

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 #2

Assessing the radiation budget in the Arctic and the impact of clouds is still challenging and often limited due to missing simultaneous detailed observations of radiation, thermodynamic, and cloud properties. The comprehensive observations of the MOSAiC expedition provide thus a great opportunity to quantify the solar, terrestrial, and net fluxes for the Central Arctic over a complete year and to estimate the cloud radiative effect (CRE). With this work, the authors thus add another important puzzle piece to the Arctic radiation budget and the corresponding cloud impact. In order to estimate the radiation budget and the CRE, different data sets/products and methods are used, i.e., on the one hand, a column (1D) radiative transfer simulation using the TCARS setup and on the other hand, the satellite-based CERES SYN product. The results are presented in four main sections with 4.1 presenting an overview of the atmospheric and surface conditions, 4.2 summarizing the results of the cloudless simulations with TCARS and CERES SYN, 4.3 presenting radiative closure studies, 4.4 evaluation of the net terrestrial flux and 4.5 presenting the radiation budget and CRE during MOSAiC.

Major comments:

The paper is generally clearly written, and the methodology is sound. My major concern is the length of the manuscript, particularly the extensive sections 4.2 and 4.3. The number of figures, tables, and numbers provided is simply overwhelming, i.e., 10 figs. + 12 figs. in the appendix, 6 tables and 3 tables in the appendix. Just moving information to the appendix does not solve this problem. I think the discussion of the yearly cycle of the net terrestrial flux (4.4), the radiation budget, and the CRE (4.5) are the most interesting sections. However, with the detailed and lengthy comparisons in the sections before, it is hard to follow and keep the reader's attention (which is really a pity since the results of 4.4 and 4.5 are really a highlight).

I recommend reorganizing the manuscript and drastically shortening/synthesizing the content. I have some recommendations but I would also leave it to the authors to decide which parts to shorten/remove.

1) It is unclear to me why you need 3 different simulation setups with 3 different surface albedo inputs. Eventually, you anyhow focus on TCARSe2. Of course, taking into account the impact of the spatial variability (of the surface albedo) is of interest, particularly since you also use a satellite product with a larger footprint. However, for the reader, it would have been much easier to follow if you had a dedicated section on this (e.g., “Impact of spatial variability”) and otherwise used one TCARS product only (which you think is best suited for your analysis).

The purpose of using different sets of simulations was to better differentiate the sources of flux differences. The TCARSe1 simulations, which considered the CERES SYN surface albedo, allowed for a direct comparison of cloudless simulations without attributing significant differences to other variables. Additionally, this experiment enabled us to quantify the flux differences related to cloud properties between the CERES SYN and TCAR simulations, excluding any differences attributed to surface conditions.

TCARSe2 aimed to evaluate the accuracy of ShupeTurner retrievals while also testing the experimental setup based on local observations. The comparison between TCARSe1 and TCARSe2 helps clarify the radiative interactions between surface conditions and cloud properties. Finally, TCARSe3 was used to incorporate spatial variability into the analysis.

A brief description was included in Section 3.2.

“The objectives of each experiment were: to verify the consistency of radiative flux calculations between TCARS and CERES SYN without adding variables (i.e., TCARSe1); to validate Shupe-Turner retrievals and quantify the radiation budget and cloud radiative effect (i.e., TCARSe2); and to confirm TCARSe2 results while analysing spatial variability (i.e., TCARSe3).”

2) Do you really need section 4.2 “Consistency of cloudless simulations”? 4.3 deals with the radiative closure assessment including also dedicated cloud-less comparisons. Any findings from 4.2 might be perhaps included in the discussion in 4.3. So I would remove this section completely.

We consider section 4.2 necessary because it assesses the consistency of clear-sky simulations between TCARS and CERES SYN during the MOSAiC period, rather than only focusing on its accuracy for approximately 20% of the time. This evaluation is crucial for calculating the CRE. Therefore, it was needed to ensure that any final differences in the CRE calculations were due to cloud properties or surface interactions, rather than discrepancies in the radiative transfer simulations between CERES and TCARS. However, we concur with the overall suggestion to shorten the manuscript; thus, we have decided to reduce the text in this section.

3) For me, the separation of 4.4, particularly Fig. 6, from 4.5 (Fig.7) is unclear. I suggest having one dedicated section about the yearly cycle of the (net) fluxes (including Fig. 6-8) and one section on the yearly cycle of the CRE (including Figs. 9+10).

In my opinion, it makes sense to first present Terr-N SFC in Fig.7a and subsequently zoom in and discuss the monthly distributions of Terr-N (Fig. 6), since the bimodality can not be seen in the monthly boxplots. For me, this would be a more natural way to follow.

The text of that section was modified accordingly. Also, some paragraphs that were not necessarily relevant in the section were deleted.

Specific comments:

line 36: Reference “D. and Rex”. Please check.

Corrected

line 37: “Satellite observations”: Do you refer to particular ones? Of clouds?

Specified to ‘Satellite observations of clouds’

line 42: “Additionally, Hartmann and Ceppi...”: What is the logical connection between this sentence (trends in radiation) and the one before (comparing ground-based/satellite cloud observations)? Please rewrite/motivate...

The text was modified as follows:

“Satellite observations of clouds have particular advantages due to their spatial coverage and long duration of service (Stubenrauch et al., 2013; Christensen et al., 2016; Huang et al., 2017).”

lines 48 ff: Can you also comment on the uncertainties in the trends of these studies in particular of the trends in clouds?

We included the following text into that section:

“Duncan et al. (2020) analyse the trends in the surface radiation budget of the Arctic boreal zone using CERES Energy Balanced and Filled (EBAF) data products from 2001 to 2017 and report a decrease of the reflected solar radiation by $1.3 \pm 0.6 \text{ W m}^{-2}$ per decade and an increase of the outgoing terrestrial radiation by $1.1 \pm 0.4 \text{ W m}^{-2}$ per decade suggesting a greening of the Arctic tundra. These results are subject to the overall monthly uncertainty of 3 W m^{-2} for the solar and terrestrial fluxes (Loeb et al., 2018).

The study by Lelli et al. (2023) extensively analysed the regional and seasonal radiative effects of clouds on radiation, based on GOME and SCIAMACHY observations over two decades. This research revealed that the reduction in Arctic albedo at the top of the atmosphere is offset by an increase in atmospheric reflectivity, attributed to a significant increase in liquid-phase clouds. This increase is dependent on changes in the regional Arctic climate and the underlying surface type. It is important to note that their findings are affected by uncertainties in cloud properties of $\pm 0.4 \%$, which have a spatial impact but not a temporal one (see their Appendix E).”

lines 99-100: Ebell et al. (2022) refers to the HATPRO measurements, not MiRAC-P. To be corrected in Table 1, too.

Corrected

lines 122-123: verb missing

Corrected

lines 149-150: “which is defined for the calculations as 20 km”. What do you mean?

The sentence was misleading, so we decided to delete it.

lines 152-153: Are cloud properties in the CERES SYN data set provided for each cloud layer? Can you comment here on the vertical cloud profile information?

Yes. Cloud properties are also provided at 4 different heights.

The section was edited as follows:

“Considered in this study are cloud base pressure (PB), cloud top pressure (PT , cloud top temperature, cloud base temperature, cloud fraction (CF), LWP, ice water path (IWP), liquid droplet effective radius $r_{E,L}$, and ice crystal effective radius $r_{E,I}$. The latter products are available for the entire atmospheric column and at four different heights (i.e. surface to 700 mb, 700 to 500 mb, 500 to 300 mb, and higher than 300 mb).”

lines 172-174: So, what was the result of the closure analysis? Did the ST2015 product perform better than the ARM Microbase cloud product?

The following sentence was included:

“The results in ST2015 indicated that ShupeTurner performed better than the ARM Microbase cloud retrievals.”

lines 179-180: The sentence is odd. Please rewrite.

The sentence was edited as follows:

“The calibration of this radar was adjusted to align with the atmospheric profiles of temperature, pressure, and humidity obtained from the radiosondes. The LWP data was taken from either of the two microwave radiometers to ensure maximum coverage.”

line 197-198: “TCARS uses various sources of input data such as ... aerosols..”: Please mention already in this section that aerosol data are actually not included in the TCARS simulations of this study. It is mentioned later, but it would be helpful here.

For clarity, we deleted the “aerosols” as it is not applied in the current study.

line 209: “driplet” should be droplet

Corrected

line 253: “which subtracts the observed radiative flux from the cloudless simulation”: this should be the other way around, i.e., which subtracts the radiative flux of the cloudless simulation from the observed radiative flux

True. The sentence was corrected.

line 256: “The atmospheric CRE...”: You introduce the atmospheric CRE but don’t show any results. Why?

We mentioned the atmospheric CRE as the difference between the TOA and the surface, while before submission we had a longer description on this topic, we reduced it due to length concerns. We plan to delve deeper into the atmospheric CRE and heating rates for MOSAiC in an upcoming publication as it is mentioned in Section 5.

lines 267-268: “...into four periods:...”: but you explicitly mention only two (Oct 15-Mar13 and Mar 14-Sep20). Can you mention all four?

Thanks for pointing this out. We corrected the text as follows:

“We divided the time series into four periods: two during the polar night (from October 15 to December 31, 2019, and from January 1 to March 13, 2020) and two during the polar day (from March 14 to May 31, 2020, and from June 1 to September 20, 2020) to characterise seasonal differences (Fig. 1).

lines 282-283: “The stratospheric temperature also dropped below 200 K (Fig. C1).”: Fig C1 does not depict the stratospheric temperature. Please explain.

This section was corrected as shown below.

“The 2-m air temperature decreased as low as 231 K, (Fig. B2). However, there were two exceptions to this pattern.

Fig. 3: Can you introduce/present Fig.3 in section 4.1? It is never presented in detail in the manuscript but it would make sense to do so in the “overview of atmospheric conditions” part.

Fig. 3: So in ShupeTurner, the effective radius of liquid cloud droplets is a fixed value, also in the vertical, right? Could be mentioned once more when presenting the results.

Figure 3. is now introduced in Section 4.1 General overview of atmospheric and surface conditions and it is specified that the effective radius does not vary in height.

The following paragraph is included

“The microphysical cloud properties based on CERES SYN and ShupeTurner are illustrated in Figure 3. The annual variation of the LWP and IWP are depicted in box plots and the annual variation of $r_{E,L}$ and $r_{E,I}$ are shown at four different heights corresponding to those described in the CERES SYN products (see Section 2.2). For CERES SYN and ShupeTurner, the values of LWP increase during polar day as it is expected. The IWC decreases during summer based on the ShupeTurner dataset, but CERES SYN does not follow the same tendency. It is important to note that the CERES SYN statistics are influenced by periods with optically thick clouds, and there are times when the presence of clouds is either missed or underestimated (see Fig. 2). The $r_{E,L}$ shows variation with height according to the CERES SYN data, while the ShupeTurner dataset maintains a constant value of 9 μm . Conversely, the $r_{E,I}$ is greater in the ShupeTurner data compared to the CERES SYN data, showing an overall decrease with height.”

line 413: Should rather be “(Fig 4d, 4l)” only since you focus on Terr-D at this stage.

Corrected

line 421: Should be Fig. 4 not C5

Corrected

lines 426 ff: why do you use the surface albedo from CERES at all since you later also point out that it is underestimated?

The motivation for using CERES SYN albedo was to confirm the consistency of TCARS methodology without including another variable that could mask or interact with the radiative flux comparison. A brief description was included in Section 3.2 stressing this point.

“The objectives of each experiment were: to verify the consistency of radiative flux calculations between TCARS and CERES SYN without adding variables (i.e., TCARSe1); to validate Shupe-Turner retrievals and quantify the radiation budget and cloud radiative effect (i.e., TCARSe2); and to confirm TCARSe2 results while analysing spatial variability (i.e., TCARSe3).”

lines 438 ff: LWP and IWP are not the only values impacting the atmospheric opacity. What about the effective radii? This needs to be discussed jointly.

Additional sentences were included in this direction. See below:

“The size of r_e is comparatively larger for CERES SYN than ShupeTurner, resulting in a smaller optical depth as less sunlight is reflected.

lines 466 ff: You analyzed hourly mean values for different single-layer cloud types. How many cases do you have for each class in the end? What if you have different single-layer cloud types within one hour? I assume that this is actually quite often the case.

The classification was made considering the minute resolution of the simulations, once the cases were sorted into the different classes (liquid, ice mixed-phase), each individual class was interpolated to hourly resolution to calculate the hourly flux difference.

Considering single-layer clouds only, about 32.2 % of the time were liquid clouds, 33.1 % were ice clouds, 14.7 % were mixed-phase clouds, and 20 % contained snow or liquid precipitation. The occurrence of each cloud type was included in Table A2 and Table A3. Note that the percentages calculated for Table A3 only considers the period where the solar flux was available.

lines 462-463: “For ice clouds, there is a positive bias for TCARS of about 20 Wm^{-2} , suggesting an overestimation in cloud opacity,...”: This should be an underestimation of cloud opacity.

The text was corrected.

line 467: “as they absorb Sol-D less effectively.” Just to be precise, the clouds do not absorb, but the atmospheric gases in the cloud layer.

Changed to as they scatter Sol-D more effectively.

lines 467-477 and ff: To be sure that I understood it correctly: For the Terr comparison, differences only occur because different CERES SYN columns are used due to the different ground stations considered, right? The TCARS Terr simulations are the same for each location, right? And for the solar part, the TCARS simulations differ also because of the different surface albedos that are used for the different stations, correct?

Yes, that is correct.

lines 499 ff: This is about the net SURFACE terrestrial radiative flux. At least I think so... Please add this information.

The structure of that section was edited as suggested earlier. This section is now part of the subsection of analysis at the surface of the radiation budget during MOSAiC.

lines 505-506: The sentence is redundant. “we analyse opacity by examining the net terrestrial flux as this variable (i.e. the net terrestrial flux) is related to ... opacity in the terrestrial spectral range”.

The sentence was deleted.

lines 509-511: This could be mentioned in the introduction of this section. See my questions before.

Suggestion considered and included in the introduction of the section.

line 525: “data limitation was based on”. Please rephrase.

Changed to:

“For October and November 2019, the data was limited to the data coverage of ASFS-40 observations and for March and April, the datasets were limited to the data availability at Met-City. The rest of the months were limited to the observations at ASFS-30.”

line 531: “showing relatively similar distributions”, referring to what exactly?

Referring to the the Terr-N flux. The text was clarified as follows:

“In October 2019, the last 15 days of the month were analysed, and in September 2020, the first 20 days were evaluated, showing relatively similar distributions of Terr-N flux and indicating a higher occurrence of opaque atmospheres.”

line 586: “Figures 7c and 7b” should be 7c and 7d

Yes. This was corrected.

line 587: “With an underestimation of surface albedo by -21.01 %” Why do you use the CERES SYN surface albedo at all in TCARS then?

As it was mentioned earlier, the motivation for using CERES SYN albedo was to confirm the consistency of TCARS methodology without including another variable that could mask or interact with the radiative flux comparison.

lines 606-611: Regarding the higher values of Terr-N TOA for CERES SYN: are these primarily due to the lower/underestimated surface albedo values? Or is this also an effect of how clouds are represented in the data set?

We believe the reviewer refers to the Sol-N flux. This behaviour is due to the interactions of clouds with a lower surface albedo and lower sun elevations for this period of the year. The latter was mentioned in the manuscript.

line 609: “indicating that less solar radiation is absorbed... in TCARS simulations”. This should be “more solar radiation is absorbed” since the TCARS value is smaller than the CERES SYN value

Corrected

line 612: “At the TOA, the radiation budget...” Since you analyze solar and terrestrial fluxes (down, up, net) and total (solar + terrestrial) fluxes in this paper, the reader can easily mix up the different components. Please check throughout the manuscript that you always use a clear naming. Sometimes, you can deduce from the context what kind of flux is meant but I would try to be as clear as possible. Here, the total net radiation budget (sum of net solar and net terrestrial) is discussed.

Thanks for the suggestion. We went through the manuscript again and made sure to clarify each flux to avoid any confusion

lines 661-662: The accuracy of the cloud micro- and macrophysical products is indirectly evaluated in terms of radiative closure studies. Maybe you can add this here.

Sure, the sentence is included.

line 693: “surface” Net-Terr flux?

Yes, corrected.

line 710: “net/total” CRE?

Corrected to Total CRE.

Fig.4 : Can you add x-ticks in all subplots?

Ticks were included and the plot was modified following the suggestion of Referee 1.

Fig.4: I would still expect one baseline TCARS setup to be shown. I find using a mixture of e1 and e2 confusing. I would simply use e2 and say the the Terr simulations are extended to cases when solar radiation calculations are not possible since the Terr flux calculations in e1 and e2 anyhow do not differ (apart from the time period being covered).

We agree with the Referee’s comment. We changed the description following the suggestion. We included a clarification on this aspect in the last paragraph of Section 4.3 (Radiative closure assessment)

“For clarity, the analysis with TCARS simulations will refer to experiment TCARSe2 for the solar and terrestrial fluxes. Note that the analysis of the terrestrial flux is extended to the data availability of TCARSe1

(Fig. A2), as the parameter that was altered in the input (i.e., surface albedo) does not influence the calculations of terrestrial fluxes.”

Fig. 6 This is for the surface, right?

Yes. The figure caption was modified.

Fig. 7 Please specify the fluxes: “net” terrestrial and “net” solar flux at the surface, net radiation budget at the surface: maybe you can introduce all terms when you introduce the CRE. Also, be consistent: on the y-axis (e) it says “Total SFC”. I know what you mean, but for clarity, just use one dedicated term for each variable throughout the manuscript.

We refer to net flux as the difference between the downwelling minus the upwelling flux and the total as the sum of the solar and terrestrial flux.

Fig A1. Remove time periods not covered by MOSAiC for clarity.

The plot was changed accordingly