bit confused by the large amount of information given. On my opinion there are points in the manuscript, especially in the discussion section, to be clarified and synthesized to make the conclusions retraceable. Additionally, one subsection (6.3) needs to be reworked because the interpretation of the presented results is inaccurate. Further, the conclusions may be more specific and distinguish between the confirmation of established knowledge, the support of recent studies and the statement of new hypotheses. Even though there are no fundamental changes in structure and method required, the discussion part of the manuscript needs substantial revision for acceptance. For this reason I recommend accepting the paper after major revisions. Authors' reply: The authors appreciate these relevant suggestions on reorganisation of the paper sections and conclusions, as well as the important remark on the inac-

curacies of the interpretations in the sub-section 6.3. In order to clarify our discussion and conclusions we separated the results from the discussion as suggested in general comment #2 and we hope that with these revisions, the conclusions will be better retraceable. The section 6.3 has been totally rewritten (6.3.Snow cover and bedrock discontinuity control) by replacing the analysis of Figure 8 with the discussion points of the section 6.1, 6.2 taking into account all the comments and advices on these sections (see our replies below). The former content of the section (6.3.Heat flux and bedrock structure) is totally removed and mentioned as a perspective in the section 8. The reason for this choice is that if we rework the section as suggested to discuss the non-conductive processes, we would go beyond the scope of this article.

Reviewer #2, General comment #1: Terms and definitions: Many different terms e.g. permafrost conditions, permafrost temperature regime, thermal regime, temperature characteristics, annual regime are used and it is not clear how they are defined and used in this context. Please simplify. Authors' reply: This is true that these different terms are employed and may confuse the reader. In the revised version, we simplified by only employing “permafrost thermal regime” instead of “temperature regime”, “annual regime”, and “permafrost conditions”. We limited the use of the term “characteristic”. Reviewer #2, General comment #2: Structure: The manuscript consists of the main elements that are typical for empirical field studies such as: A) Problem statement, B) Site description, C) Methods, D) Results, E) Discussion and F) Conclusion. The methods are subdivided into the sections 3 Monitoring systems and 4 Dataset preparation. Section 5 and 6 consist of a presentation and discussion of the results grouped by measurement type (rock surface measurements vs. (lower) borehole measurements). Because other features are apparent in rock surface and borehole temperatures, the discussion in the two sections addresses different topics (reflected in the subsections of 5 and 6). I think this is a possible general structure and does not necessarily require revision. However, within section 5 and 6 many details are described, it is often hard to distinguish between results and discussion and the subsections titles are some times not very meaningful for its content. Further, it is not apparent how the “Complementary permafrost measurements” are considered in this structure. This makes these two sections not easy readable even for readers that are familiar with the topic. It is challenging to attribute the subsections or paragraphs to the conclusions finally stated (section 7). I recommend some reorganization of the internal structure (details below) of section 5 and 6. Authors’ reply: In the revised version we considered these remarks and separated the results description from discussion (see the first authors’ reply) within sections 5 and 6 and gave sub-titles that are more meaningful (5.3. Snow cover and micro-meteorology influences; 6.3.Snow cover and bedrock discontinuity controls). The content of the “Complementary permafrost measurements” is restricted to the only data that support observations. The other installations and data descriptions are merged with the site description in the revised version. Reviewer #2, General comment #3: Abstract: What is new from this study, what confirms recent studies, what is established knowledge (e.g. influence of aspect)? What is the influence of stated “important factors” specifically? It would be nice to answer this to some extent already in the abstract. Authors’ reply: We reworked the abstract taking into account these advices that are really relevant and we hope that the revised version will meet these expectations. The revised abstract contains the following statements: “Analysis of the temperature data confirm previous studies, some of them being demonstrated empirically for the first time: micro-meteorology controls the surface temperature, active layer thicknesses are directly related to aspect and ranged from <2 m to nearly 6 m, warm and cold permafrost (about –1.5°C to –4.5°C at 10-m-depth) coexists within the Aiguille du Midi, resulting in high lateral heat fluxes, thin accumulations of snow and open fractures are cooling factors. Some observations extent existing knowledge: thick snow accumulations warm north faces but cool south faces, possibly inhibit active layer refreezing in winter and delay its thawing in summer. Latent heat consumption due to interstitial water phase changes in bedrock discontinuities possibly dampens the active layer and permafrost changes, whereas an open fractures act as a thermal cutoff in the sub-surface thermal regime.” Reviewer #2, General comment #4: Introduction: Clarify aim / research question in the last section: “Drawing up a detailed description

of (: : :)” is very general. To have more specific questions would make it much easier to read the discussion and the conclusions! Authors’ reply: We moved the research goals described in the section 2.2 (see reply to following comment) of the submitted draft in the introduction to settle the global aims of our monitoring system, to specify those addressed in this article, and to specify our research questions. We extended the background in the introduction to build up research questions. Reviewer #2, General comment #5: Study site: The scientific aims of the monitoring program (section 2.2) do not belong to the site description in my opinion. They would be better placed (merged) within the introduction (aim of the study). Authors’ reply: We totally agree with this remark and as explained in the previous reply, we moved the section 2.2 in the introduction.

Reviewer #2, General comment #6: Description of installation (Monitoring system): For the interpretation of the nonconductive heat fluxes at the borehole sites it would be important to know if/how the space between the drilling diameter (66 mm) and the casing tube (40 mm) is filled. Could water enter and refreeze? A clarification on page 2837 – line 7 may be helpful and possibly this point needs to be considered in the analysis (section 6). Authors’ reply: This is a very relevant remark and in the revised version we precised: “Space between the drilling hole and the casing tube has not been filled.” in the section 3.1. However, the discussion on the non-conductive heat fluxes is largely reduced and this precision is more relevant now for the installation description than for the discussion for the revised version. In the discussion of the revised version (sect. 6.3) we only precised: “Nevertheless, despite this dominant cooling effect, water percolation can occur along the fracture and heat advection could locally warm the rock (Hasler et al. 2011b), but no signal is detected in such sense in the temperature of BH_N.”

Reviewer #2, General comment #7: Section 3.2 is a mix between a site description, a method description and the results of this method. If these methods are a key element for the analysis (section 5 and 6) this section should be restricted to the description of the method and how the method will be used in the present study. Otherwise the content of this section may be introduced into the site description. It is unclear how and where it is applied in the present study. Authors' reply: The authors totally agree with this remark. As the scope of the paper is not only to present the data but also to present the site, we kept section 3.2 and moved it in the site description as suggested. We reduced the description of these complementary measurements (less details) but completed them with supplementary installations (crack-meters and ground-penetrating radar measurements). Only the data described and discussed in this paper data appear in the method section 3.2 of the revised version.

Reviewer #2, General comment #8: Data preparation: The gap filling for data with pronounced diurnal cycles by linear interpolation over days from the two nearest data points could add considerable errors even to annual means. This gap filling for "short gaps" should be applied only if the gap is shorter than the typical wavelength. For rock temperatures at 0.1m / 0.3 m depth and one gap of 5 days such as the case, an error of the MAGST up to 0.2 / 0.1°C may be expected. The errors of several gaps within the same year may then add up. Are there many gaps filled by this procedure in the data? Wouldn't it make sense to use a smaller threshold for "short gaps" (e.g. 0.5 days)? Authors' reply The gap filling has been performed after daily aggregation to keep the same basis as air temperature records provided by Météo France at the daily resolution. Thus, no effects of diurnal cycles are expected. The daily aggregation is performed only if all the data from the day are recorded. However, this has not been mentioned in the submitted version. We added this precision: "First, daily means from rock temperature time series were calculated for complete days of records. Then, short gaps (< 5 days) were filled by linear interpolation...." The rock temperature data were relatively clean, and small gaps were only a few. For N1, N2, S1 and S2 only 1 day by time series was filled with linear interpolation in 2007. For E2: two gaps, one of 3 days and another one of 1 day were filled with linear interpolation in 2009. For S3, only 1 day in 2009 was filled with linear interpolation and for W2, only 1 day in 2009. However, the air temperature time series was more discontinuous. In the

worst case, 36 days spread in 13 different gaps <5 days were filled (2009). This is true that so many days may have affected the final annual mean. However, since all surface offsets were calculated with the same air temperature records, the comparison in between sensors presented in the study may not be affected by these gap filling strategy. Interannual changes are only discussed by comparing the sensors, but not in absolute values, so here again our interpretation may not be biased by the possible error in MAAT or mean seasonal air temperature value. Concerning the gaps < or >1.5 month, one precision is also missing in the submitted version. This is true that no gap >1.5 month was filled but this threshold is applied by year. So the gap from December 2007 to February 2008 was filled with linear regression because it does not constitute a gap >1.5 month neither for 2007 nor 2008. In the revised version, we clarified this point: "longer than 1.5 months per year". We changed the design of Table 2 for more visibility. The "white areas" indicate gaps that were not filled, and thus neither seasonal nor annual means where computed. The red areas (formerly stripped areas) indicate the gaps (<1.5 month a year) were filled only for calculation of the annual mean but were not used for calculation of the seasonal mean, as indicated in the table caption. For the revised version we precise "1.5 month per year" in the table caption.

Reviewer #2, General comment #9: Results and discussion (Rock surface temperatures, Borehole temperatures): These two sections contain many details on the observations and their possible causes. Often each observation is explained directly by one particular process, which leads to repetitions regarding these processes. The confirmation of existing knowledge and statement of new hypotheses alternates and makes it hard for a reader to follow what is being supported or answered. Further, it is confusing for a reader that does not know the location and all the sensor labels (given the redundant labelling; see detailed comments) to follow this reasoning and get a coherent picture of the observations. As a result the conclusions drawn seam a bit arbitrary. May bee it would help to separate results and interpretation (discussion). Alternatively a slightly different structure (e.g. ordered by "influence of snow cover", "influence of rock discontinuities", etc.) may help to organize the statements around processes instead of data aggregation levels (annual vs. seasonal offsets). A brief intro to the discussion of each topic (such as done e.g. for aspect control in near-vertical rock on p.2840, l. 1–7) would help to distinguish between the confirmation of previous studies and new statements. As an example some comments on section 5.1: In the subsection Annual surface offset patterns the effect of snow cover on the spatial and temporal variability of the SO is investigated. This is not clear by the title. On page 2841 many statements on the influence of the snow cover on the interannual variability of the SO are made based on the description of figure 4a and the SO difference of the two years 2011-2012. The section contains e.g. a nice reasoning why the south facing snow covered sensors have smaller SO-variabilities. However, it appears on figure 4a that for other years this would not be true (but the figure does not easily support such an analysis). At the end of the section the main observations are summarized and the following conclusion is drawn: "These findings show that the effects of snow cover and micrometeorology can differ greatly between different aspects." I completely agree with this general statement, however it is not novel and the detailed reasoning above is not required for that. The final conclusions (section 7) do not simply summarize these comprehensive statements but go back to more detail again. This makes it hard to retrace how these final conclusions are drawn. Regarding section 6.3 and the interpretation of figure 8 I have clear doubts about the interpretation made: The inflection in the temperature profile BH_N (figure 7) is indeed a very nice finding! Congratulation! However, figure 8 is not suitable to show heat flux discontinuities (or non-conductive heat fluxes) as stated in the text. After formula 1 it is simply the temperature gradient along the borehole assuming a constant thermal conductivity. This assumption is inappropriate for fractured rock because the apparent conductivity is much lower across a fracture. The values shown in figure 8 BH_N at the depth of the fracture are therefor not realistic conductive heat fluxes. Non-conductive heat fluxes are not shown in the figure. For the interpretation of figure 8 BH_N the exact locations of the thermistors should be considered. From what is shown in figure 8 one can not easily conclude on a "heat input" or "localized warming". The yellow bubble around 2.5m depth in summer

may simply be a result of the large temperature gradient across the fracture when the rock surface is warmed. The yellow and blue bubble above/below that depth in winter may be a heat sink causing reverse heat fluxes at both sides of the fracture. Probably a plot with temperature profiles for different points in time including thermistor depths and fracture may be more appropriate to understand what happens around the fracture. Alternatively, the data could be analysed with a heat conduction scheme and estimate heat sources/sinks (cf. Hasler 2011; p. 157; http://opac.nebis.ch/ediss/20121355.pdf). I suggest to rework this section and the respective conclusion. Authors' reply: We considered this remarks in the revised version and have separated the results and interpretation. In the rock surface temperature section, we gathered the annual and surface offsets description (sect. 5.1), keep the daily temperature records from AT and snow-covered sensors in its proper section (sect. 5.2 instead of 5.3) and moved all the discussion points from these two sub-sections into a proper discussion sub-section 5.3 (see reply to general comment #2). All the detailed comments related to these (sub-)sections have been answered (below) but some replies will not be relevant any more with the here proposed re-organisation. Concerning the remark on the interannual variability of SO on the south face, our statement is that snow covered sensors have higher interannual variability than snow free sensors on the north face but not on the south face. This statement is based on the years of records where a comparison is possible: from 2010 to 2011: S3 = -0.8°C, BH_N = -1.2°C, BH_S= -0.5°C; for 2011-2012: S3 = +0.3°C, BH_N = +1.1°C, BH_E = +1.1°C. According to these data, that are all the available data, the snow covered sensors always have lower interannual changes on south faces than on north faces. So the statement to which the remark refers ("it appears on figure 4a that for other years (than 2011-2012) this would not be true") is contradicted by the data. Concerning the fact that these findings are not novel: this is true for certain points (e.g. snow cover cools south rock faces), but in the other hand, additional information are given on the potential effect of snow on steep alpine bedrock (e.g. it can have a warming effect on north faces, while only the cooling effect of snow was considered in steep bedrock permafrost in previous studies). We hope that in the revised discussion section 5.3, the confirmation of previous statements and the extension of the existing knowledge are more clearly distinguished and that the presented reasoning appears more relevant to the reader. Finally, concerning the section 6.3, we deeply thank you for these interesting and highly relevant comments and explanations. We thus decided to remove this reasoning in the revised version and built the discussion section (sect. 6.3) with the discussion lines of 6.1 and 6.2 from the submitted manuscript to discuss the snow and the bedrock discontinuity controls on active layer and permafrost thermal regime. We considered the suggestion of the T(z) profiles for revision as suggested. However, we realised that they would not any bring new relevant insight compared to Figure 7 so we didn't replace Figure 8. The study of non-conductive heat transfers will be part of the research perspectives mentioned in the conclusion and they will take into account your remarks and explanations: "The BH_N fracture constitutes an opportunity to investigate non-conductive heat transfers with adapted method such as a heat conduction scheme."

Reviewer #2, General comment #10: Conclusions: The conclusions are quite descriptive (describing the observations), which is good. Explanations for these observations are sometimes vague (see detailed comments). At the end of the conclusions it is stated that these conclusions confirm other studies and provide new insights. It would be useful to know what it the case for which conclusion. To base the conclusions on a clearly stated problem statement (or research gap; see comment on introduction) would help to do so. It is hard to attribute the different conclusions to statements in the discussion. They relate to the topics of some subsections but they do not fit the final statements. Recommendation: I recommend structuring the discussion part by research questions that should be briefly outlined in the introduction. This does not require a change of the general content but helps the reader to follow a storyline and to understand how the conclusions are drawn. However, several details in the discussion may be rethought and possibly left away (see detailed comments). Authors' reply: In the revised version we followed these recommendations, from the research questions in the introduction to the discussion sections and the conclusions (see replies to gen-

eral comment #4). We hope that the revised version will meet these expectations. We left away the content of the section 6.3 of the submitted version. Reviewer #2, Detailed comment p. 2832 l. 2-5 This sentence states "thermo-hydro-mechanical processes" as a crucial factor but later that (these?) processes are poorly understood. Further it is not clear what "such locations" and "they" refers to in this sentence (Permafrost or rock wall stability are not locations). This is a confusing sentence to start an abstract. Authors' reply: We apparently missed some language subtlety and to avoid these confusions and misunderstood in the first sentence of the manuscript, we reworked the abstract by changing some confusing words and expressions. We kept this first sentence that introduces the general context and motivations of this work: "Permafrost and related thermo-hydro-mechanical processes are regarded as probable factors in high alpine rock wall stability, but a lack of field measurements means that the characteristics and processes of rock wall permafrost are poorly understood." Reviewer #2, Detailed comment p. 2832 l. 19 "below -4°C": There are other sites with temperatures in this range (e.g. PERMOS 2013; but no boreholes). Precise or leave away. Authors' reply: We precised: "...from an Alpine permafrost site where borehole temperatures are below –4°C..." Reviewer #2, Detailed comment p. 2833 l. 17: "cause a cooling of up to 3°C in permafrost temperatures". Further this statement applies to "radiation exposed" rock faces (see Hasler et al. 2011a; last conclusion). Authors' reply: We thus added this precision in the revised version: "....thin accumulations of snow on micro-reliefs and cleft ventilation may both cause deviations of 1°C (shaded faces) to 3°C (sun-exposed faces)..."

Reviewer #2, Detailed comment p. 2833 l. 18-22: reformulate. microtop. and structure does not only effect surface layer. Authors' reply: We replaced "surface layer" by "permafrost distribution" Reviewer #2, Detailed comment p. 2833 l. 24: "models" instead of "modelling strategies" Authors' reply: Done

Reviewer #2, Detailed comment p. 2834 l. 7ff.: "Four years : : :" Isn't this rather a conclusion? Authors' reply: The last part of the sentence may effectively belong to a conclusive section. In the revised version, the study is announced as following: “We analyze seasonal and annual patterns of surface temperature, active layer and permafrost thermal regime from eight years of surface records and four years of borehole data discussing our results at the light of former studies and providing new empirical evidences of the poorly known effect of snow and fractures on permafrost in steep rock walls.” Reviewer #2, Detailed comment p. 2837 l. 8: Did the drilling water enter the fracture system? Could this water influence the temperature field after the drilling? Was there a related temperature decays in the first months of the temperature records? Authors' reply: Yes, it did enter the fracture system but rapidly run out. Note that the boreholes have been drilled in September 2009 (p. 2836, l 10) but that the thermistors chains have been inserted in December 2009 for BH_S and BH_N, and April 2010 for BH_E (p. 2837, l 12); so we do not suspect any influence of the water on the temperature records and no temperature decays were observed.

Reviewer #2, Detailed comment p. 2838 l. 27: “0.3 m rock temperature” instead of “0.3 m-deep temperature” Authors' reply: Done. For your information, we paid a professional translator to correct this kind of language subtlety. Reviewer #2, Detailed comment p. 2839 l. 18 “Rock surface temperatures” would be simpler. Unclear what “temperature characteristics” are Authors' reply: We changed the title as proposed in the revised version. Reviewer #2, Detailed comment p. 2840 l. 02 Where are these 12°C from? Source? PERMOS, 2013 p.11 states 10°C. Authors' reply: This was a mistake, we corrected: 12°C was replaced by 10°C. Reviewer #2, Detailed comment p. 2840 l. 07ff: “This is because : : :” is a discussion of Allen et al. and not result here. Authors' reply: This is true, so we changed the sentence to suggest this statement as an interpretation rather than as a result. This sentence is now in the discussion subsection (5.3): “In New Zealand, thus at a similar latitude of the Alps, Allen et al. (2009) reported a maximum ASO value of 6.7°C. Such a lower value can be ascribed to a reduction of direct solar radiation due to the influence of the oceanic climate and related frequent cloud cover.” Reviewer #2, Detailed comment p. 2840 l. 14: If surface offset (SO) is defined as above (MAGST – MAAT) a seasonal surface offset (SSO) could

be a confusing term for the difference of seasonal means because it is not described in the methods. It could be mistaken with the intra-annual variation of the SO. To be very clear you could explain that it is meanTrock_season –meanTair_season Authors' reply: For more precision we did so in the revised version: “... and seasonal SOs (SSO) using seasonal means of rock surface and air temperature of the season for winter (December to February)...” Reviewer #2, Detailed comment p. 2840 l. 26: e.g. “S3 (no. 6)”: to avoid this double labelling would be nice (see comment fig.4) Authors' reply: The authors totally agree with this suggestion and actually prepared a first version of this figure with the sensor labels instead of numbers. This first version was not convincing but since then, the figure has been reworked, and finally the figure seems more convincing (see reply to comment fig.4).

Reviewer #2, Detailed comment p. 2840 l. 28: What about latent heat of snow fusion? Authors' reply: This is true that this factor deserves consideration and was not clearly mentioned in the submitted version. However, latent heat of snow fusion was mentioned in the section 5.3 through the zero curtain effect as at this point it is clearly suggested by the data. As in the revised version we gathered all components from the discussion in section 5.3, the latent heat of fusion will be mentioned as the same time as the albedo effect: “...This cooling effect results from the combination of (i) a thin snow cover with negligible thermo-insulation, (ii) an increase of surface albedo, (iii) and melt energy consumption (Harris and Corte, 1992; Pogliotti, 2011)...”

Reviewer #2, Detailed comment p. 2841 l. 4: “no. 10 and 12”? no. 11 is snow covered, right? Authors' reply: Yes this is true, that was a mistake. With the application of the lapse rate this sentence doesn't have any sense any more and has been removed from the revised version. Reviewer #2, Detailed comment p. 2841 l. 4: are these 100 m vertical difference corrected by a lapse rate? Authors' reply: It was not in the first draft but it has been corrected in the revised version (see detailed reply to reviewer #1, specific comment P2838, l 11-13). Reviewer #2, Detailed comment p. 2841 l. 7ff, p. 2845 l. 1 and p. 2850: The term interannual variability of the surface offset needs some explanation for not being confused with interannual variability of the MAGST or MAGT. Interannual variability (or changes) alone is not sufficient in this context. The difference of the means of 2 years should not be called interannual variability. And, can a variability be negative? Authors' reply: This is true that some explanations would be relevant. In the revised version, interannual variability of SO will be used for data description section (5.1), but in the discussion section (5.3), we explain the meaning of the change in SO in terms of change in MAGST: "On the north face, the higher ASOs at snow-covered sensors (BH_N) compared to at snow-free sensors (N1 and N2) show that the thermo-insulation of snow significantly increases the MAGST. On the south face, the lower ASOs at snow covered sensors (BH_S and S3) compared to snow free conditions (S1 and S2) indicates a lowering of MAGST due to snow." We agree that the means of 2 years should not be called interannual variability. However, in the submitted version, interannual variability is only described and discussed with annual surface offsets that are not averaged, and concerning the seasonal surface offsets (that are averaged over several seasons) we only describe the spatial pattern.

Reviewer #2, Detailed comment p. 2841 l. 9-10: ": : : depend mainly on : : :": where can we see that? Fig. 5? Authors' reply: This is true, this is not visible on Figure 5; we considered it as common knowledge. However, this sentence was a part of the discussion that has been merged in the discussion section and doesn't appear like this in the revised version.

Reviewer #2, Detailed comment p. 2841 l. 18: S2 is snow free? But difference (var.?) is not smaller. Authors' reply: Yes, S2 is snow free, and yes the difference is not smaller than snow covered sensors of the same aspect. And this is exactly what the text aims at pinpointing: on south faces, snow covered sensors do not have higher inter-annual differences than snow free sensors, unlike shaded faces. And here the sentence aimed at shwoing that snow free sensors had opposite trend than snow covered sensors.

Reviewer #2, Detailed comment p. 2841 l. 21: if insulation is the dominant process Authors' reply: This is another relevant comment, and we hope that in the revised

draft, with a specific section dedicated to the discussion, we didn't omit other possible processes and clearly explained the relative influence of these different processes in the annual energy balance. Reviewer #2, Detailed comment p. 2842 l. 6: phrase: ": : : surface temperatures of snow covered sensors were: : :" Authors 'reply: Yes, and the phrase will be merged in the new discussion section (5.3). Reviewer #2, Detailed comment p. 2842 l. 10: What means "consistent with their aspect"? Where can we see that? Authors' reply: The term "consistent" is possibly not appropriated. The sentence meant that "interannual changes depended on aspect as shown by the similar changes observed for sensors in the same aspects and different interannual changes from one aspect to another (cf. p 2841, l 29 – p. 2842, l 1-4). However, this conclusive sentence from sect. 5.1 is merged and reformulated in the discussion section (5.3) of the revised version.

Reviewer #2, Detailed comment p. 2842 l. 20 ff repetition regarding the effect of snow cover Authors' reply: The authors don't have the feeling that this sentence repeats previous statement. It states that seasonal SOs of snow covered sensors is opposite to those of snow free sensors. Previously, it has been stated that interannual variability of snow covered sensors is different from those of snow free sensors (sect. 5.1) and section 5.2 starts with the description of SSO patterns for snow free sensors. Anyway, here again such confusion should not occur any more in the revised version with description and discussion distinction.

Reviewer #2, Detailed comment p. 2843 l. 3-5: This is really well established knowledge and may belong to an introductive paragraph. Authors' reply: This is true that it is well known, even though the warming effect of snow is not usually considered in steep rock walls. We took into account this remark in the revised version and placed the common knowledge in the introductive paragraphs of the discussion sub-section (5.3) to better highlight our specific observations.

Reviewer #2, Detailed comment p. 2843 l. 5: Is the observed N-S difference only a result of radiation or is the thickness of the snow cover different as well? (Compare

p. 2844 l. 3ff) Authors' reply: At that level of the description only the aspect (solar radiation) can be discussed. The comparison with p. 2844 l. 3 confirms that the solar radiation is the most probable factor explaining this difference: the smoothing of daily oscillations suggests a snow accumulation that is thick enough to dampen daily air temperature signal (unlike BH_E) and except from a small period December 2012 to March 2013, S3 is not permanently colder than BH_S for instance, so from these observations it is not possible to state about a thickness effect to explain the differences. Reviewer #2, Detailed comment p. 2843 l. 11: "a different effect". Which effect? Authors' reply: "warming" or "cooling" effect, referring to the previous sentence. Reviewer #2, Detailed comment p. 2843 l. 11: Why "smoother than expected"? Authors' reply: "smoother than the expected daily oscillation" means that we were expecting to see the temperature curves following the daily oscillation of air temperature. In the revised version, "expected" do not appear any more as the discussion and description are separated: "... temperature curves of the snow covered sensors are smoothed compared to air temperature oscillation ..." Reviewer #2, Detailed comment p. 2844 ff note: Many similar comments/questions as on last 3 pages appear. I stop this level of detail here. Authors' reply: The confusions and questions likely come from the mixed description and discussion as explained in the general comments. So, we hope that with the revised version and the separated description and revision, such questions and comments will not arise any more.

Reviewer #2, Detailed comment p. 2844 l. 9: According to earlier statements in the manuscript (e.g. p.2841 l. 25ff) such a threshold would depend on aspect. Authors' reply: This question possibly comes from a misinterpretation/misexpression of the discussion lines. Indeed, it is stated that the threshold of the snow thermo-insulation depends on aspect, but rather that the effect of the thermo-insulation on the final annual balance depends on the aspect. On the south face, the warming effect from thermo-insulation still occurs (as long as the snow is thick enough), but weights less than the cooling effects of the albedo and latent heat consumption in the annual balance. However, similarly to other comments, this question should not arise any more in

the revised version because we rewrote the discussion section and took this question in consideration in order to make our statements more understandable. Reviewer #2, Detailed comment p. 2844 l. 18: Fig. 6 is not temp profile. Authors' reply: True, it is replaced by "daily temperature" in the revised version. Reviewer #2, Detailed comment p. 2845 l. 3-4: ": : : comparison : : : is difficult : : :". Why? What is fundamentally different at AdM? Authors' reply: The comparison is difficult because there are only a few sites in steep bedrock such as the AdM in the PERMOS network. In the 2013 reports, only 3/25 sites are indicated as "bedrock" in Table A. 1a of the Appendix: Gemstock 0106, Matterhorn 0205, Tsaté 0104 (even though Schilthorn and Stockhorn are also indicated as rock wall sites on page 1, but indicated as "debris" site in the Table A. 1a). Anyway, we tried to avoid basic comparisons without relevance such as : " the AdM permafrost is colder because the site is higher...". However, in the revised version we made an effort to support our discussion and to enrich the comparison of our data with other sites in the Alps and high latitudes: "In terms of permafrost thermal regime, the BH_N shows deep temperatures colder than -4°C that is a value typical of high latitude monitoring sites such as in Svalbard (Noetzli et al., 2014a) or the warmest boreholes of the continuous permafrost zone in Alaska (Romanovsky et al., 2014). The spatial and temporal variability of ALT is consistent with values reported for Swiss boreholes in bedrock (PERMOS, 2013). For instance, thickness and timing of the ALT in BH_E are similar to those reported at the Matterhorn-Hörnligrat site (3295 m a.s.l, vertical borehole on a crest), with values ranging from 2.89 to 3.66 m between 2008 and 2010, and maximum depth occurrence from early September to early October. In bedrock slopes, active layer thickness changes seem strongly controlled by summer air temperature. During the hot summer of 2003 for instance, the ALT at Schilthorn (2909 m a.s.l) has been deepened by twice, from 4-5 m to > 8 m depht while on debris-covered slopes such as Les Gentianes moraine or the Arolla scree slopes, located in the same area and at similar elevations, any specific thickening has been observed (PERMOS, 2013)." Reviewer #2, Detailed comment p. 2846 l. 21: How "amplitude" is defined? Peak-peak? meanJan – meanJuly? What is exactly shown in Fig. 7b? Authors' reply: The amplitude is calculated from peak to peak. The precision is mentioned in the revised version: “In 2011, the largest amplitudes in daily temperature (peak to peak) at the surface”. Figure 7b shows the “maximum and minimum temperature at each depth” p. 2844, l. 19. But this is true that some precisions are also missing here, so we explained that this is max. min. of the daily temperature recorded throughout the year 2011 in the temperature envelope introduction: “...mean annual Temperature-Depth (T(z)) profiles, and annual temperature envelopes (i.e., the maximum and minimum daily temperatures at each depth in 2011; Fig. 7).” Reviewer #2, Detailed comment p. 2847 l. 6ff: What about transient effects and lateral heat fluxes? Is a not linear profile a sufficient indication for non-conductive processes in this situation? Authors' reply: We interpreted these inflections as non-conductive processes because we knew that they coincide with the fracture depth of BH_N and we extended this interpretation to BH_E because of the similarity in local inflections and consistency through years at BH_E. But we totally agree that this statement is too straightforward. If we extend the sentence: “The linearity of the BH_E profiles indicates that the most important process is heat conduction (Williams and Smith, 1989),...” based on explanations from the same reference, the change in profile linearity would indicate changing conductivity. However, here we face an inflection (a sharp and local change), which is different from the change in profile shape described by William and Smith (1989, p. 95). And concerning BH_S, we did not state about non-conductive processes and we agree that here the transient effect and lateral heat fluxes may be responsible for the profile shape. In the revised version, these interpretations are in section 5.3 and we took into consideration these questions by avoiding the assumption about non-conductive heat transfer from our records. Reviewer #2, Detailed comment p. 2848 l. 1: What is meant with “heat-exchange processes”? Note that advective heat transport by percolating water is not equal non-conductive heat transport (what may be implied with the next sentence). Authors' reply: In p. 2848 l.1 “heat exchange” is not employed (l.1: “6.3 Heat flux and bedrock structure”), so we guess that the question concerns l. 2, and it refers to the exact term employed by the reference that we cite (Hasler et al. 2011b): “Here, we

develop a conceptual model that incorporates the main heat-exchange processes in a rock cleft.” (quoted from Hasler et al., 2011a, Abstract); and this term is largely employed all along the referred article. We used this terms as a general term to refer to different heat transports and to stay as close as possible from the reference. We noted that advective heat transport by percolating water is not non-conductive heat transport. This precision is relevant but the following sentence implies the cold inflection in the profile that we attribute to air ventilation.

Reviewer #2, Detailed comment p. 2848 l. 18: Regarding “Significant heat inputs : : :”: This is a misinterpretation of figure 8: If the (conductive) heat flux is large this does not mean that there is a heat input. Negative heat fluxes are not equivalent to heat loss (See general comments). Authors reply: Thank you for these precisions. Indeed we misinterpreted Figure 8 and we avoided this kind of misinterpretation in the revised version by supressing the Figure 8 and related interpretation.

Reviewer #2, Detailed comment p. 2850 l. 11: : rephrase “spatial distribution of surface temperatures : : :”. Do you mean “pattern of MAGST”? Authors reply: We used “surface temperature”, similarly to the title of section 5 as it concerns as much MAGST as mean seasonal and daily temperature. So we kept the same formulation in the revised version.

Reviewer #2, Detailed comment p. 2850 l. 13: This is not shown in this study. Authors reply: The text p. 2850 l. 13 states: “These monitoring data confirm characteristics predicted by numerical experiments, including the coexistence within a single rock peak of warm and cold permafrost, which generates lateral heat fluxes from warm to cold faces.” And p. 2846 l. 17-19: “Annual Temperature-Depth T(z) profiles (Fig. 7A) revealed different thermal regimes. The AdM's Piton Central has both warm (ca. –1.5°C at BH_S) and cold (ca. –4.5°C at BH_N) permafrost (Table 3).” p. 2847 l. 16-21: “The temperature gradient was negative (ca. -0.1°C m-1) at the bottom of both profiles, in contrast to the positive gradient at the bottom of the BH_N profile. This feature is consistent with numerical simulations showing the role played by lateral heat flux from warm to cold faces in the thermal regime of alpine peaks (Noetzli et al., 2007), and confirms recent simulations of rock temperature distribution in a cross-section through the AdM (Noetzli et al., 2014)." So, the conclusion p. 2850 l. 13 was demonstrated in the text. The mixed description and discussion may explain such confusion and we hope that with the re-working of the discussion sections in the revised version this question would not arise anymore. We kept the same conclusion than p. 2850 l. 13, slightly reworked it, and the text states in section 6.3: "The coexistence of warm and cold permafrost and the opposite temperature gradients between BH_S and BH_N, that likely result from lateral heat fluxes, is in accordance with previous statements deriving from numerical simulations (Noetzli et al. 2007)". Reviewer #2, Detailed comment p. 2850 l. 16-20: I simply don't understand this conclusion. Yes, in near-vertical bedrock the micrometeorology (mainly dependent on topography) controls the SO. In section 5 the state of the art in this regard is outlined. What is the (new) finding here? Authors reply: This is true that nothing is fundamentally new in this observation even though the AdM is the first set of sensors installed so closely with slightly different settings, and with pluri-annual records to allow such conclusion on the different effect of micro-topography. In the revised version the conclusion is divided in two sections, one gathering the confirmations of previous statement and a second gathering the new findings. This conclusion is now part of the conclusions confirming previous studies and reworked in accordance with the discussion (sect. 5.3): "Interannual changes in MAGST are not uniform around a same rock peak even in snow free conditions. This may be ascribed to variable cloud formation from year-to-year." Reviewer #2, Detailed comment p. 2851 l. 1-9: From what I got thin and thick snow cover coincides with N and S in your data. What is the difference between the two points? What means "more consistent with insolation parameters"? Authors reply: In the first point we explain that we can observe this effect of snow depth threshold in the insulating effect, and the second points explains that this insulating effect is of greatest importance in the annual balance of shaded sensors while it is of reduced impact in the annual balance of sun-exposed faces. As this is apparently not clear, we will propose a different

formulation in the revised version, the conclusion will be reformulated as following: "On south faces, a thick (insulating) snow cover may cool the MAGST because of a prevailing effect of increased surface albedo and latent heat consumption. On north faces, thermo-insulation can dominate and snow can warm MAGST similarly to gentle mountain slopes. "consistent with insolation parameters" refers to sect. 5.2 where we explain: "The SSO pattern at BH_E suggests that duration of insolation was the main controlling factor and that snow cover had a limited effect." But in the revised version, this confusing expression doesn't appear anymore.

Reviewer #2, Detailed comment, table 1: An indication of the snow conditions at the sensor locations would help in this table. Authors' reply: Thank you for this suggestion. We debated this idea and finally decided to do not show any indication of the snow conditions in Table 1 because it is globally estimated from camera and snow records for BH_S and BH_E; and for BH_N and S3 we estimate the depth >1m because each time we have to dig to reach the logger (all the year long excepted at the end of the summer) we notice that the snow depth is > 1m. Since it is suggested, we added these precisions on snow depth in Table 1 and how we inferred the thickness values (especially for BH_N and S3 as it was already explained for BH_E and BH_S) in the table caption and in section 3.2. "Snow accumulation over BH_N and S3 is estimated from field observations. At BH_N, snow accumulation is restricted to the relatively large ledge above which the borehole is drilled and the snow patch is over 1 m thick for most of the year. S3 is also frequently covered by > 0.5 m of snow accumulating during winter and spring on the small ledge above which the sensor is installed. But snow depth is more variable on S3 than on BH_N because of the intense solar radiation that leads to more frequent melting."

Reviewer #2, Detailed comment: Figure 4 To replace numbers (e.g. no. 6) in figure with location labels (e.g. S3) would be nice (if easy to implement). Authors' reply: True, so we reworked the figure and now it appears definitely better. Note that we used a vertical labelling because it was easier to associate the points and label compared to horizontal labelling. We also slightly reworked the legend and the text has been adapted with this new labelling.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C2002/2014/tcd-8-C2002-2014-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 8, 2831, 2014.

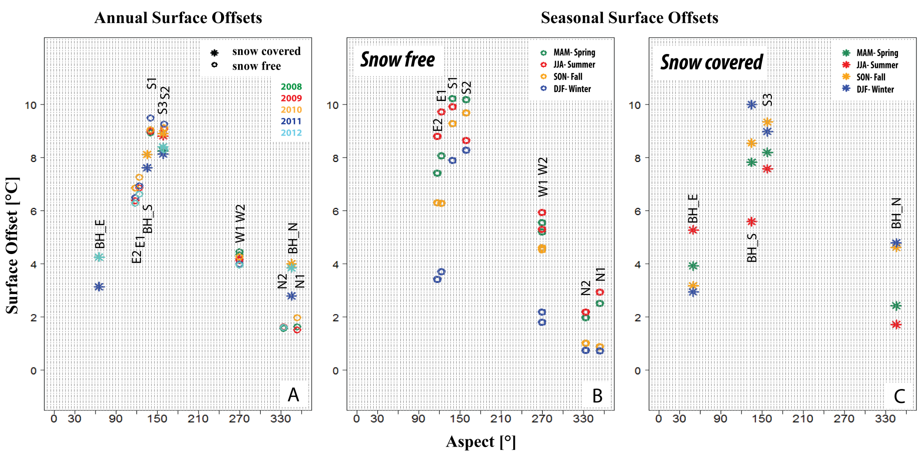

Fig. 1.