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

I thank the authors for the thorough replies and adjustments to the manuscript. Before publication, I think that it is important that some few further changes are applied to the manuscript. Some important information, for example, which has been given in the reply to the reviewer, has not been or only partly included in the manuscript itself. Please see my specific comments below.
To better follow the previous discussion, I have included my original questions/comments (in black), the answer of the authors (grey) and my follow-up comment (red).

Comment 1

old comment: 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?

answer: It was not our aim to describe in detail the Cloudnet methodology since this was covered in Griesche et al., 2020. However, we hope that with the modified version of the manuscript the question has been answered. See below an extract of the modified text in Section 2.1.2 (Page 6).

new comment: I didn't meant to have a detailed explication/equation on how the adiabatic liquid water content is calculated. Lines 170- 178 are not needed (but thanks for clarifying!). Just mentioning that the liquid water content profile is assumed to be an adiabatic profile is fine. Are you scaling the profile with the LWP of the MWR? This information could be added after the sentence in line 167.
What is not clear to me is how reff can be calculated by Frisch et al (2002) in case of mixed-phase clouds. Since Z is dominated by the ice you do not have a Z for the liquid cloud droplets only. Please comment and also add further inormation in the manuscript.

Comment 2

old comment: 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.

answer: The radiative transfer model has a limit of 200 atmospheric levels. Therefore, it was not possible to proceed with the exact same vertical grid as Cloudnet since Cloudnet has 413 atmospheric levels equally separated every by ~31.2 metres, starting from 165 metres to 13 km. It was opted to use the double Cloudnet's vertical grid (i.e., 62.5 m) for the first 10 km of the atmosphere. The temporal resolution of Cloudnet is about 30 s, so the cloud properties were interpolated to every minute for the simulations. This is explained in more detail in section 2.3 of the manuscript.

new comment: Please expicitely mention in the manuscript what you did with the cloud properties, e.g. averaged 2 bins vertically and linear interpolation in time. You just mention which grid you use in the RTM but not how you adjust the cloud properties accordingly.

Comment 3

old comment: Fig. 5 b: Why can the thermodynamic phase not be given in case that there is precipitation?

answer: As per the standarized Cloudnet classification only liquid is classified as precipitation ('drizzle/rain and cloud droplets' and drizzle or rain). When there is snowfall, there is a flag within Cloudnet data related to an uncorrected attenuation. It has not been planned to subdivide precipitation within the standard Cloudnet target classification.

new comment: I am sorry. This question was misleading. What I was hinting at: single-layer clouds can be devided into liquid, ice, mixed-phase. Why is there another category precipitation for single-layer clouds? To me it is not clear why single-layer clouds which are precipitating can not be classified as well.

In the figure caption you mention "mixed-phase clouds of type 1 or 2" but only the first one is shown.

Comment 4

old comment: 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.

answer: The values obtained from the sensitivity analysis are an approximate based on the input parameters used for the radiative transfer simulations during clear-sky conditions. Thus, it is used as a reference for subsequent studies.

While we did not include aerosols in the simulations due to the lack of observations, we quantified the radiative effect on the SWD from CERES with Figure B1. Under clear-sky conditions, the aerosols can contribute with a mean of -10.8 Wm^{-2} and under cloudy conditions up to -4.5 10.8 Wm^{-2}.

new comment: I would have liked to seen a least a short critical discussion in the manuscript about how realistic these assumptions on the uncertainties in the input parameters are and which uncertainties might be there in addition which have not been considered. As I mentioned, there are other uncertainties (using hourly model data not capturing the full temporal variability of water vapor and temperature, the assumption of Ts=T10m,…)

I think that it is really important to remind the reader of those ones as well.

Comment 5

old comment: “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?

answer: “Initially, we compared different atmospheric input data sets into the radiative transfer model [i.e., radiosondes, ERA5] and analysed which results were closer to the observations. However, we did not find large differences between both simulations since, in general, there is a good agreement between ERA5 and radiosondes (see Fig. 3c and 3d). For consistency, we decided to use ERA5 as a homogeneous input parameter. Additionally, we created several sensitivity analyses varying the humidity and temperature profiles to determine how more humid or warmer the atmosphere needed to be to match the LWD observations. In some cases, the mean flux difference was larger than the radiosonde uncertainties (see section 3.2 and Table A1). Therefore, we hypothesised that the biases of the downward LW might be due to the temperature fluctuation of shipborne instrumentation working near the pyrgeometer (see conclusion point 3).”

and later to a similar comment:
“The LW bias has been observed for CERES and T-CARS (see Fig. 12a and Fig. 12c). Our hypothesis also includes that the operation of the shipborne instrument near the pyrgeometer might have caused an increase in the instrumental uncertainties, or the ship acted as a rather warm island in contrast to the ice floe. We, therefore, plan to compare shipborne and ice floe pyrgeometer measurements carried out during MOSAiC to test this hypothesis. (see conclusion number 2).”

new comment: I think that this information, i.e. the further analyses you did to better understand the bias (i.e. varying input, testing with radiosonde,…) and the conclusion that the pyrgeometer measurements are likely influenced e.g. by the shipborne instrumentaion are of high relevance. This should definitely be included in more detail in the manuscript and in particular when you present and discuss the results of Fig.12. Just mentioning it as a aside in the conclusion points 2 and 3 is not sufficient.

Comment 6

old comment: 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?

answer: The second peak around -30Wm-2 come from instances where the position of the sun was near the edge of Polarstern (65.6° in solar zenith angle) at 17:00Z approximately (see Fig. 7b). The multiple reflections caused by the relatively low sun position might have caused an increase in the SWD, increasing the bias from the simulations and observations. […]

new comment: The reason for this second peak should also be included in the manuscript, i.e. when discussing of Fig. 12.