Referee comment on revised “Thermal characteristics of permafrost in the steep alpine rock walls of the Aiguille du Midi (Mont Blanc Massif, 3842ma:s:l:)” by F. Magnin et al.

The manuscript experienced considerable changes since the first submission. These changes are: a) minor reorganization and reformulation throughout the manuscript; b) major changes in the discussion part (sect. 5 & 6) with clearer separation between observation and interpretation; c) the sub-section on heat fluxes has been completely erased; d) new content regarding latent effects in discussion, conclusion and abstract.

These measures resulted in an increased understandability of the manuscript. As a consequence it allows questioning some (partly new) points of the discussion. Regarding the latent heat effects the paper does not present convincing figures to support these conclusions. In other parts of the discussion (interannual variability) it is still hard to follow the reasoning. Generally, it is not evident for each conclusion how it is based on the data!

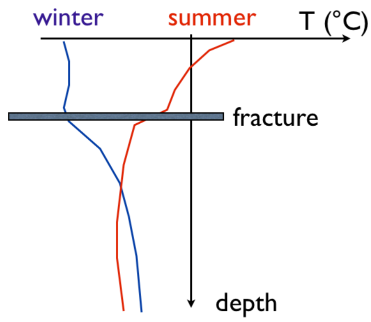
The almost complete reduction of the part on the non-conductive heat transport gives much less weight to one of the most interesting findings in the presented data set. The language of the revised manuscript has still shortcomings. On my opinion the manuscript needs the following revision for publication in TC (ordered by priority):

Major concerns:

Latent heat:
The dampening of active layer thickness by latent heat consumption is postulated in the revised version as one of the main new findings (abstract, discussion, conclusions 10). This effect is well known in arctic ice-rich soils but I could not see the empirical evidence (zero curtain, isothermal profil) for this effect being relevant in the present data from steep bedrock.

Language:
Regarding terms and language the manuscript still needs improvement. Often I had to read the sentences twice to understand. The sentences could be expressed simpler and straighter to the point. There are several mistakes in grammar and style. Terms and adjectives may be used more accurate. Some of them are mentioned in the **Detailed comments**. The manuscript clearly needs a second revision regarding the language.

Non-conductive heat transfer (former section 6.3):
I think to simply erase this content from the manuscript is a petty. I don’t agree that doing an analysis as suggested would go beyond the scope of the paper. Making a profile plot of several points in time of BH_N is a minor effort and would allow seeing (qualitatively; see below) where the fracture acts as a heat sink/source and where it simply causes a step in the temperature profile. (The other suggestion with a heat conduction scheme is a larger effort, I agree).



Example Figure: *if temperature gradient is similar on both sides of fracture no heat sink (summer); if inflection to left fracture acts as heat sink (winter). Location of thermistors relative to the fracture is important!*

Interannual variability

A large part of the discussion on "Snow cover and micro-meteorological influence" is focused on the interannual variability. Tree (of ten) conclusions are made on the interannual variability of the MAGST. Given the small evidence presented (without reference to an illustrative figure) to support of these conclusions this strong focus is not justified.

I still have problems with the use oft the term "interannual variability": The difference of 2 years is generalized as interannual variability. Accordingly I can not understand the following answer from the authors to the first review and I don't see how the concerns where considered in the revision:

Reviewer #2: „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.

To make clear what I meant: To compute an interannual variability it needs more than two values (SO or MAGST of more than two years). The spatial pattern of interannual SO variability that you postulate on Line 332–350 are based on one single observation (2011 vs. 2012) according to Line 269–273. I agree with your reasoning, that the effect of insulation and reflection on south slopes COULD result in a reduced interannual variability but the presented examples (Line 269ff) do not clearly show this: E.g. why -0.3°C at E1 is considered as larger interannual variability (or better "difference") whereas +0.3°C at S3 is taken as example for a small variability?

Why interannual variability of the MAGST appears in the conclusions whereas the results and discussion where mainly on the interannual variability of the SO? This two things are not quite the same because of the variations in MAAT. I don not see this being considered in the text.

Please rephrase Lines 269-277 to clearly express the relevant differences and give a comprehensive overview of the "interannual differences" in a figure to demonstrate an empirical evidence for the spatial pattern that you describe. Otherwise restrict this "hypothesis" to a minimum and avoid to state it as empirical finding of the study in three different conclusions.

Detailed comments:

Line 23:	"Analysis …" Why plural?
Line 24:	",some of them …" rephrase; "them"?, what of the following is demonstrated the first time?
Line 29ff:	reference of second part of the sentence is unclear.
Line 30:	inhibit? not delay or reduce
Line 30ff:	Consider general comment on "Latent heat"!
Line 76:	These scientific goals overlap with the research questions below. Avoid repetition (The precise research questions addressed here are most relevant). Just mention point tree as additional point and why this article may be relevant for it (describtion of installation and data quality).
Line 89–93:	Style/grammar: Rephrase questions
Line 89 / 92:	These two questions could be merged into one.
Line 121:	Shift to acknowledgements.
Line 205:	This local snow accumulation will influence the profiles because the deeper part is influenced by the surrounding snow-free rock. How much?
Line 230:	This is still confusing compared with the statement on Line 218. The gap is longer but only a part is within the respective year, right?
Line 231:	Why you "felt" so if the effect on the annual mean was so small (see line 228)?
Line 242:	"ASO" is a pleonasm if SO is defined such as on Line 235
Line 266:	At S2 the autumn SSO is larger!
Line 294:	grammar: "micro-meteorological influences"

Line 321ff:	(i) is not a cooling effect compared with snow-free rock (but a reduced warming effect compared with thick snow cover). next sentences need to be adapted accordingly.
Line 329:	Where is this zero-cutain effect visible? Neither in autumn 2010 nor 2011.
Line 337:	grammar: "from one aspect"
Line 341:	style: "poorly"?
Line 363:	style: "max. ALT occurred in…"; for BH_S it is problematic to make such a statement due to the missing data
Line 389:	I can not see how Table 3 supports this statement! MAAT and MART show different evolutions.
Line 395–398:	Rephrase sentence!
Line 405:	temperature gradient can not be correct "–0.2°C/m" ?
Line 423ff:	"In bedrock…": Sentence contradicts content below! "any specific thickening…": no thickening? Be precise which study states what!
Line 433:	Daily SO! This is new data that is not shown anywhere! This makes this paragraph not retractable. What is the main message and where can we see that?
Line 442:	where can we see the "isothermal conditions"? Compare comment Line 329.
Line 449:	grammar: "temperatures"
Line 450:	grammar: "smoother"
Line 454:	Style: What means "usually greater"? Better: "According to a modelling study…"
Line 456ff:	I can not see an empirical evidence for the reasoning presented. Latter refreezing with larger ALT is a simple result of heat conduction. The freezing in figure 6 BH_S looks quite linear. Detailed data is not presented.
Line 472:	Logic: "MAAT changes up to 10m depth"?
Line 473–476:	Interesting! Where this is shown?
Line 483:	Inflection in the range of measurement error?
Line 496 and 503:	Style: "deep temperatures"? Borehole temperatures?

Line 568:	Correct typographical inconsistences.
Line 600:	Gruber AND Haeberli.
Line 757:	“depth” instead of “deep”,
Line 761:	“depth” instead of “deep”, AT is missing in caption