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Comment on amt-2021-63

Hendrik Huwald (Referee)

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-RC3, 2021

Review of AMT-2021-63:

“Effect of snow-covered ground albedo on the accuracy of air temperature measurements”

by C. Musacchio, G. Coppa, G. BegesÌ□, C. HofstaÌ□tter-Mohler, L. Massano, G. Nigrelli, F. Sanna, A. Merlone

This paper investigates the influence of a snow cover on near-surface air temperature measurements which are generally subject to solar radiation-induced errors (even when sensors are protected in a shield), Such errors are amplified by a contribution of surface-reflected shortwave radiation, additionally heating sensor shields and sensors. To quantify this so-called albedo effect, several pairs of identical sensor/shield combinations are deployed at two neighboring locations, experiencing the same meteorological conditions. At one site the seasonal snow cover was unchanged, at the other snow has been removed after every snowfall such that these sensors were mounted above bare ground. Prior to the field experiment, the sensor pairs have been tested and characterized in controlled laboratory environments to determine relative errors and biases. Air temperature, shortwave radiation and wind speed data have been collected and analyzed to quantify the albedo effect on air temperature measurements by comparing temperature observation at the two sites. Observed temperature differences were related to the magnitude of incident solar radiation and wind speed to assess their relative importance. Albedo-induce errors were found to be as large as 3.8°C. The study follows certain WMO guidelines and proposes recommendations for measurement protocols to be applied on reference stations, e.g., in the context of GCOS.

The paper constitutes an important and interesting contribution to the improvement of measurement practices and the quantification and reduction of measurement errors, here specifically albedo-induced uncertainty and error in air temperature measurements. The study proposes an interesting experimental setup and obtains useful results from the acquired data. The paper is a perfect topical match for AMT.

Nevertheless, I have several concerns, suggestions, and questions which I would like to bring to the attention of the authors, detailed in the list of specific and sometimes more general comments below. It seems to me that the available data and results are partly underexploited and carry potential for more conclusive results. The manuscript needs substantial revision as indicated by the comments below. Integrating these suggested elements, clarifications, and discussions could strengthen this work and make it a more mature and consolidated paper. I am aware that I am raising a lot of points, but I hope that these comments are constructive suggestions and helpful for further improving this paper.

- The manuscript needs linguistic editing to improve spelling, grammar, and wording.
- The abstract should be more specific with respect to results and findings of the study.
- Field data have been acquired at 2m above ground. When the paper talks about "near-surface" air temperature measurements, what is be the upper limit for this term, and up to which height do you expect the albedo effect influencing air temperature measurements? E.g., sensor on a 10m mast?
- Related to comment #2, given a 5m radius of the snow-free surface and a 2m height of the sensors above ground, how does this relate to the field of view of the radiometer used which normally sees a hemisphere. In other words, what is the footprint the sensors (pyranometer and thermometer) see? Is a 10m diameter sufficiently large to neglect contributions from outside the circle?
- Section 2: I don't see much of a "theoretical study" in this section; is this statement related to previous work of the authors? Maybe consider revising the section title.
- Section 2.2 could briefly mention the influence of the identified quantities. In particular, the role of snow depth and the snow properties should be mentioned. Why is snow density not considered?
- Has the solar zenith angle been considered to have an impact? This is not only influencing the shortwave radiative heat flux but also the snow albedo (even significantly at large zenith angles).
- P05L143: Why is the sensor at the central location considered the "reference" for the other two air temperature measurements if subsequently not considered in the analysis?
- Tables 1 and 2: Provide manufacturer, device model and sensor accuracy, resolution and response time information for temperature sensor and shield in the Tables. Additionally, describe the differences between sensor/shield combinations C and D, as well as E and F; this information is absent from the text and table. Which data logging devices have been used in this study?
- Figure 2 is not necessary and can be removed. The completed Tables 1 and 2 will be more useful.
- Figure 3: what does the sign of the results indicate? Could be shown as absolute differences, to improve Figure clarity. Are all sensors sampled at the same frequency? Not clear for C and D; solid lines and color would help, also for attribution the sensors. Refer to the corresponding table for the letters in the legend or include this information in the caption or directly in the legend. Why are E and F not shown?
- Figure 4: I suggest replacing the relief with a detailed local topographic map; add an inset showing a larger area for context. The blue dot is too small if this figure gets rescaled. Add a North flash and a scale. I would combine Figure 4 and 5 in one figure of two panels; maybe even removing Fig.4 and only giving the site coordinates.
- Looking at Figure 5, I am surprised by the site selection; this is very heterogeneous terrain with buildings, trees, roads, streams, and complex topography nearby, all being elements that should be absent or far away according to the cited list of criteria. How do you justify the choice of this site given these constraints? The discussion section gives some explanation and justification for this choice. Nevertheless, my understanding of flat, open, homogeneous terrain is very different, and I would have

selected a location with homogeneous fetch of at least a few 100m in all directions. I even fear that in wintertime sensors could experience shading from the tall trees South of the sites when the sun is low. Did the recent studies of Coppa et al. (2021) and Garcia Izquierdo et al. also consider potential effects of albedo in the hemispheric field of view of the sensors, influence of obstacles on local turbulence and sensible heat fluxes or terrain effects by topography and vegetation on longwave radiation?

- Figure 6: Where are the radiometers installed? I don't see them in the picture, and I would have expected them to point South, i.e., towards the observer to avoid shading from the other instruments.
- I suggest combining Figures 7 and 8 in a single two-panel figure.
- Section 3.2 lacks information on the experiment duration, time periods measured, sampling frequency.
- Section 2.2. raises expectations on how snow thickness and snow conditions have been measured with reference to Section 3.2, which then only states that any fresh snow has been removed, and the snow at site (a) has not been characterized. If no snow has been removed at site (a), the snow surface characteristics change over time influencing the snow albedo. How has the snow depth been measured, manually or with an automatic acoustic range finder? Not clear in the manuscript.
- P08L232 (cf. also #12): Apart from topographic shading, shadows should actually be excluded based on the site selection criteria. And if an instrument is shaded by another sensor this should be identical for both stations. Otherwise, there are undesired perturbations from nearby objects or topography which should be removed. Also, vegetation, water level in rivers should not be a factor for a homogeneous site (see P08L240-241). It is not clear to me, how you account for these influences, e.g., the water level in the streams on both sides of the measurement field, and I have a hard time understanding the values presented in Table 4. I think this needs further explanation.
- Figure 9: Include the sensor type and shield in the figure legend. Y-axis label: put the units in [brackets] or (parenthesis), otherwise it could be misread as "delta_t per degree C"; this applies to most figures in the manuscript.
- P10L292: I understand the motivation for choosing a threshold value for the difference in reflected SW radiation between the two sites for identifying the largest albedo effect. However, it would be interesting to have a quantification of this effect over the continuum of differences in reflected radiation. Could this still be considered? Otherwise, include a reference to Figure 10 where this choice is graphically justified.
- Figure 10: Which data is shown in this figure? What is the timestep on the x-axis? Are you showing instantaneous values of the 10min resolution data? If not, what exactly is shown here? If these are selected values excluding certain measurements not meeting the selection criteria, there should be data gaps (values cannot be equally spaced in time as the figure suggests). This information should be included in the caption. The graph looks like a bar plot; but why are there some diagonal lines in the graph? Panels (a) and (b) could be lumped using color. Why are there negative values in panel (c)? The reflected radiation over snow should always be larger than that over bare ground?! How do you explain the negative differences sometimes exceeding 100 W/m2?
- Section 4.2 does not explicitly discuss the difference of passive vs. actively ventilated radiation shields which is a crucial point that definitely should be included in the discussion. This section also gives the misleading impression that the differences depend on the sensor type while the main cause is rather the sensor shield absorbing (incident and reflected) shortwave radiation. This point should be clarified.
- Figure 12: Add the sensor type and radiation shield information to the panel letter to facilitate reference.
- Figure 13: Use color for better readability. I would also try to identify clusters to characterize different behavior of the available sensor/shield combinations.
- Figure 14 and Section 4.3: I am surprised that any possible difference between ventilated and passive shields is not at all discussed (apart from one sentence in Section 5). Figure 14 suggests that an aspirated sensor is not necessarily performing

better than a non-ventilated, cf. A vs. D, respectively. From Figures 13 and 14 it can be concluded that the magnitude of the albedo effect is a combination of the influence of reflected shortwave radiation and wind speed; radiation alone does not explain the temperature differences (see Figure 14) and wind speed seems to dominate radiation effects. Sections 4.2 and 4.3 would largely benefit from such kind of a discussion. Additionally, a discussion on how the albedo-induced error in air temperature measurements compares to the magnitude of errors due to incident solar radiation heating of the shield and sensor may be another interesting point. Perhaps even identifying a ratio/relation between the two. And finally, what is the advantage of a helical shield in comparison to a standard multi-disk shield?

- Section 4.4: Certainly an interesting point, but out of the scope of this study (as you already mention) since it is not related to albedo and solar radiation but rather to longwave radiation. You risk opening a can of worms here and I would remove this section completely (as well as the related lines 359-361).
- Have the instruments been monitored with an automatic camera to see occurrences of precipitation deposition on the sensors (especially the radiometers) or effects of riming? This way, spurious data can be flagged and removed.
- A major point that really surprised me is that the albedo has not been plotted, analyzed, and exploited. Given that incoming and reflected shortwave measurements are available at both sites, I would have expected a plot and a detailed analysis of the albedo for the entire period. This is actually what the paper title suggests. Also, it is a pity that surface temperature has not been measured at the two sites which I consider would have been very useful for the analysis. IR surface temperature is an interesting indicator linked to longwave emission and possible sensible heat flux contributions. I strongly suggest including these elements, at least the albedo if IRT measurements are not available.
- The "recommendation" at the end of the manuscript suggests the generation of a correction for the albedo influence on near-surface air temperature measurements, but the paper does not propose such a correction for the study performed. Such a correction, maybe even shield-specific, would be of interest to the measurement community.
- This study quantifies the albedo effect on air temperature measurements which is the objective of the paper. This effect has been quantified to be as large as 3.8°C. For this value and as an example, the given conditions could be added, e.g., the corresponding incident and reflected SW radiation as well as the wind speed. It should be mentioned that these albedo-induced errors do not include radiative errors due to heating of the sensor shield from incident solar radiation; that error has to be added to determine a complete shortwave radiation-induced error on air temperature measurements. Since no reference air temperature independent from radiation errors is available in this study, the total error due to heating of the sensor by solar radiation is not quantified which should be mentioned explicitly (one again in the conclusions).
- Almost throughout the manuscript, why do you call your results, analysis, tests etc. "preliminary"? e.g., L 134, 149, 154, 234, 273, 275, 304.

P01L019: Rather say "reflected radiation" instead of "albedo radiation".

P02L052: It is global radiation, not only direct. Alternatively, say "incident solar radiation". Also on L053.

P04L096: Remove "backward".

P04L103: Instead of "some meters of radius", better give the exact number.

P04L104: Replace “unique” by “single”.

P05L135: Provide a few details about the project and the initiator/project owner.

P05L148: I suggest “Laboratory tests”.

P05L155: Replace “systematic values” with “systematic bias”.

P06L159-160: What is meant with “sensor dynamics”, the sensor response time?

Tables 3 and 4: The table caption should include the term u_delta-t_instr

P06L161: The term u_delta-t_instr should be defined in the text, e.g., before the reference to Table 3.

P07L205: I would probably not call this site “heavily instrumented”.

P07L206: Give a reference for the installation protocol.

P07L215: To which model are you referring here?

P08L237: Write “incident solar radiation” (to distinguish from incoming longwave radiation) which is > 0.

P09L261 and P11L318: “expanded uncertainty” should be defined and explained.

P09L267: Write “incident and reflected solar radiation”

P09L269: Rather say “(highest albedo)”

P09L270: “Snow was removed on…”

P09L271 and 272: Soil or short grass? This is important for albedo classification.

P10L283 and throughout the paper: cross-references to Figures, Tables, Sections, Equations are capitalized.

Caption Fig.10: “…is indicated by the red horizontal line.”

Caption Fig.11: “…, for records exceeding the selected threshold for reflected shortwave radiation of 200 W/m2.”

P12L356: Insufficient information regarding “k=2”.

P15L442: “…exposed to the additional reflected solar radiation…”

Throughout the manuscript: perhaps use “pairs of instruments” instead of “couples of instruments”

Author contributions of BegesÌ□, Hofstätter-Mohler, Nigrelli and Sanna are missing.