Response to review comment 1

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Title: High spatial resolution retrieval of cloud droplet size distribution from polarized observations of the cloudbow
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Response to report #1

We thank Bastiaan van Diedenhoven for his comments and suggestions which we address in the following. The authors' answers are printed in italics, and with gray background.

Summary:

This is a review of the paper titled "High spatial resolution retrieval of cloud droplet size distribution from polarized observations of the cloudbow" submitted to AMT by Pörtge et al. The paper describes the measurements of polarimetric cameras which are part of the specMACS instrument deployed on the HALO aircraft. The data processing is described, as well as the application of polarimetric cloudbow retrievals of the cloud top droplet size distribution. A few case studies are described in detail.

The paper is generally well written. The data and the results are very interesting and show a lot of potential. I recommend publication of the paper after addressing some minor comments and questions listed below and some corrections to the text and figures.

General comments:

1. The data is corrected for the displacement of the clouds using ERA5 wind fields, as mentioned in section 3.2. As this correction seems not entirely trivial to me, I would suggest adding a bit more detail. For example, the ERA5 resolution is much coarser than the observations, so are the ERA5 fields interpolated to the observation locations? If not, I would expect strange effects at the ERA5 boundaries. (Please give the horizontal and vertical resolution of ERA5.) Also, how is the vertical variation of the wind field taken into account, as the cloud top height is not known yet at this step?

   →*Thanks for pointing this out. This is indeed an important information and we added the following text to the section:*

   "To correct for horizontal displacements of the cloud, the method was extended to include data of the horizontal wind from the ERA5 reanalysis dataset (Hersbach et al., 2020, 2018). The dataset has an hourly temporal resolution, a horizontal resolution of 0.25° × 0.25°, and 37 vertical levels from the surface to 1 hPa. During EUREC4A, clouds were typically observed at a vertical altitude of 1 km to 2 km where the ERA5 dataset has a vertical grid spacing of about 250 m. For testing purposes, we also used the ERA5 data on the original model levels with 137 vertical levels, and a higher vertical resolution (about 150 m at a vertical altitude of 1 km to 2

km) but this had no significant effect on the derived stereo heights. First, the stereo method is performed without additional wind information, and the 3-D coordinates of the identified pixels (stereo points) are retrieved. Then, the ERA5 data are interpolated to these coordinates to extract the corresponding wind data. The stereo method is performed again, but this time taking the wind data into account. The whole process is iteratively repeated 5 times, each time updating the wind data with the ERA5 wind interpolated to the heights and locations of the previously found stereo points. Further increasing the number of iterations did not notably change the results."

2. If I understand correctly, the cloud top heights are first determined for cloud (parts) that show prominent features and those results are then interpolated to the rest of the field. Again, this interpolation seems not entirely trivial to me and more details are needed. Is it just a simple 2D interpolation in lat/lon space to the nearest points?

→*The interpolation is now explained in more detail:*

"The cloud top heights from the single points of the stereographic method are interpolated to the entire image (Fig. 4 c). The interpolation process first consists of a linear interpolation of the stereo pixels onto all image pixels inside the convex hull of the stereo pixels. Then, the regions outside the convex hull of the original stereo points are filled by a nearest neighbor interpolation. The resulting cloud top heights are assigned to the selected cloud targets."

3. In section 4.1., the detection of large cloud drops are discussed. In line 325 it is stated that the detection of these large drops "is confirmed by high reflectivity values of the polarimetric Ka-band MIRA-35 cloud radar measurements". However, this radar is sensitive to rain drops that are much larger than the large cloud drops observed by specMACS. These cases likely have bi-modal size distributions, as discussed in the manuscript, but the large drops mode is not a rain mode, as suggested in line 333, but more likely drops in the 'size gap' as discussed in line 345. I think the discussion in the manuscript is a bit confusing now, somewhat suggesting the large drops are rain drops. I suggest rewriting the discussion to emphasize the distinction between rain and the large drops. Sinclair et al. (2021) also discussed the association between large cloud drops at cloud top observed with a polarimeter and rain detected by radar.

→*We addressed your comments by changing the discussion to:*

"The retrieved reff values of the higher cloud are very large. To better understand the cloud field and the large reff values, we looked at radar measurements of the polarimetric Ka-band MIRA-35 cloud radar of the HAMP instrument onboard HALO (Mech et al., 2014; Konow et al., 2021). The radar measurements from 16:47:00 to 16:48:30 UTC are shown in Fig. 8 along with a push-broom like image of the specMACS measurements and an indication of the HAMP radar field-of-view within the specMACS image. Within the high cloud from 16:47:00 to 16:48:15 the radar shows bands of enhanced reflectivity > 0 dBz and positive fall speeds (not shown). This likely corresponds to sedimenting droplets. Together with our observation of droplet sizes clearly larger than the usual cloud droplet size range (<15 µm) this points to drizzle development and we may see impacts of precipitation formation deeper in the cloud within the polarimetric signal originating from cloud top. Although our technique is not able to observe the precipitation droplet range (>100 µm) directly it is still sensitive to the

intermediate size range below a possible drizzle droplet mode. This case study is particularly interesting, as the retrieved reff lie within the size gap where neither the diffusional growth, nor growth by collision-coalescence is effective (Grabowski and Wang, 2013). The recent study by Sinclair et al. (2021) discussed the correlation between large cloud droplets detected by the RSP polarimeter and rain observed with a radar in great detail and found that the estimated cloud top precipitation rates are strongly correlated to radar derived precipitation rates and rainwater paths. For our polarimetric technique, it is necessary to make an assumption on the shape of the DSD. Currently, we use a monomodal gamma distribution for this purpose. In Alexandrov et al. (2012b) it was shown that for clouds with a bimodal DSD (e.g., due to drizzle), the polarimetric retrieval based on monomodal DSDs is biased towards the dominant mode. To overcome this problem, the rainbow fourier transformation (RFT) was developed, that retrieves the DSD without any assumptions on the number of modes of the distribution (Alexandrov et al., 2012b). When comparing the polarimetric technique to the traditional bi-spectral retrieval, it should be noted, that bi-spectral retrievals are (normally) also based on simulations with monomodal DSDs (Platnick et al., 2017) which tends to underestimate the true reff and has been investigated in several studies (e.g. Zinner et al., 2010; Zhang et al., 2012; Zhang, 2013)."

4. In figure 13 an effective radius "profile" is shown. It is probably good to point out the profile is not a profile of effective radius inside a cloud, but statistics of effective radius at different cloud top heights. Some more details of how this profile is obtained would be good. For the interpretation, you may want to refer to Rosenfeld and Lensky (1998). https://journals.ametsoc.org/view/journals/bams/79/11/1520-0477_1998_079_2457_sbiipf_2_0_co_2.xml

→Thanks for pointing that out. We added an explanation of how we derive the vertical "profile" and referred to Rosenfeld and Lensky (1998) for the interpretation:

"This dependence of the reff on the cloud top height is presented in more detail in Figure 13. Here, the derived reff of all individual clouds of the case study are plotted against the corresponding cloud top heights (as in Rosenfeld and Lensky (1998)). We refer to this plot as a vertical profile, although it does not show the actual reff profile within a single cloud. The idea is that the individual clouds of the cloud field are captured at different stages of their vertical growth. It is then assumed that the retrieved reff which is sampled at the cloud top is representative of the actual reff at the same height inside a single cloud. This assumption applies only to non-precipitating clouds. By combining the measurements of the individual clouds at different stages of their vertical development, a vertical profile is constructed, which is assumed to correspond to the vertical profile of a single growing cloud. The figure shows a strong increase of reff from about 5 µm at a height of 800 m to 9 µm at 1350 m. This rapid growth of droplets with height may indicate the dominance of growth through coalescence and is typical for maritime clouds. Rosenfeld and Lensky (1998) refer to this zone as the "droplet coalescence growth zone".

5. In line 431-439 some reasons for the detection of large effective variance values, mainly at the cloud edges. It is suggested that errors in the aggregation of angles may have caused the

cloudbow signal to be distorted. But would these cases not be filtered out by the RMSE and Qual requirements?

→*We again checked the targets with large veff. We think that there are some targets that show such large veff values which is why we do not want to rigorously filter out all veff > 0.32 as suggested by Reviewer #2.*

a) *Some signals actually came from ocean targets. In this case, the structure of the aggregated signal is relatively linear and the fit function can be fitted perfectly to the signal by keeping the parameter A (which scales the phase function) in the fitting function small. The fit has a very small RMSE, and as Qual ~ 1/RMSE this might also lead to a relatively high Qual index (at least > 2 as our original Qual filter threshold) and the signal is not filtered out.*

b) *Other signals (especially at cloud edges) did probably suffer from errors in the aggregation of angles. These did show a cloudbow signature, but the signal was not very clear, which could result from a mixture with ocean measurements. We increased the Qual filter threshold from 2 to 4. This did also filter out some targets, that are located quite central within the clouds, but where the cloud showed a lot of variability (shadows) and where an error in the aggregation of angles also could have a large effect.*

c) *There were some targets with veff = 0.32 which were located quite central within clouds (especially in case study 1) where the cloud did not show a lot of variability. The signals looked good and we think that these targets do have a veff of 0.32 or even larger. Such targets were not filtered out by the new Qual threshold of 4 and we decided to keep them in our results.*

Minor corrections:

- Line 24: These two sentences basically say the same thing twice. I suggest to remove the first sentence and move the IPCC reference to the second.

→*Changed as suggested*

- Line 53: Add "using the bi-spectral technique " after "is not possible".

→*Changed to:*

"Furthermore, the bi-spectral technique does not provide information about veff (Nakajima and King, 1990)."

- Line 72: this should be "singly-scattered photons".

→*Changed as suggested*

- Line 75: I suggest changing the order of words into "which is a parameter that is directly linked to entrainment and mixing processes."

→*Changed to:*

“Furthermore, the veff of the DSD is derived in the polarimetric retrieval. This parameter may be directly linked to entrainment and mixing processes at the cloud top.”

- Line 80: An example of cloudbow retrieval applied to airMSPI is given by Xu et al. https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2017JD027926

→*We added Xu et al. 2018 as a reference.*

- Line 99: Please place a hyphen between ‘bias’ and ‘adjusted’

→*Changed as suggested*

- Line 205: I think “from” should be “of” in this sentence.

→*Changed as suggested*

- Line 235: “the radiance measurement is binned”. Do you mean Stokes parameter Q?

→*Yes, we mean the Stokes parameter Q as in the previous sentence. To make it clear that the two sentences belong together, we have changed them as follows:*

"The individual measurements of the same target of the Stokes parameter Q are combined to generate the aggregated polarized radiance measurement. For further processing, the aggregated measurement is binned onto a scattering angle grid with a step size of 0.3°."

- Line 240: “The P12 element is also called the polarized (single scattering) phase function and is a good approximation for the measured polarized radiance Q”. The absolute value of P12 itself is not a good approximation of measured Q, but the relative variation is. Also, it is not Q, but Q rotated to the scattering plane. Please rewrite this sentence accordingly.

→*We changed the sentence to:*

“The P12 element is also called the polarized phase function and is approximately proportional to the measured polarized radiance Q in the scattering plane (Bréon and Goloub, 1998).”

- Line 245: The effective variance also determines the width of the secondary minima.

→*Thank you for pointing this out, we added "and widths" to the sentence:*

"The veff, however, determines the amplitude and widths of secondary minima of the radiance distribution but has only a small effect on the position of the minima."

- Line 294: Please place Eq. 8 here in the sentence.

→*We added the reference to Eq. 8 to the first sentence and removed it from the second:*

"As a second quality measure, we calculate the quality index “Qual” as in Equation 8 (first defined by Bréon and Doutriaux-Boucher (2005)). This is the ratio between the variability of the measurement, which corresponds to the squared amplitude of the cloudbow (A·P 12), and the RMSE of the fit."

- Line 342: Please remove the comma after ‘statement’

→*We restructured the whole paragraph (see specific comment 3).*

- Figure 7 and 10: Please add some information about the (approximate?) scale to the figures. Either a scale bar or x and y axis labels.

→*Changed as suggested and added explanatory text, on why the image is distorted.*

"Fig. 7 g) shows the RGB image of the measurement from the polB camera. The labels on the four sides of the image indicate the distances between the neighboring corners of the image. It is noticeable that the side lengths of the top (14.44 km) and bottom (27.02 km) differ greatly. This happens, because the camera is installed at a slight angle in the across-track direction and therefore, the lower part of the image covers a much larger distance in the along-track direction. This is also the case for the measurements of the polA camera, but here, the upper part of the image covers a larger distance."

- Line 378: I suggest adding Zhang et al. 2012 as an additional reference. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2012JD017655

→*We added Zhang et al. 2012 as a reference.*

- Line 453: I suggest adding "to interpret" after "more difficult".

→*Changed as suggested*

- Line 475: A period is missing after "0.07".

→*Changed as suggested*