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Comment on acp-2021-1004

Anonymous Referee #2

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-RC2, 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 Barrientos-Velasco et al.

In this study, the authors undertake the analysis of ship-based measurements of cloud, atmosphere, and radiative flux parameters against CERES satellite observations and radiative transfer simulations taken during a 1,5 month cruise on the Arctic Ocean in 2017. The topic is very suitable for ACP and the manuscript is well-designed and evidently based on a large body of work in planning and execution of the measurements and the subsequent analysis. The literature of investigating and verifying satellite-based radiative flux component estimates and assessing closure/CRE effects over the Arctic has been too sparse, so there is a clear need for this type of contribution.

Overall, I found the manuscript clearly written with a logical outline to follow. Measurement uncertainties appeared quite well considered and the authors demonstrated a good grasp of existing literature and sources of discrepancy likely to affect the obtained results. The suite of observation data was quite good, although this reviewer lamented the lack of even one source of SWU data for albedo quantification, but this was apparently caused by the continuous cruising vs. a drifting ice camp. The authors' plans for a similar analysis with MOSAIC data should well address this limit, though.

There were some issues where clarifications and revisions of the text appeared in order, and some aspects of the visualizations could still be improved further, but overall this reviewer was not able to detect any major issues with the analysis as presented. With a careful revision, this reviewer believes that the manuscript will attain quality

commensurate with publication in ACP.

Comments by line numbers or content piece:

Ln 49-50: The data sources used by Riihelä et al. were CERES, GEWEX SRB (a separate dataset), and flux components calculated with the FluxNet-Streamer RT code driven primarily by CLARA cloud and surface parameters. Please clarify this point and note GEWEX data.

Ln 147: Upon first read, I expected to find the specs for the horizontal size and resolution of the "pixel grid", only realizing later that the authors wanted to say that there is only one 'stack' of grid cells in the vertical direction. Please revise to clarify, noting at least the ballpark figure or estimate of the horizontal coverage/footprint of the shipborne measurements.

Ln 159: If the QL retrievals are based on training against radiosondes, are you certain that the relationships based on a single source site in Ny-Ålesund are sufficiently robust to work anywhere else over the Arctic Ocean?

Ln 165 and 170-172: The impacts of rain and liquid/ice mixtures on QL are noted, but isn't QI affected just as well, as cloud radar reflectivity is a driver for it too?

Ln 183-185: Here it was difficult to follow what it means when "Cloudnet pixel type…(is) assigned value to zero". Does it mean that aerosols and insects are discarded from analysis entirely? Yet the later manuscript estimates CERES aerosol radiative effects, would there have been a chance to analyze similar aerosol effects from in situ data? This is a bit confusing.

Section 2.2: The CERES data product background is nicely described, but please also state the name of the data product used. Is it SYN1deg?

Ln 227 – 228: The text reads like the PS106 radiosonde data was assimilated into ERA5. Was this indeed the case?

Ln 242 – 244: The impact of ice crystal habits on RT has been investigated and the effects are not negligible (e.g. Wendisch et al., 2005: https://doi.org/10.1029/2004JD005294). Please provide some consideration for the potential impacts of assuming spherical ice crystals in T-CARS?

Ln 289: Please be careful here – the ERA5 underestimation described by Pohl et al. had its roots also in the use of (simple) literature-based constants for the albedo of various ocean/ice surfaces – if SIC and ice albedo were perfectly simulated but melt ponds missing, the resulting albedo should be an overestimation since melt ponds darken the surface relative to snow or bare ice. The text now suggests that missed melt ponds will result in albedo underestimation, which is not generally so.

Ln 414: The SWU effect is very large, but quite consistent with e.g. radiative kernel calculations for radiative energy balance disturbance following a certain change in albedo (e.g. Bright and O'Halloran, 2019) - you may wish to note this for reinforced belief in the result given.

Bright, R. M., & O'Halloran, T. L. (2019). Developing a monthly radiative kernel for surface albedo change from satellite climatologies of Earth's shortwave radiation budget: CACK v1. 0. Geoscientific Model Development, 12(9), 3975-3990.

Ln 432 – Looks like a broken reference here to a scatterplot figure X?

Ln 581 - 582: A larger negative bias in CERES all-sky fluxes due to "the presence of clouds" seems like a half-formed sentence. Clouds are included in all-sky fluxes in every case, how do they now contribute to bias increases? Please be more specific.

Ln 611: Interesting to see a fog case noted, since those would be expected to be the ones where satellite-based fluxes could be very biased since fog conditions are challenging for them. Was this the only case of fog during the cruise?

Ln 719 – 734: Here the attention seemingly slipped, resulting in broad repetition of content between the two paragraphs and generally hard to follow descriptions. Fig 17c and d are not really "subdivisions" of 17b since the y-axis unit is not the same, but they are the same sample set divided by albedo threshold. Please revise this section carefully for consistency and clarity.

Figures 2 to 4: Since you already have the visualization available on Polarstern being in open water, MIZ, or dense ice in Fig 16, why not include the same information here? It is especially relevant for Fig 2.

Figures 8 and 10: Please note that light yellow is a color very easily lost during printing, perhaps a shade or two darker would be more apparent.

Figure 11: The “pale yellow” shading appeared either red or orange (on screen and paper) – or is it the rectangular regions at ~10Z and ~23Z that you refer to here? Also, on this figure it seems that the Cloudnet-CERES differences in QL and QI are quite stable in time, but the CRE difference fluctuates considerably? I may have missed the explanation in the text, but why is this the case?