Report #2

Submitted on 17 Aug 2021
Referee #1: Craig Smith, craig.smith2@canada.ca

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I would like to thank the authors for making the suggested revisions and addressing this reviewer's questions regarding this manuscript. It appears that this is now much improved over the previous version. There are still a few minor revisions required before this can be published. These are mostly wording and grammar issues. I would recommend, however, that a header for a sub-section on Discussion be added where noted in Section 4. This would offset the presentation of results from the summary and discussion and improve the flow. There was a Discussion section in the previous submission but the heading seems to have been removed. This should only be a minor problem to remedy. The specific issues are noted in the attached annotated manuscript. Once these revisions are complete, I can recommend that this manuscript be accepted for publication.

> We wish to thank once again the reviewer for his work to improve our manuscript. All the grammar issues pointed out in the commented PDF have been addressed. Regarding the comments on headers and sections that seem to have been deleted, somehow these issues were generated when converting the Word file to PDF on the marked version of the manuscript. We invite the reviewer to refer to the "clean" version of the manuscript, which is immune from these issues.

Referee Report: amt-2021-63-referee-report.pdf

Report #1

Submitted on 03 Aug 2021
Referee #2: Hendrik Huwald, hendrik.huwald@epfl.ch

Suggestions for revision or reasons for rejection **(will be published if the paper is accepted for final publication)**

The authors provided satisfactory responses and modifications to most of the comments and questions and significantly revised and improved the manuscript. Some final remarks and suggestions are given below with the intention to further improve and strengthen the paper. Enumeration refers to my original comments (RC3) and the corresponding author replies.

> We wish to thank once again the reviewer for his work to improve our manuscript. During the previous stage, it seems that there have been some issues while exporting the original Word version of the marked manuscript to PDF. In particular, some sentences have been cut to the point of being rendered unintelligible. We therefore invite the reviewer to refer to the "clean" version of the manuscript, which is immune from these issues.
> We are sorry for the nuisance.

1. While the language is significantly improved, I suggest a final language check, especially of the revised parts and the figure captions to eliminate remaining language errors.

> A final round of language check has been performed.

6. The albedo of very thin snow covers of low density may be influenced by the dark ground surface. In this case, density could be a significant quantity.

> We agree with the reviewer: this has been added to the revised manuscript (lines 131-133)

7. The provided explanation is fine but underlines the sub-optimal location of the field site (see also point 13). The ratio of reflected over global radiation is exactly the definition of the broadband albedo, which typically increases over snow for large zenith angles. While discussed in the theoretical framework of Musacchio et al., 2019, these considerations also find application in this study and should not be ignored.

> We agree as well. This consideration has been underlined in the revised text (lines 127-130)

9. Of course, such studies are not a competition, but objective results are useful for users and manufacturers (analog to model inter-comparison projects, MIPs). The latter can always improve their products, but science should have the information for selecting best suited instruments, especially for critical climate studies. Up to the authors, but I'd suggest disseminating this information.

> Thank you for your understanding. Manufacturers lent us the equipment after a verbal non-disclosure agreement and we intend to keep our word. However, an expert eye can recognize at least some of the models from the pictures.

10. I still think that this picture is not needed. It does not convey the size of the shields unless you provide a scale for these in the foreground and those in the background. The laboratory phase is

described in the text and well discussed (also documented by Figure 3). Fine if you insist, but personally, I don't see any use. Concerning Figure 3, I would rather show the absolute differences since the order of subtracting is arbitrary (this would also allow for a better resolved y-scale of Fig.3).

> The picture was important to us, especially in the first draft of the manuscript, because it was the only detail of the instruments. Given the addition of figure 5b (now 4b), we tend to agree with the reviewer and the picture has been removed.
> Figure 3 (now figure 2) has been redrawn following the suggestion of the reviewer.

13. I understand the constraints. However, I would be careful with the statement that influences by obstacles cancel out because of symmetry considerations, and I would explicitly mention in the paper that the site is a compromise and not the perfect location for the study and analysis carried out to minimize potential criticism.

> Agreed. Few considerations have been added on the obstacles (lines 216-219).
> The second statement was already present on line 215 of the previous clean version ("The chosen area turned out to be a reasonable compromise…"), and we do not think it is necessary to emphasize it: only a brief mention about the non-perfect siting, as suggested by the reviewer, has been added (line 218).

18. Ok, see also comments of point 13.

> Agreed.

19. Ok. A sentence that relates the letters in the legend to the different sensors defined and shown in Table 1 and Figure 5b could be included in the caption.

> A sentence has been added in the caption of figure 9 (now figure 6), redirecting the reader to Table 1 and figure 4b. We still do not think it is wise to describe each system at length in the text: sentences like "*A* refers to a fan-aspirated, "spheroidal" type shield, *B* is a passive, "classical" shield, etc", would impair readability and weigh down the text. We think Table 1 is still the best place for such long descriptions.

22. Even though the authors do not consider it the principal purpose, the difference of actively and passively ventilated is an important point, certainly worth being discussed in the context of this work, and I would encourage a few related sentences.

> Few sentences about the comparison between actively and passively ventilated shields were already added (lines 342-349 of the previous clean version). Following the reviewer's request, this comparison is expanded in the new revised version and few other sentences are added (line 353-ff).

23. Yes, it is duplicate information, but it eases reference instead of turning pages. Up to the author's preference.

> Thanks. We will leave the captions as they are: there are 4 plots in total that refer to the instrument pairs by their letter, and we direct to Table 1 and Figure 4b only the first time, as explained in reply to comment #19.

24. Looks good. Probably no longer necessary to use different symbols in the 6 panels.

Agreed. The plot has been redrawn with a common symbol.

25. I agree with the authors that only one actively ventilated sensor does not allow for a systematic comparison. However, some points of my original comment 25 are still interesting to be mentioned as they are not necessarily intuitive. Also agreed that a specific shield may host different sensors and the same sensor type is installed in different shields. Therefore, it is so important to further update Table 1, which is still lacking the sensor accuracy, which I consider essential, much more important than the sensor resolution provided. I strongly advice adding this information, especially if the authors decide not to reveal manufacturer and model of the sensors and shields in use.

There may be a cultural misunderstanding here in place. Following metrological guidelines, stated in the Guide for the Expression of Uncertainty in Measurement (GUM) and the Vocabulary of International Metrology (VIM), “accuracy” is not a measurable quantity, rather a quality of the measurement in terms of closeness to the “true value” of the measurand. An instrument is “accurate” if it fulfils a target uncertainty specific for a given application.
On the other hand, the “uncertainty” of the instrument (or, to be more precise, the uncertainty on the measurement results of an instrument) can and must be evaluated. It can be done in an *absolute* way, or in a *relative* way: we decided to focus on the relative uncertainties between the instruments in each pair in order to avoid, for instance, those relative to calibration (as already stated in 140 and following of the current revised text). As a matter of fact, some of the pairs of sensors were not even calibrated (data available on Zenodo.org, cited in the manuscript, are provided for some sensors as raw electrical resistance measurements rather than temperature).
In this investigation, more than the absolute values, the key parameter is the instrument's stability or, even better, the stability of the relative differences between the instruments of a pair.
For the purpose of evaluating *relative* differences, knowing the *absolute* uncertainty of a measurement is not useful at all. Instead, this adds unneeded components of uncertainty, which would make difficult to discriminate the studied effect. In fact, in the whole paper there is not a single reference to a *temperature*: only to *temperature differences*.
For this reason, it is not possible to put uncertainty values for each instrument in Table 1: in their stead, we provided relative uncertainties which are expressed in Tables 3 and 4 and the evaluation of their stability as a key uncertainty component, which in turn can be considered as part of the overall sensor’s quality.

27. Ok. FYI, good automatic cameras are available for a few hundred €.

Thank you. Energy costs were also a concern, given that it was given us for free by the municipality and the local weather agency, so we did not want to abuse their hospitality or set up complicate arrangements. Should the experiment be repeated (as we are hoping to) a camera will surely make a useful addition.

28. I am glad to see the new Figure 8 showing the albedo computed from the radiation measurements. However, I disagree that such analysis is out of the scope of this paper, in fact, it is one of its core points – once again – as indicated by the paper title. I strongly suggest a discussion and at least a brief analysis of the albedo data. Here I reiterate my impression that the data is ‘under-exploited’ and carries potential for further insights.

We agree that the data on albedo is under-exploited. However, for our purposes it showed that radiation values, both global and reflected, are much more important than their ratio (the albedo) on the radiative heat transferred to the sensors: for instance, having $\alpha = 1$ at 600 W m^{-2} is not the same than $\alpha = 1$ at 200 W m^{-2}.
This has been added to the revised manuscript (lines 309 and following), as well as a new Figure 8 showing the relation between albedo and ΔT of the instruments.

P26 Fig.8: Why are there so many values >1? I would have assumed that this happens only for low incident solar radiation values, but greyscale dots suggests that this is not only the case. I would consider filtering, removing values for R_inc < 10 Wm-2 (for instance), and all unphysical values and provide an explanation for the cause of these erroneous values and how many have been removed by filtering. If this does not eliminate albedo > 1 there may be a problem with the radiation measurements. Also, I would remove the grey figure background to increase the contrast of the grey dots (or use a color scheme as in Fig.12).

As a matter of fact, the explanation for $\alpha > 1$ was already provided in the text (now at lines 307-311 of the clean revised text). Values at low incident radiation, as the reviewer pointed out, are due to the intrinsic uncertainty of the radiation meter, which was evaluated as ~35 W m^{-2} (line 324). Values at high incident radiation have been interpreted as the instrument being covered by snow. Problems with the measurements are likely to be excluded given that all outliers present in one site are also present in the other: the probability of two equally malfunctioning instruments is very low.
The plot (now Figure 7) has been redrawn removing the grey background, as the reviewers suggests, and with an incident radiation threshold set at 35 W m^{-2}.
A brief sentence has been added in the revised text (line 308) about the frequency of outliers.

Abstract:
P01L021: representative of what? how is "representative" defined?

Representative in terms of the variety of instruments and shields which are available on the market. Added to the text.
The same concept is expressed in the text in line 405.

P01L022: I would replace "appropriate" with "acceptable" (cf. point 13 above).

Agreed. Changed in the text.

P01L023: I think it should be "global" not "direct" radiation since it is not mentioned how the diffuse component has been subtracted (global = direct + diffuse). Please check that you use the applicable term for radiation throughout the manuscript (shortwave, solar, incident, global, reflected, direct, diffuse – I would avoid the term "backward").

Agreed. Changed in the abstract and elsewhere.

P01L024: "has been immune to"

Changed.

P01L025: "and days of high wind and or low incident radiation," Changed.

P04L115: It is stated that wind speed, relative humidity and solar radiation have been identified as “major quantities of influence”, however, humidity is not at all discussed in the paper. Why has it been ignored after considered a major control?

The reviewer is correct in pointing this issue out. As explained in Musacchio et al (2019), humidity is not expected to have a significant role in the albedo effect. The scattering effects caused by humidity are proportional to the air column, which is the same for the two sensors (causing the same effect to both), except for the 2 m between the ground and the sensors. In these 2 m, higher albedo should locally generate lower humidity conditions, but this effect is too small to have a measurable role.
As a matter of fact, humidity was included in the preliminary analysis, but later excluded since no discernible relation between it, the albedo and the temperature differences has been found. Such information has now been added to the revised text (lines 115 and following)

In general, indicating specifically where (page/lines) new information has been added in the revised manuscript would be very helpful and appreciated instead of statements like ‘this is now mentioned in the revised paper’ or similar.

Agreed. This reply features this information, based on the clean version of the manuscript.