

Interactive comment on “Bipolar long-term high temporal resolution broadband measurement system for incoming and outgoing solar UV radiation, and snow UV albedo, at Sodankylä (67°N) and Marambio (64°S)” by O. Meinander et al.

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Authors’ response to the review of the Anonymous Referee #2

The Authors are thankful for the valuable review comments given by the Referee #2. The comments of the Referee#2 are helpful to improve the quality of the revised version of our manuscript. Our respond to each remark of the Referee#2 is given here below. (The language check will be made only to the final revised version of the manuscript.)

It can be noted that our manuscript was originally planned to contain both the technical description, and thereafter a summary (which we called “review” in the original manuscript) showing what has been found in these measurement data so far. Based on the comments of all the three reviewers we suggest use the word “*summary*” instead of the word “*review*”. In addition, we suggest shorten the contents of this summary to 1-2 chapters, as suggested by Referee#2, and move it into the Introduction-section. The literature references on our earlier work are given for the benefit of any future data user. This fact we suggest to be more clearly stated in the revised version, too.

A) General comments

Referee #2 comment:

“The manuscript of Meinander and others has two main aims: (1) to provide a technical description of UV radiation and albedo measurement stations in the Finnish Arctic (Sodankylä) and maritime Antarctic (Marambio), including an identification of potential errors; and (2) provide a literature review of existing publications (mainly scientific reports, which are presumably not widely available) at one site (Sodankylä). The first part provides a useful scientific reference for potential users of the data and for establishment of similar high quality UV measurement systems elsewhere (although the section on measurement ‘challenges’ really needs to quantify error sources and provide methods to rectify them). The second part is rather ineffective consisting of a disparate collection of observations and somewhat speculative interpretation. The paper could be shortened considerably by using more concise language and better structure to avoid the frequent repetition. There are also a couple of places where the language is convoluted and unclear, together with other basic problems of expression such as undefined acronyms.”

Authors’ reply

We agree totally to the contents of the general comment given by the Referee#2. We are most pleased with the Referee#2 statement saying that “*The first part provides a useful scientific reference for potential users of the data and for establishment of similar high quality UV measurement systems elsewhere*”. We also agree that it would be most useful to quantify the error sources (Referee#2 comment “*the section on measurement ‘challenges’ really needs to quantify error sources and provide methods to rectify them*”). However, to properly include and quantify all the error sources can, in our opinion, easily be the subject of an entire paper itself. Therefore, we suggest to include here in the revised version of this manuscript some of the first quantifications of error sources, as specified in the detailed replies due to Referee#2 specific comments.

We also agree to the Referee#2 general comment on the second part, and suggest to replace review by a shortened summary, and move this summary in the Introduction-section. The literature references on our earlier work are given for the benefit of any future data user. This fact we suggest to be more clearly stated in the revised version, too. The language check will be made to the final revised version of the manuscript.

B) Specific comments

Referee #2 comment 1

1. The Marambio measurements appear to be made in a large container, although this isn't entirely clear. This setup needs a fuller explanation and an assessment of the effect of the container on the measurements through shadowing and impact on wind drift needs to be evaluated.

Authors' reply

We agree that a clarification on the measurement place is needed. A new photograph (below) is suggested to be inserted in the revised manuscript. The photo shows the horizontal pole where the downward sensor will be (is) attached (the pole will be/has been put as far from the container as possible; in this figure the pole is attached from the middle).

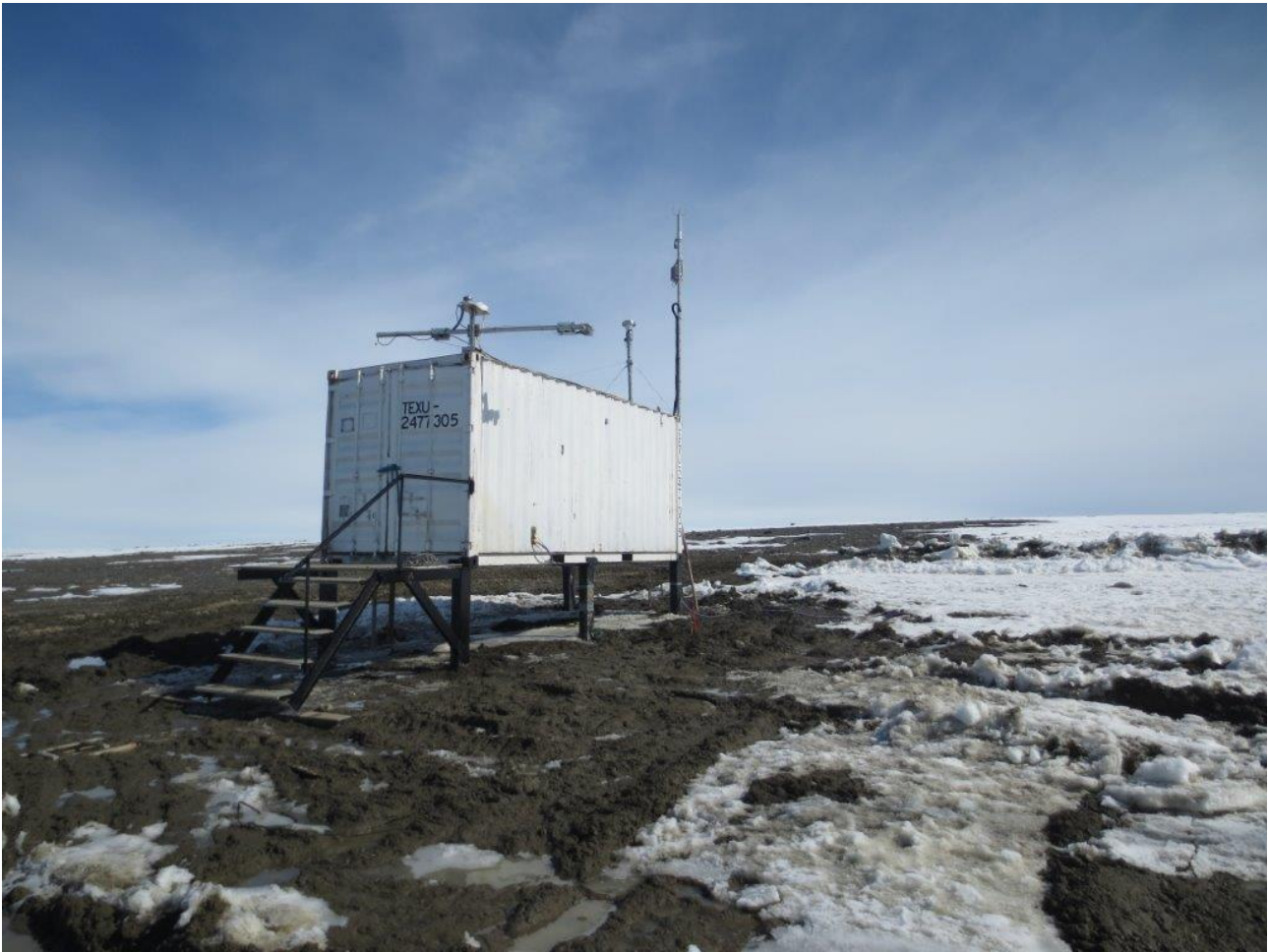
We agree with the referee that the effects of shadowing and wind drift are important to be included in the considerations of the revised manuscript. We agree that more effort needs to put in describing both of these effects, as will be explained here below. However, we also state that both of these effects could be a subject of an entire paper, and such papers have been published, too.

Thus, the shadowing effects due to the container could, in our opinion, be an interesting subject of its own independent paper. Earlier, we have presented calculations for a similar albedo edge effect case in Meinander and Räisänen (2010) <http://www.atmos-chem-phys-discuss.net/10/C11474/2010/acpd-10-C11474-2010-supplement.pdf>). Therein, in case of albedo measurements on snow patch with size 80 m North-South, and 50 m East-West), and at 3 m distance in one direction (North), assuming the true snow albedo were $\alpha(\text{snow}) = 0.7$ and the environmental albedo $\alpha(\text{env}) = 0.1$, the measured albedo would be 0.629, indicating an error of -0.071 in the measured albedo.

In Marambio, the container was placed in the middle of an open large field with >100 m by >100 m and sensor was placed toward the Sun (North), and the distance to one direction (South), i.e., the container, was appr. 2-3 m. The Marambio measurement height was appr. the same (2 - 2.5 m) as for Meinander and Räisänen (2010) case. Therefore, considering these distances, the error due to the container in the albedo measurements are not to be expected to be bigger than the error caused in the case of Meinander and Räisänen (2010).

However, as the snow surface is in practice not flat in Marambio, due to wind drift, as brought up by the Referee#2, some of the assumptions presented in Meinander and Räisänen (2010) calculations are violated in case of Marambio. The effects of wind drift on albedo in Marambio can, however, be identified using the photographs taken on a weekly basis for the albedo measurements.

As an outcome, we suggest to include in the revised manuscript a first estimation to the shadowing effect of the container, in case of flat snow surface, based on the case by Meinander and Räisänen (2010). We also suggest including in the revised manuscript a more detailed description of the photographs and their availability for the albedo data user. For the incoming solar irradiance data, the shadowing and wind drift effects can only affect via multiple reflection from the environment reaching the upward looking sensor. Changes in these effects from the irradiance point of view we state to be insignificant for the usage of the irradiance data.



Referee #2 comment 2

2. Section 2.5 identifies a number of potential error sources (mistermed as 'challenges'), but doesn't go any further than this. Without quantifying these errors and explaining how to remedy them this section isn't much use to potential users of the data.

Authors' reply

We agree with the Referee#2 that all the error sources are finally needed to be identified and quantified. However, as stated in our reply to the Referee#2 Comment 1, the effect of these errors could be subject of their own papers. Therefore, we have also given the name "challenges" to these error sources, that are only identified, not quantified. We suggest to state in the revised version of our manuscript that "This paper identifies the known error sources affecting these data, and gives the first estimates of the range of these errors, as error sources such as shadowing or wind drift effect could be a subject of their own papers." These error estimates will be based on our own calculations (e.g., Meinander and Räisänen 2010), or literature references, such as WMO (1996), Hulsen and Grobner (2007), given in Meinander et al. (2008):

"Here, use was made of erythemal UV albedo measurements by broadband SL501 radiometers with similar spectral responses, thus resulting in errors of less than 1% due to differences in the sensors (WMO, 1996). According to Hulsen and Grobner (2007), the typical total uncertainty for SL501

instruments is from 1.7 to 4.3 %. “

In addition, we suggest to add in the revised manuscript the following Equation of Briegleb et al. (1986) to explain the U-shape of the detected albedo:

$$R(\mu) = R_0 \frac{(1+d)}{1+2d\mu}$$

where μ is the cosine of the SZA, and R_0 is the reflectivity for $\mu = 0.5$ as given in their Table 2, and d is an empirical parameter.

Reference:

Briegleb BP, Minnis P, Ramanathan V, Harrison E. Comparison of Regional Clear Sky Albedos Inferred from satellite Observations and Model Computations. *Journal of Climate and Applied Meteorology*, 25, 214-, 1986.

This Equation we think can be of use for the albedo data user to understand the SZA dependency of the albedo data.

Referee #2 comment 3.

3. Section 3 (the second part of the paper) is a rather disparate collection of observations, simplistic analysis and somewhat speculate interpretation which doesn't seem to have clear guiding objectives. In order to understand this section the underpinning data need to be displayed in graphs and tables and the measurement methods, for example how sampling for impurities was conducted, need to be explained. Some similar observations, e.g. daytime hysteresis, have also been made in broadband albedo measurements of snow, but there is no cross referencing to this literature to identify common explanations. Section 3 might be better presented as either a short section (1 or 2 paragraphs) just reporting the main findings, or else removed entirely and written up as a full review paper which could provide the required critical evaluation and cross-referencing.

Authors' reply

We totally agree and will present the second part of the manuscript shortened to 1-2 paragraphs, and as a part of the Introduction.

c) Technical corrections

Authors' reply

The authors are grateful of all these detailed technical corrections notified by the Referee#2. Changes will be made accordingly.

L18 'large amount of' is vague and can be removed: agree

L24, and L57 and elsewhere in the paper 'for the first time' doesn't need to be stated as it should be obvious that a scientific paper is presenting something new: agree

L24-L28 *A very convoluted sentence which doesn't make grammatical sense. Rewrite as two or more sentences, and explain what the 'comparison' is between:* agree and will explain

L35 and L68-L70 *'literature review' implies a critical evaluation of a broad sweep of published material which is not what you do. 'Summary' would be a better term:* agree, summary will be used

L36 *explain these publications are mainly reports which are not widely available (if that is the case):* agree, will be explained but also this will be shortened

L53 *RT is not defined:* agree, will define

L57-60 *Same comment as for L24-L28:* agree

L80 *don't abbreviate 'figure' and be consistent in this throughout the paper:* agree

L93 *insert 'a' before 'snow surface':* agree

L103 *'September or October' is vague, give the exact date:* agree

L173 *This is not a good title for the section. 'Error sources and their treatment' would be a more informative title Section 2.5 This section could be written much more concisely:* agree

L181 *'regions, characterized by:* agree

L182-L183 *'low solar elevation' vague, give the exact elevation:* agree

L185 *'Due to frequent calm or low wind speeds' would be a better way to start this line L187 'really low' vague, give the exact elevations:* agree, will give

L191 *when does the sun appear exactly?* will give the exact when

L192 *again, give the solar elevation:* agree

L198 *'maintain clean and free'; 'visited only once':* agree

L199-L202; *wind appears 3 times as points ii), iv) and v). Isn't wind just a single hazard?* in the revised version we will give a more detailed description of the wind velocities in Marambio

L200 *what exactly is meant by 'uncomfortable' and is this really relevant to the scientific integrity of the data?* agree, does not concern the data user, will be removed.

L205 *Should this section be called 'Summary of results from previous studies using Sodankylä and Marambio data':* agree, will use the summary word, and will shorten the contents of the summary.

L206-L211 *This overview of the paper is repetition and misplaced here. Integrate with*

section 1: agree

L212-L215 repetition of section 2.3: agree, we thank the reviewer for the careful reading of the ms.

L217 define 'SZA': agree, will define

L234 define 'A': agree, will define

Figure 4 only seems to show the UVB sensors and not the UVA sensor. Is there a reason for this? We have only the UVB sensors.

Figure 5 please provide a legend. The caption should just explain what is shown in the figure, the description of methods used should be moved to the text of the paper: agree

Helsinki, 21 June 2016

The revised version of the manuscript will be provided after all the comments of the all three Referees have been replied to, and all the changes due to all these comments have been implemented.

Prior to our reply to Referee#2 here, we have given our reply to Referee#1. Next, we will reply to Referee#3.

Sincerely,
Outi Meinander, on the behalf of the co-authors