

Atmos. Meas. Tech. Discuss., author comment AC1
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Reply on RC1

Chiara Musacchio et al.

Author comment on "Effect of snow-covered ground albedo on the accuracy of air temperature measurements" by Chiara Musacchio et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-63-AC1>, 2021

All the authors comment are given in bold character.

Referee comment on "Effect of snow-covered ground albedo on the accuracy of air temperature measurements" by Chiara Musacchio et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-63-RC1>, 2021

The paper entitled "Effect of snow-covered ground albedo on the accuracy of air temperature measurements" by Musacchio et al. describes a combined laboratory and field experiment to establish the impact of reflected radiation due to the presence of snow cover on the apparent temperature observation. Although temperature sensors employ radiation shields to prevent radiative errors to the measurement, most designs considered only minimizing errors from incoming radiation and not the errors associated with reflected radiation from snow cover (especially during conditions where the snow cover is fresh during low wind and high solar radiation conditions). The authors, after examining instrument uncertainty in a controlled laboratory, set up a field experiment using pairs of sensors located over a snow-covered area and over an area where the snow in the area of influence has been cleared. The results showed that the temperature error due to reflected radiative heating varied by configuration, but could be as large as 3° C.

I thought that the experiment plan was well thought out and appeared to be quite robust. The analysis and reported results were interesting and useful. I felt that the leading sections were generally well written with some wording and grammar issues that need to be cleaned up. The methodology was largely easy to follow and the figures, with some minor suggested edits, are appropriate. However, I felt that the Discussion (Section 5) and Recommendations (Section 6) are quite disorganized, poorly worded, and require a substantial amount of work before this can be published. My major concerns are as follows:

1) Generally, Sections 5 and 6 need to be re-written so that they convey the main points more succinctly. These require a thorough proof-read for English wording and grammar. I tried to make some recommendations, but since this needs to be re-worked anyways, I'm not sure they are that helpful.

The sections will be rewritten in the revised manuscript according to the indications of the reviewer.

2) In Section 5, many of the main considerations (lines 345-358) were confusing. The bulleted list is a good idea but this needs to be more clear. See the annotated file for more specific issues.

Thanks to the reviewer for pointing that out. Following his suggestions, the authors are going to revise these considerations in order to make them clearer and easier to understand

3) The discussion of the night time intercomparison in Section 5 (lines 359-361) doesn't really add much to the discussion and should probably be removed. You mention this also in the Section 4.4, stating twice (I think) in the paper that it is out of scope.

We agree with the reviewer and those sentences will be removed from the revised manuscript.

4) I thought that your discussion of your study limitations in Section 5 (lines 362-385) were too negative, somewhat detracting from the valid results. I think that you can point out your limitations and compromises with more brevity with a short discussion about how these may or may not have impacted your results, and thus improve the flow and readability of the section.

We thank the reviewer for this comment. We feel that limitations should be mentioned, but we agree with the reviewer that this section is somewhat too lengthy and will be reduced in the reviewed manuscript.

5) In Section 5, on line 388, you state "A complete correction curve in function of reflected radiation, wind speed for a specific instrument can be generated by users" but earlier you note that the relationship between temperature differences and reflected radiation are not always clear: line 329 "...the spread in values does not allow for a definition of a relation by fitting, nor it was possible to calculate a function between $\hat{\Delta T}$ and $\hat{\Delta Rad}$ ref." Maybe I misunderstood the messages, but it appears contradictory.

Line 329 refers specifically to measurement results presented in the paper (fig 14) that didn't allow to find a fitting function for the relation between Δt and ΔRad .

At line 388, the purpose is to suggest to manufacturers/ end user to characterize their own instruments to evaluate the albedo effect as a function of reflected radiation, wind speed etc. to obtain a correction function.

Since there is no certainty that a 'complete' correction function can be calculated, also in the case of a single instrument, the level of approximation that can be achieved must be taken into account.

In this view authors agree that the sentence must be rewritten to be clearer and not be in contradiction with the results of the analysis presented.

6) In Section 5, line 390, you compare your methods to Huwald et al., (2009), referencing the use of a sonic anemometer on several occasions. Unless I missed something, you don't really talk much about the Huwald et al. methods so the reference to the anemometer use is confusing.

As a matter of fact, we do not reference the sonic anemometer several times but only once, just to emphasize the difference between our relative measurement approach and the one by Huwald which used a sonic anemometer as fixed temperature reference.

We agree with the referee that the work by Huwald can be described a little bit more in detail in the text and that will be done in the revised manuscript.

7) In Section 6 (Recommendations), it is good that you point out that recommendations have been submitted to WMO committees, but I would recommend that you summarize the recommendations based on what is found in this paper, perhaps in bullet form. If the purpose of the study is to make these recommendations, then they should be clear and concise, and not just quoted from another report. Also, pay particular attention to the redundancies in this section.

Authors thanks the reviewer for the suggestion, in the revision of the manuscript, the "recommendations" in section 6 will be summarized and presented in a clearer and concise way.

8) I would like to see some discussion in Section 6 as to why you think the different configurations are being impacted differently by reflected radiation, wind speed, etc. Can you make some general recommendations on shield/instrument design given your results, and if so, include these in the recommendations section? Further related to this, it would have been interesting to see a non-shielded pair of sensors in the experiment. Why was this not considered as a control?

The main purpose of the paper is to quantify the albedo effect involving different configurations to obtain a result as general as possible. However, the analysis is still limited to some possible configurations and the aim of the authors is not to force the choice of a type of configuration. However, some recommendation should be added in the discussion given the result of the analysis.

In environmental air temperature measurement, as a general rule, the sensor should be in thermal equilibrium with air without any other source of heating.

A completely non-shielded sensor is subject to direct radiation which causes a certain overheating depending on material, shape, dimension ecc. so in our view there is little value in using a non-shielded sensor as a control.

9) Section 7 (Conclusions) can be written more concisely, quickly summarizing the experiment, major findings, and main recommendations. The content is generally ok but it is too wordy with some confusing statements that muddle the message.

More specific comments and suggested revisions are embedded in the attached annotated document.

We again agree with the reviewer and the conclusion section will be reduced in the revised manuscript, focusing more on the findings and recommendations

Please also note the supplement to this comment:

<https://amt.copernicus.org/preprints/amt-2021-63/amt-2021-63-RC1-supplement.pdf>

All the comments will be addressed properly in the revised document.