

## **Review of “Estimation of the radiation budget during MOSAiC based on ground-based and satellite remote sensing observations” by Barrientos-Velasco et al.**

We appreciate the feedback and comments obtained from both Referees. We replied to the comments for each Referee individually. We addressed all the comments in light blue font colour and included specific changes made in the paper in cursive. Additionally, the Referees can find the main differences between the submitted manuscript and the revised version in a diff.pdf file where the deleted sections are in red and the added ones are in blue.

Based on the comments from both reviewers the main changes to the paper were the following:

- Shorten the paper by removing repetitive or unnecessary text.
- Improvement of some figures in style and size.
- Improve the paper structure in the results section.
- Removal of non-essential plots in the appendix.
- Addressed all minor corrections and improvements on readability.

### **Reply to the Comments of Referee Aku Riihelä**

Main comments:

1.The algorithm components are clearly described, but the overall scheme not quite so clearly. I gathered from section 3 that the MOSAiC measurement data is first used as input for the ShupeTurner cloud retrievals, which are in turn used as input for TCARS, correct? A simple flow chart could bring clarity here, though the number of display elements in the manuscript is already very large. Please consider how to make the processing flow fully unambiguous to the reader.

We noted the comment and included a flow chart in the appendix to help describe the methodology. We also modified briefly the description in Section 3.2 as follows:

*“The overall workflow of the TCARS framework is shown in Figure A1 detailing the input parameters and the output variables derived from the radiative transfer simulations.”*

2.I may have missed this, but it felt that the uncertainty of the derived flux components was not very clearly discussed. If my assumption of the processing chain in previous point is correct, what is the sensitivity of the flux components to original MOSAiC measurement uncertainties plus whatever the ShupeTurner + TCARS add on top? I was glad to see unambiguous targets for closure of the downwelling components, but the Lanconelli reference is old and thus would not correspond directly with the observational uncertainty, right? And what were the closure targets for the upwelling fluxes?

The discussion of input parameters extrapolating the uncertainties in the radiative simulations was not strongly discussed as pointed out by Referee 1, thus we included the following text in the manuscript to clarify this point.

*“Barrientos-Velasco et al. (2022) examined how uncertainties in various input parameters, such as atmospheric temperature, water vapour, skin temperature, ozone, and surface albedo, propagate through radiative transfer simulations under clear-sky conditions. The findings indicated that the propagated uncertainty in the radiative fluxes was  $\pm 2.6 \text{ W m}^{-2}$  for the Terr-D and  $\pm 3.7 \text{ W m}^{-2}$  for the Sol-D. Potential uncertainties in the cloud product arise from input measurements, the categorical classification of cloud types, and the retrievals applied, and these interact in complex ways with the variable environmental uncertainties associated with spatial variability and scale mismatches for different observations and parameters. While it is nearly impossible to disentangle or quantify the individual impacts of these uncertainties, the radiative closure employed here is one means for assessing the overall uncertainty of all components collectively.”*

3. Section 4 was, for me, challenging to unravel. Besides the sheer length and scope of treated material, there were items I suggest the authors pay attention to:

1. Subsectioning and reuse of tabular content: The present text, particularly in 4.3 and 4.5 is composed of lots and lots of numbers in watts per sq.m with rather invisible movements from one radiative energy budget component to the next in the text. I would advise considering the use of additional subsectioning to separate flux components for clarity, and a careful review of where (and how) it is really necessary to reiterate table contents – particularly if the reiteration is not placed into further context, as is the case towards the end of 4.5 (lines 649-660 in particular).
2. Many descriptions of display elements are to me unnecessarily wordy, often repeating large parts of caption content. This sometimes extended to other parts of the text, e.g. conclusion 2's first half seemed like a very long way to say that the results of Rabe et al. (2024) on the spatial continuity of the longwave flux were confirmed here. Please examine where fewer words could deliver the same message.

*The length of the sections mentioned was reduced and the text was carefully changed to depict the main results. Note that the structure of Section 4, changed as suggested by Referee 2 which suggested that the analysis of Terr-Net flux should be part of the radiation budget during MOSAiC at the surface.*

4. In 4.3 (lines ~490 or so), the message implied from the numbers seems to be that the radiative effects of sea ice albedo changes at TOA are roughly halved by cloud presence, which is the number we're seeing from a variety of other data too. This is a point which could use a bit of spotlight, I suggest you reference e.g. Sledd and L'Ecuyer (2019) on this.

*We agree with the recommendation, and we also appreciate the recommended citation. The text was changed as shown below:*

*“The latter highlights the masking effect that clouds have on TOA reflectance as discussed in Sledd and L'Ecuyer (2019).”*

Minor comments:

- Terminology: If the authors prefer “solar” and “terrestrial” in stead of “shortwave” and “longwave”, sure (even if “downwelling terrestrial” sounds a bit curious for me), but in any case please make the equivalence fully clear early on for those of us used to the latter option.

The terminology was clarified more specifically. We included the following sentence in section 2.1.2: *“For clarity, the manuscript uses the terms solar and terrestrial radiation to refer to broadband shortwave and longwave radiation, respectively (see List A1).”*

- Figures: Most figures in the review version seemed unnecessarily small, affecting readability somewhat. For figures 4-6, I would like to see the closure acceptance limits for the fluxes displayed as colored vertical lines or shaded regions to avoid the need for checking in the text.

The new figures 4-6 were changed accordingly.

- Daytime vs . daily mean albedo (lines 221-225 and Figure B3): Just to confirm since you are comparing with CERES – using the daily mean flux ratio avoids solar elevation screening, but is the resulting number then same as the daily mean albedo used in CERES? During the melting season the sea ice floe’s albedo can have a notable non-symmetric diurnal cycle, is the orbital sampling behind CERES albedo such that it's assuredly an apples to apples comparison, given the varied footprints too?

For the TCARSe1 simulations, we extrapolated the hourly surface albedo from CERES SYN to a minute resolution. For TCARSe2 and TCARSe3, we utilized the computed daily observed surface albedo at ASFS-30 and ASFS-50, respectively. We chose this averaging method to eliminate small-scale variability.

The analysis of several case studies revealed that the measurements taken at ASFS-30 and ASFS-50 captured specific local spatiotemporal variability in solar radiation. This variability resulted in significant differences even between the two stations (see Fig. B3). To address this issue, we decided to use a daily average to minimize the effects of local variability. We did not include the discussion of these cases in the submitted manuscript due to length constraints. However, we stressed the caveats of this assumption more emphatically in the manuscript and reiterated that an in-depth analysis of the surface albedo considering spatiotemporal variabilities and cloudy and cloud-free interactions is needed and remains outside the scope of the current manuscript.

Page 19, line 607:

*“Stapf et al. (2020) argued that the calculation of the solar cloud radiative effect should be reassessed by distinguishing between the roles of cloudless and all-sky surface albedo. They discuss the applicability of a broadband parameterization that considers the presence of liquid clouds. Based on aircraft observations, Jäkel et al. (2024) additionally considered the impact of surface type including snow coverage and melt-pond faction, and compared these observations to the surface albedo scheme of an atmospheric model. Understanding how factors such as surface type, solar zenith angle, and cloud properties including the thermodynamic phase of clouds influences broadband surface albedo is important but beyond the scope of the current work”*