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Comment on acp-2021-812

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Author comment on "An assessment of macrophysical and microphysical cloud properties driving radiative forcing of shallow trade-wind clouds" by Anna E. Luebke et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-812-AC1, 2022

We would like to thank the referees for taking the time to review the manuscript and provide constructive questions and comments for its improvement. Each comment has been thoughtfully considered and the manuscript has been revised accordingly. The comments are listed in their entirety below with our responses directly following in a different font. Line numbers and figures with revisions refer to the lines and figures of the attached document, which contains a markup to show differences between the original and revised manuscript.

Responses to Referee #1

Specific Comments:

L50: The authors stated that "… irradiance is not measured directly, which is one reason why airborne observations of this quantity are so important …". Did the authors mean that the airborne can measure the irradiance directly? If so, could you please briefly explain how as this is a highlight of your study compared to other studies only using satellite retrievals?

We have reworded the text in this paragraph to make it more clear that the airborne measurements by the broadband radiometer are in-situ measurements of irradiance. Also, as we state in the text on L.52 – 53, "the radiative flux densities are usually derived from combinations of radiance observations and radiative transfer models or the Clouds and the Earth's Radiant Energy System (CERES) instruments." By using combinations of observations or models to retrieve the irradiance, significant uncertainties are introduced into the retrieved irradiance values. A direct measurement of the irradiance does not suffer from those same uncertainties. A further benefit is that we can ensure the instruments are calibrated before and after the measurement campaign. To address this, the text has been revised on L.55 to include that "uncertainties are introduced due to the assumptions made in the calculations of irradiance" and on L.58 – 60, which says, "Among these measurements are in-situ airborne observations of solar and terrestrial broadband irradiance by broadband radiometers, which directly measure irradiance without the constraints of satellite-based measurements."

L.89: Why is the circle pattern independent of the meteorological situation?

The circle flight pattern was repeated in the same location regardless of the specific meteorological situation at that location on any given flight day. Furthermore, the synoptic situation is known to be very stable in this region at the time of the campaign, and trade-wind clouds appear regularly. Instead, the focus of the flight strategy was achieving flights at differing times of day to fully capture the diurnal cycle of the region, which is of more interest. The text on L. 93 – 95 has been revised to make this clearer: " As the synoptic situation is largely stable in this region with regularly occurring clouds (Bony et al., 2017), the chosen flight strategy for the campaign was independent of the meteorological situation and instead focused on achieving flights at different times of day to fully capture the diurnal cycle in the region (Vial et al., 2019; Konow et al., 2021)."

L.92: by BACARDI?

We meant to indicate that the observations in the analysis, which are from BACARDI and GOES-16, are constrained to the location defined by the circle flight pattern. The text has been revised on L.96 – 97 to state this more explicitly: "…the work here focuses on the circles so as to capture and constrain the observations used in this analysis to a single location."

L.96: Do the uncertainties depend on the wavelength? Are these instrumental uncertainties? If so, could you please provide references.

The uncertainties reported here are not dependent on wavelength because the radiometers provide broadband irradiances, not spectrally resolved data that are integrated afterwards. The instruments are calibrated before and after each campaign by the manufacturer, who also provides the calibration-based uncertainty of the sensor sensitivity. Thus, the uncertainty could vary between the same instrument models, and a general reference with these specifications cannot be provided. However, L.102 – 103 now state that the uncertainties are "calculated during calibration procedures before and after the campaign", and the instrument manuals have been included in the references.

L.157-167: Fig.1 and the corresponding discussion could be moved to the appendix since it is only a quality check of the BACARDI measurements and GOES-16 retrievals. Would it be more illustrative to provide the exact measurement time (in UTC), location (lat, lon , and the flight height), and the measurement area in the caption of Figure 1?

We agree with the referee and have moved the figure and associated text to the Appendix (starting on L.418). Two additional panels using different fields of view (FOV) have also been added to the figure to more clearly demonstrate that the FOV angle of 102° was the most appropriate. We have included the details of the measurements used in this quality check in the caption as recommended. Because the measurements used for the quality check correspond to the location of the circle and this information is relevant to the manuscript, the latitude and longitude of the center of the circles (cent ered at 13.3° N, 57.717° W) is now provided on L.92. Furthermore, we have revised the text on L.172 – 179 to state that "A quality check was performed to ensure the compatibility of BACARDI and GOES-16…This quality check is described in more detail in Appendix A".

We also found that FOV of the BACARDI footprint used in this study had been incorrectly reported as 51°. This value refers to only half of the FOV, but it should be 102° for the full FOV. This has been corrected throughout the manuscript.

L.349: Could you please spell out the possible competing mechanisms explicitly?

The text has been expanded on L.365 – 368 to include the following: "For example, because ΔF_{ter} is so strongly tied to z_{ct} at high cloud fractions, the effects of LWP could be masked by the combination of high and low clouds in the high cloud fraction class. Thus, it could be necessary to further categorize the data by cloud top height or other macrophysical characteristics before drawing conclusions about the effects of microphysical cloud properties.”

Technical Corrections:

L.84: in-situ

This correction can be found on L.88.

Fig 3: add the y-ticks in the rhs column of the figure to improve the readability.

This has been added to what is now Fig. 2.

Fig.9: the numbering [(b), (c), (d)] appears to be missing.

This has been added to what is now Fig. 8.

Responses to Referee #2

General Comments:

You use the satellite measurements to derive the cloud microphysical and macrophysical properties. But, as you mention, due to the coarse resolution the satellite measurements might miss small clouds. Could measurements from other instruments on HALO help to overcome that problem?

Measurements from other instruments on HALO are certainly useful for overcoming the coarse resolution issue of satellite measurements. However, the footprints of BACARDI and these other instruments (e.g. imagers) are quite different. The field of view of BACARDI is 154°, which at 10 km in flight altitude equals a footprint with a radius of 44 km, while the imagers have a field of view (FOV) spanning approximately 6 km across at a flight altitude of 10 km. Furthermore, even the airborne measurements of cloud fraction are prone to uncertainties. As shown by Konow et al. (2021), the different measurement techniques have different sensitivities to the small clouds and differ in cloud fraction by up to 30%. We are confident that we could combine BACARDI and the information from other airborne instruments, but further work is needed to ensure their compatibility in time and space before we can quantify the added benefits of higher resolution observations relative to the satellite-based approach that we use in this study. It is also important to keep in mind that we are not assessing the cloud radiative forcing of individual clouds, but looking at the larger scale picture that BACARDI observes. Nevertheless, as we state on L.410 – 412, “additional work is planned for a subsequent study including imaging remote sensing at a high spatial resolution (below 10 m), such as the VELOX thermal IR camera that was also present on board HALO during EUREC^{4}A”.

On 31 January measurements were associated with dust transport into the measurement area. Dust is known to have the ability to modify cloud properties at different ways. Thus, one would not expect to see differences in the retrieved results. However, on 28 January the retrieved properties are quite similar to the one on 31 January. What might affect the situation on 28 January (reduced cloud fraction and cloud height)?

Based only on the information in the flight reports (e.g. reports of dust or deeper convection) and the cloud property information from the satellites, it is difficult to come to concrete conclusions about what makes two days look similar or different. The fact that there were no reports of dust or deep convection on the 28 January suggests that there is some mechanism suppressing the vertical growth of the clouds on that day. This could also be a case where the coarse resolution of the satellite prevents a more accurate calculation of cloud fraction. To prevent readers from being misled by our suggestion of the role of dust, which cannot be assessed by the observations used here, the text has been revised at L.247 to say that "confirmation of this assertion would require further information and analysis".

Besides the effect of dust on cloud properties, dust layers are also expected to have a radiative effect. Might this affect your results?

It is possible that the results are affected by dust layers. The simulations used to calculate the cloud-free situation do not include aerosols. So it is possible, that the remaining cloudy signal also contains the signal from dust aerosol particles. The text has been revised at L.155 – 157 to say, "It should also be noted that aerosol particle properties were not included in the simulations, as their effect is anticipated to be minimal relative to clouds, but their influence may still be included in the cloud effects calculated here."

Some of the measurements were performed in similar cloud regimes. Would one expect similar results?

We hypothesize this to be the case, or at least that clouds from a similar regime would follow similar patterns, but further analysis would be required to determine if it is true. The critical point is to derive a quantitative measure characterizing the cloud regime, which is difficult to derive from observations. Comparisons with model studies, wherein the categorization or determination of different cloud regimes could be more straightforward, could overcome this issue but are outside of the focus of this manuscript. As stated on L.394 – 395, future studies plan to include a more thorough categorization of the data to look for such patterns because "it could be possible that the addition of other information or ways of categorizing the data could be the key to extracting even more clear conclusions".

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2021-812/acp-2021-812-AC1-supplement.pdf