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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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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.

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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.

General comments:

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.

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

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6.

In the following general comments on the different sections of the manuscript are made:

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.

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!

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).

Description of installation (Monitoring system): For the interpretation of the non-conductive 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).

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.

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)?

Regarding the filling of gaps longer than 1.5 months I found the manuscript inconsistent: On page 2839 line 5 it is stated that these gaps are filled for Dec. 2007 to Feb. 2008. Line 15 of the same page states that no gaps longer than 1.5 months were filled. In the caption of table 2 gaps < 1.5 months (shouldn't it be "gaps > 1.5 months") are indicated for Dec. 2007 to Feb. 2008. What about the other gaps indicated in table 2? Where are these "longest gaps" finally filled and used for the MAGST and SO calculation?

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.

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).

Detailed comments:

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.

p. 2832 l. 19: “below -4°C”: There are other sites with temperatures in this range (e.g.

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PERMOS 2013; but no boreholes). Precise or leave away.

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).

p. 2833 l. 18–22: reformulate. microtop. and structure does not only effect surface layer.

p. 2833 l. 24: “models” instead of “modelling strategies”

p. 2834 l. 7ff.: “Four years ...” Isn’t this rather a conclusion?

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?

p. 2838 l. 10ff: What air temperature lapse rate was used? Wouldn’t it be relevant with more than 100 m vertical extent?

p. 2838 l. 27: “0.3 m rock temperature” instead of “0.3 m-deep temperature”

p. 2839 l. 18: “Rock surface temperatures” would be simpler. Unclear what “temperature characteristics” are.

p. 2840 l. 02: Where are these 12°C from? Source? PERMOS, 2013 p.11 states 10°C.

p. 2840 l. 07ff.: “This is because ...” is a discussion of Allen et al. and not result here.

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

p. 2840 l. 26ff: e.g. "S3 (no. 6)": to avoid this double labelling would be nice (see comment fig.4)

p. 2840 l. 28: What about latent heat of snow fusion?

p. 2841 l. 4: "no. 10 and 12"? no. 11 is snow covered, right?

p. 2841 l. 4: are these 100 m vertical difference corrected by a lapse rate?

p. 2841 l. 7ff, p. 2845 l. 1 and p. 2850 l. 21: 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?

p. 2841 l. 9–10: ". . . depend mainly on . . .": where can we see that? Fig. 5?

p. 2841 l. 18: S2 is snow free? But difference (var.?) is not smaller.

p. 2841 l. 21: if insulation is the dominant process

p. 2842 l. 6: phrase: ". . . surface temperatures of snow covered sensors were. . ."

p. 2842 l. 10: What means "consistent with their aspect"? Where can we see that?

p. 2842 l. 20ff: repetition regarding the effect of snow cover

p. 2843 l. 3–5: This is really well established knowledge and may belong to an introductive paragraph.

p. 2843 l. 5ff. 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)

p. 2843 l. 11: "a different effect". Which effect?

p. 2843 l. 14: Why "smoother than expected"?

p. 2844ff. note: Many similar comments/questions as on last 3 pages appear. I stop this level of detail here.

p. 2844 l. 9: According to earlier statements in the manuscript (e.g. p.2841 l. 25ff) such a threshold would depend on aspect.

p. 2844 l. 18: Fig. 6 is not temp profile.

p. 2845 l. 3-4: "... comparison ... is difficult ...". Why? What is fundamentally different at AdM?

p. 2846 l. 21: How "amplitude" is defined? Peak-peak? meanJan – meanJuly? What is exactly shown in Fig. 7b?

p. 2847 l. 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?

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).

p. 2848 l. 18ff: 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).

p. 2850 l. 11: rephrase "spatial distribution of surface temperatures ...". Do you mean "pattern of MAGST"?

p. 2850 l. 13: This is not shown in this study.

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?

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"?

table 1: An indication of the snow conditions at the sensor locations would help in this table.

figure 4 To replace numbers (e.g. no. 6) in figure with location labels (e.g. S3) would be nice (if easy to implement).

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