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Comment on acp-2021-1004

Anonymous Referee #1

Referee comment on "Radiative closure and cloud effects on the radiation budget based on satellite and shipborne observations during the Arctic summer research cruise, PS106" by Carola Barrientos-Velasco et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1004-RC1, 2022

Review of "Radiative closure and cloud effects on the radiation budget based on satellite and ship-borne observations during the Arctic summer research cruise PS106"

by Carola Barrientos-Velasco, Hartwig Deneke, Anja Hünerbein, Hannes J. Griesche, Patric Seifert, and Andreas Macke

The authors present results of the research cruise PS106 that took place in the Svalbard region from May to July in 2017. In their study, they focus on cloud observations from both ship- and satellite-based measurements. This data is used for radiative closure studies and also to assess the cloud effects on the radiation budget.

Enhancing cloud observations in the Arctic is crucial to better understand Arctic clouds, their radiative impact and their impact on the Arctic climate system. Especially in the harsh Arctic environment and especially in the Central Arctic, it is quite challenging to acquire such data.

The present study is thus of high relevance. I very much appreciated that the campaign measurements were also set into a larger context using the CERES products.

However, I have some further general comments:

With 18 figures, 4 tables, 5 supplementary figures and 7 supplementary tables, the reader is quite overwhelmed with information. I strongly recommend to reduce the number of

figures and tables and try to concentrate on the key information. I have added some suggestions in the specific comments, but maybe there is a way to further reduce the length of the manuscript. I think that this would even raise the value of the manuscript. E.g. the discussion on the direct radiative effects of aerosols open up a new topic which is distracting from the key topic of the manuscript. This is definitely a section which I strongly recommend to remove.

I also think that "radiative closure" could be more carefully discussed. At the moment, only the bias is considered and I am not so sure how valid the conclusions are which are drawn here. Please also see the specific comments.

Specific comments:

ll 160-163: please provide more details on how q_L is determined (equation?) No information on the retrieval of r_E,L is given. How do you handle mixed-phase clouds, since in these cases the "standard" retrievals for liquid clouds do not work?

ll 164-166: maybe Q_i can be introduced here as well. I assume that it is calculated by vertically integrating over q_i?

ll 202-206: I assume that the cloud properties from CERES are not vertically resolved. Please clarify in the text.

ll 220-: Is the vertical grid used in T-CARS determined by the Cloudnet vertical grid? Do you need to interpolate cloud properties in time? I assume that the Cloudnet temporal grid has a resolution of 30 s.

ll 229: Do you have independent measurements of surface skin temperature (maybe for a shorter time period) during the Polarstern cruise? How well does T10m with the surface skin temperature agree? I am wondering how large the uncertainty of the LW upward flux at the surface is due to this assumption.

ll 279- … and Fig 2a: Is the intercomparison of CERES and ERA5 surface albedo really needed? You further use CERES, which you state "yield accurate results". An evaluation of ERA5 surface albedo is in my option out of the scope of this study. Since the paper is already very long, this discussion could be removed and just the CERES albedo shown.

ll 296-… and Fig 2b) Is there a need to show the T10m from the radiosonde? There are sometimes larger discrepancies between T10m from the mast and the radiosonde. I am also not sure how trustworthy the radiosonde measurements are at that height. I suggest to omit the RS T10m here. If you have surface skin temperature measurements which have been taken during the Polarstern cruise (also for shorter time periods), it would be interesting to see those ones.

Fig 3. (but also the other figures): The figure captions could be more concise. In this way the relevant information can be better captured. E.g. for Fig 3:
"Time-height plot of atmospheric profiles obtained along the PS106 cruise track. (a) ERA5 atmospheric temperature anomalies. (b) ERA 5 specific humidity anomalies. Anomalies have been calculated with respect to the mean profiles of the cruise. (c) Mean profiles of atmospheric temperature and (d) mean profiles of specific humidity for ERA5 (orange) and radiosondes (blue). The sub-Arctic summer standard atmosphere (Anderson et al., 1986) is displayed in black. The grey-shaded area indicates the minimum and maximum values, while the brownish-shaded area shows the interquartile range of the ERA5 profiles."

Fig. 4a: I am not sure which information to take from this plot. It is also not really discussed in the text. I would remove this plot. Instead you could add a plot of the vertical cloud fraction/ frequency of cloud occurrence for the entire cruise.

Fig. 5 a: I find that, in stacked column charts, individual values are sometimes difficult to capture. Since the focus in Fig. 5b is on single-layer clouds, it would be good if the values (RFO) of the single-layer clouds could be easily read. So I suggest to change the order of the columns. At the bottom "single-layer", followed by "multi-layer", then "clear sky" and then "no data" on top. The colors could be the same.

Fig. 5b: I am confused here. In Fig. 5 b, the phase of single-layer clouds shall be shown. Since you focus on this class only, why are there again the "clear-sky" and "no data" categories? Either you show "clear-sky", "no data", "multi-layer", and "single-layer", while for single-layer you discriminate also the different phases (that would be somehow the same plot as in Fig. 5a except that the single-layer class has further sub-classes). Or you simply show the single-layer statistics only so that the phase types sum up to 100%. The latter would be my preference.

Why can the thermodynamic phase not be given in case that there is precipitation?

ll 387-396: As far as I understood, water vapor is also taken from the ERA5 reanalysis. What is the uncertainty due to the fact that hourly values are interpolated to the minute resolution of the RT simulations? You only include instrumental uncertainties in your sensitivity analysis but I think that the interpolation of hourly values might also cause uncertainties at least in the same order of magnitude. Same for the temperature profile.

You have temporally highly resolved IWV measurement on board from the HATPRO radiometer. Why didn't you include these ones?

ll 397-404: Also for the uncertainty of the surface skin temperature a small value of 0.3°C is used (at least in the final estimate of the overall uncertainty). This uncertainty is related to measurement accuracy of the T10m temperature sensor but does not include the uncertainty due the assumption that Ts=T10m. Can you comment?

ll 435-437: I would argue that the uncertainty estimates that you use to determine the overall uncertainty are rather at the lower end and most likely much higher due to the reasons mentioned before. Also for the SW, you omit the uncertainty related to aerosols which probably also has a high impact.

ll 450-451: I would add "at the TOA" at the end of the sentence.

ll 477: "Therefore, our results confirm..."
It is true that the bias of LW_down in the clear sky case is smaller than your uncertainty estimate. Still I am a little bit concerned by this bias since you clearly see a systematic error which hints at too low temperatures and/or humidity in the RT simulations. Did you try to provide a better estimate of your input profiles, i.e. including the T10m in the lowest atmospheric level or by including the IWV from MWR or GPS?

I am also wondering if the bias only is a good indicator that RC is reached. Looking at the SWD flux differences there are quite some larger differences (second peak around -30 Wm-2, where does this come from?). So shouldn't be also the RMSE or STD discussed with respect to radiative closure?

Fig. 7: Could you add directly bias, RMSD,…. in d-b instead of putting them in the Table 2? (and enlarge Figures d-b). This would be much easier for the reader. Same for the plots of the other case studies.

ll 499-500: "…that RC is achieved…"
Again, this holds maybe for the daily mean value when you refer to the bias, but for individual 10 min intervals, RC is not always reached. Maybe you can more carefully differentiate here. RC depends also on the averaging time (10-min values, daily mean values,..)

ll 502-503: I think that also 3D cloud effects play a substantial role. Can you comment on this?

ll 504-511: How do you compare the vertical profile of ref_ice and ref_liq with the values from CERES (which are most likely not vertically resolved and representative for only a certain part of the cloud?)?

ll 512-516: I would be careful in stating that the net CRE_sfc from T-CARS and CERES are consistent. The daily mean values are similar but there are quite big differences in the individual values.

ll 525-527: Q_L is from the MWR. So why could it not be derived "due to uncorrected attenuation"?

ll 535-540: I see the point that in case of low LWP, the relative uncertainty of MWR LWP is high. Do you have LWP from IR measurements on this day as well? Maybe this would give you a better idea. However, I do not trust the CERES LWP neither. How realistic are the constant reff values of CERES?

ll 561-564: Still I am concerned about this bias in the LW simulation in T-CARS…

l 569: What is the "pristine" CERES product? The different CERES products need to be introduced in more detail in the data section.

l 580- …: "All-sky", shouldn't this be discussed rather in the next section?

Fig 12/Table B1: all values of Table B1 are already included here. Table B1 is not needed.

l 601/Fig. 13: Why is CERES CS and T-CARS shown as well in Fig.13? The focus is on the CERES all-sky flux. The other variables should be removed from all subplots in Fig. 13.

l 621: "Similarly to LWD, there is relatively good agreement between the SWD CERES simulations and observations …" "SWD" to be added.
This is difficult to see from Fig 13b alone. Please refer to 14 b as well.

ll 675-698: The different CERES products have not been introduced in detail which is quite important for this section since it is not straight forward how these products have been derived and what are they representing in detail. However, I strongly recommend to remove this whole section from the manuscript. The manuscript is already very long and

this part opens a completely new topic which distracts from the actual topic (as also mentioned in l 699). Such a study is definitely of interest but should be presented in a separate paper.

l 716: Maybe it is hard to see since the net CRE (I assume it is the net CRE, please clarify in text and figure caption) is shown while surface albedo, for example, only affect the SW CRE.

I am also not sure if Fig 17 provides new insights since many statements in the text could also be made without this figure. I am wondering if this detailed discussion is really needed and the whole section (l 708-734) could be shortened.

l 725 - 734: This paragraph is a repetition of the previous paragraph. Please check.

Fig. 15 How is ”the Arctic” in this case defined? Please mention this also in the figure caption and remind the reader of the time period considered. I cannot distinguish “grey” from “black”.

Typos/grammar

l 84: macro with “-“

l 345: macro with “-“

ll 432-433: “Scatter plots…” not a full sentence.

l 721: This is not a full sentence.