Reply to Reviewer #2:

We thank the reviewer for the time and efforts she/he spent reading our manuscript and providing valuable suggestions and advices. Please find below a discussion of the reviewer's comments (italic). Changes/additions made to the text are underlined and given in quotes.
The general comments made by the reviewer summarize the main points of the specific comments and suggestions. Therefore, we will start with our replies on the specific and sequential comments.

p3,L5: I don't see the relevance of the cited paper (Cahalan, 1994) and the associated science (plane-parallel retrieval assumptions) in this context.

Since the profile retrieval of the cloud phase from MODIS as applied later is based on the cloud particle size retrieval, we included here also the limitations of the size retrieval with respect to the bias caused by 1D assumptions. However, we removed parts of the text and added the following:

From the ensemble of retrieved effective droplet sizes, a vertical profile of cloud phase can be estimated because of the relationship between cloud phase and vertical profile of the cloud particle size (Rosenfeld and Feingold, 2003; Yuan et al., 2010; Martins et al., 2011). However, the retrieval of the effective droplet size relies on one-dimensional (1D) radiative transfer simulations, which incorporates retrieval uncertainties due to plane-parallel cloud assumptions and neglecting the net horizontal radiative transport between the satellite pixels (Zinner et al., 2006). Consequently, a decrease of pixel size causes an increase of the independent pixel bias, because the smaller the pixel, the more important is the net horizontal photon transport, particularly for the wavelengths in the visible spectral range, which are used for the retrieval of the effective droplet radius.

p3,L17+L30: These are places where the manuscript could outline how the specific work fits into the larger context that was set up previously. Currently, this page in particular looks like a list of work done by the authors and predecessors (e.g., Martins, Rosenfeld), with only tangential connection to the motivation from the previous page(s). This is not a deal breaker for the manuscript, but it would be better to see how the listed work serves a number of outstanding questions related to the introductory comments earlier on.

Starting from p2l25 a technical and experimental review is given how profiles of the cloud microphysical parameters can be derived. This review is mainly focused on passive remote sensing approaches, either from satellite observations (ensemble method), or aircraft observations but also from ground-based measurements. Data products based on these approaches will be used in this study. Therefore, it is worthwhile to describe them shortly here. Most of the cited literature is related to a technical description of the individual retrieval approaches with no or only less discussion on e.g., aerosol-cloud interaction.
To strengthened the connection between motivation and the presented phase retrieval we added the core questions at the end of the introductions:

In this paper we will address the following questions: (i) Can we observe differences in the vertical distribution of the thermodynamic phase state in DCCs for different aerosol conditions by using cloud side observations? (ii) How do the vertical profiles of cloud phase derived from cloud side observations agree with results from satellite (ensemble method) and in situ measurements?

p4,L4: In the description of the manuscript's structure, it seems that the goal is to compare remote sensing derived cloud profiles to MODIS and in-situ data, which would make the manuscript more appropriate for AMT than for ACP. If, however, there are some higher-level goals that address some of the questions brought up above, this should be made clearer.

The manuscript was submitted to be published in a special issue of AMT/ACP presenting results from the ACRIDICON-CHUVA campaign. Admittedly, parts of the manuscript include the description of the applied method, but compared to former publications dealing with cloud side observations, this manuscript presents the results of an entire campaign. We are aware, that the number of cases is limited. From the 14 scientific flights we selected the three days (AC10, AC13, and AC18) with the best conditions as stated in the beginning of Sect. 4:

(i) no cloud layer above the observed cloud (no cirrus), which contaminates the spectral signature,
(ii) high proportion of illuminated cloud parts in the vertical direction of the cloud,
(iii) flight altitude that allows measurements of an extended vertical region of the cloud considering the limited FOV of specMACS, and
(iv) isolated clouds with recognizable structures for cloud geometry retrievals.

This limits the number of cases. Similar limitations are also reported for the in situ data sampling as shown in Costa et al., (2017). They had data from cloud passages lasting between 1 and 18 minutes in sum per flight.

We adapted the outline of the paper to point out that the retrieval method is applied to different cloud cases under different aerosol conditions and discussed using also other observation strategies.

<u>The variability of vertical phase distribution is discussed with respect to aerosol conditions and compared to in situ and MODIS products.</u>

p4,section 2.1: A table with flights and clouds cases would help.

We added a table summarizing the three flights which are presented in this work.

<u>A summary of the three flights used in this work is given in Table 1.</u>

Table 1. Summary of presented flights with cloud side observations during the ACRIDICON–CHUVA campaign. The ranges of flight altitude and time refer to the studied cloud cases.

Flight number	AC10	AC13	AC18
Aerosol conditions	moderate	polluted	moderate
AOD (MODIS)	0.4 - 0.5	0.5 - 0.6	0.3 - 0.4
Number of cloud cases	9	16	10
Flight altitude range (km)	7.4 - 10.4	5.2 - 9.3	1.4 - 14.0
Time range (UTC)	17:25 - 19:20	17:55 - 19:00	15:30 - 20:30

p5,L12-18: The manuscript should elaborate on the stereo algorithm a little more. Also, L21, should this be "assign" instead of "allocate"?

Please read our response to the commentary on p.10 dealing with the stereo algorithm. Furthermore we changed the sentence as suggested:

<u>This allows assigning elevation and azimuthal angle to each point of the image.</u>

p6,L23: The description of the aspherical fraction is a bit unclear; what is measured, and what is derived?

The aspherical fraction from the CAS-DPOL is determined by measuring the perpendicularly polarized light in the backward direction and the forward scattering light intensity. While the forward scattered light intensity is used to determine the size of the particle, the ratio of the forward and the backward scatter light determines the phase of the particle. While spherical particles do no change the polarization ratio, aspherical particles do. In order to categorize into liquid and ice particles, a size dependent threshold was inferred from calibration measurements of spherical liquid particles in the AIDA cloud chamber (Järvinen et al., 2016, Schnaiter et al., 2016). Particles with a polarization ratio larger than the 1- sigma range of the inferred sphericity-threshold were categorized as aspherical. The method gives a size dependent aspherical fraction of the first 300 particles measured each second. The bulk aspherical fraction was derived from the number of aspherical particles to the number of total particles measured between 3 and 50 μm per second.

We condensed it, because the CAS-DPOL principle is similar to that of the NIXE-CAS:

<u>The aspherical fraction (AF) from the CAS-DPOL is determined by measuring the perpendicularly polarized light in the backward direction and the forward scattering light intensity. The ratio of the forward and the backward scattered light determines the phase of the particle. Particles with a polarization ratio larger than the 1-sigma range of the inferred sphericity-threshold are categorized as aspherical. The method gives a size dependent aspherical fraction of the first 300 particles measured each second. The bulk aspherical fraction is derived from the number of aspherical particles to the number of total particles measured between 3 and 50 μm per second.</u>

p6,L30: Why is the threshold this high? 0.3 g/m3 seems excessive, considering that a typical BL cloud top-level LWC is 1 g/m3.

We added the following:

<u>The Hotwire sometimes returns a signal in ice or clouds of partly frozen particles. This signal is on the order of 0.2 g m^{-3}. Thus a conservative threshold of 0.3 g m^{-3} is used to reduce the false alarm rate.</u>

p7,l1: Why is it necessary to perform 3D calculations? Only because of the geometry, or because deviations from standard 1D models are expected? If so, what are they (aside from shadows)

A convenient way to simulate cloud side reflections is to use 3D radiative transfer models, where the geometry of the observation strategy can be directly transferred to the model setup. There are ways to use 1D radiative transfer simulations instead by adapting the viewing and zenith angle. But this underlies restrictions, because no horizontal transport can be considered. This was shortly discussed in Marshak et al. (2006) and Martins et al. (2011). With respect to 3D radiative effects, less impact due to shorter photon paths is observed for absorbing wavelengths as were used for the phase retrieval. However, at cloud edges with lower optical thickness (tau < 30 after Martins et al., 2011) the cloud reflection at cloud particle absorbing wavelengths is still variable, but gets saturated starting from tau = 40 (Marshak et al., 2006). Summarized, with some limitations the phase indices could be also derived by plane parallel simulations. But in any case considering cloud shadow effects (as in section cloud masking procedure) 3D simulations were necessary.

p7,l6: Rayleigh scattering → molecular scattering?

We use the term "Rayleigh scattering" (as used in Bodhaine et al., 1999) because it describes the scattering on atoms and molecules.

p9: Here it becomes quite difficult to understand what the authors are after - the position/width of the mixed layer? Is that the purpose of the simulations? Are they done to only replicate the earlier study, or are they something new?

We hopefully introduced the purpose of the simulations better now:

<u>In the following, results from radiative transfer simulations using MCARATS are presented. The viewing geometry and the atmospheric description were adapted to the conditions during ACRIDICON-CHUVA on 19 September 2014. These simulations were performed to demonstrate that ice and liquid water phase can be separated from the transition layer under different conditions similar to the results reported by Jäkel et al. (2013). Note, that due to the different viewing geometry, also a different angular range of the scattering phase function was observed than for ground-based measurements. This might have an effect on the characteristics of phase index profile in particular with respect to separation of the mixed phase layer.</u>

p10: The geometry retrieval description is rather cryptic. Would it help to cite work related to MISR, or is the method unrelated? What happens if the cloud moves during two consecutive images (used for the stereo method)? Also, elaborate on p10,l18-19, and state with respect to which coordinate system the "elevation angle" is provided.

We omitted a detailed description of the method because it includes a lot of equations which are given elsewhere. But for better understanding we referred to a publication which discussed the mathematics for a similar experimental setup (Biter et a., 1983):

<u>The theoretical background on photogrammetry is given in Hartley and Zisserman (2004), while Hu et al. (2009) applied these techniques for cloud geometrical reconstruction. The mathematics for the geometry retrieval, as it is used in this study, is based mainly on the method described by Biter et al. (1983). They deployed a side-looking camera onboard of an aircraft to detect the position of cloud features, similar to the setup presented in this work.</u>

And later:

<u>After coordinate transformation, trigonometric methods (Biter et al., 1983) are applied to calculate the distance between the camera positions P1 and P2 to the observed point C.</u>

We added also a short comment on the meaning of the "elevation angle":

<u>Repeating this procedure for a number of points yields a relation between elevation angle and cloud height. Note, that the elevation angle represents the elevation angle of the selected tie point of the camera image after correction based on the aircraft attitude data. It basically gives the elevation angle above or below the flight altitude.</u>

We are aware that cloud movement might introduce an additional uncertainty. Therefore, we tried to reduce the time between two consecutive pictures. The GoPro delivered a movie, such that images from different time intervals can be selected for one cloud scene. All evaluated cloud scenes used data from time intervals less than 10 seconds.

The same tie points are chosen in a second image taken about 10 seconds later. Choosing a short time interval helps to reduce the uncertainty of the method induced by cloud movement.

Note, that another publication is in preparation for AMT which will discuss the 3D construction of clouds based on photogrammetry and O2-A band absorption (Zinner et al.).

p10: Others circumvented the whole (rather difficult) stereo algorithm by including an IR channel. Why was that not done here? Were these measurements simply not available? And why did the authors prefer the more complicated method to the simple IR imager?

The reviewer makes a good point here to bring up the usage of an IR camera. Unfortunately, we had no IR-camera available for this campaign. But another ground-based campaign is scheduled for September/October in 2017 in the Brazilian rainforest using IR-camera and imaging spectrometers together for cloud side observations.

p11, Applications section. What is it that the paper seeks to find out? Refer to the main question here, at least at the beginning? This whole section reads a little bit like a listing of results with no specific purpose. Quite surely there is one, and that should be clarified more.

We reordered the beginning of this section a little bit and introduced this sections with the two main questions of the case study:

From the 14 scientific flights three days (AC10, AC13, and AC18) are selected with the best observation conditions for specMACS, namely: (i) no cloud layer above the observed cloud (no cirrus), which contaminates the spectral signature, (ii) high proportion of illuminated cloud parts in the vertical direction of the cloud, (iii) flight altitude that allows measurements of an extended vertical region of the cloud considering the limited FOV of specMACS, and (iv) isolated clouds with recognizable structures for cloud geometry retrievals.
Phase profiles from AC13 representing polluted aerosol conditions will be compared to the two days with less aerosol pollution. Effects of aerosol conditions on the height and thickness of the mixed phase layer will be investigated. Second, it will be demonstrated how comparable the different observation strategies (cloud side, cloud top and in situ) are.

We restructured the subsections a little bit to separate the two major goals of this sections. At the end of Sect. 4.1 we discussed the comparability of the different observation strategies, while at the end of Sect. 4.2 the aerosol impact on the mixed phase layer is summarized.

Sec. 4.1: see comment referring to *p13,L12-14*

Sec. 4.2:
From theory, the mixed phase layer is expected to be higher for polluted aerosol conditions than for cleaner aerosol conditions, which can partly be confirmed by comparison of the three cases. We found from cloud side observations, that the lower boundary altitude of the mixed phase layer tends to be higher for polluted conditions (AC13: 6.0 - 6.5 km) than for the moderate case of AC18 (5.6±0.2 km), while the upper boundary is shifted from 6.8±0.2 km (moderate case AC10) to 7.4±0.4 km (polluted case AC13).

p12: It is a bit unclear what the in-situ measurements really have to offer here if “the direct comparison of in situ and remote measurements is difficult”. Really, the in-situ measurements *should serve as validation for the remote sensing, but what do we learn if that doesn't work? Is it still worth using the in-situ data? If the comparison does not work out, what does that mean for the initial hypotheses (if there is one: perhaps a question about the interchangeability of pixel/time mentioned earlier?)*

When introducing the in situ measurements we added the following sentences to specify the ideas behind the comparison of the different observation strategies:

<u>The variability of the mixed phase layer in depth and height within a single cloud cluster shows that the vertical distribution at least at the cloud edges is variable. In situ data are used to investigate if such a variability is also observed in the more inner part of the cloud.</u>

*p13: The in-situ data puts ice higher than remote sensing. Which is right? What do we learn here about the representativeness of satellite data and *its* consistency with the aircraft measurements?*

We didn't find much differences between the glaciation heights derived from the ensemble method based on particles sizes from MODIS (9 km) and the estimations from the CAS-DPOL (8.7 km). Both are using a larger sample of data which were averaged over the entire cloud cluster. While the time series of the NIXE-CAPS instrument shows for individual cloud passages a glaciation height of 8.0 km which is in much better agreement with the cloud side observations. An assessment about the comparability is given at the end of this subsection (please see comment p13,L12-14).

*p13,L34 (top): So what phase *does* MODIS get for 6km? The shown results are certainly not liquid drops, given the large size.*

It's a good point made by the reviewer. We assume that this second peak might indicate the bottom of the mixed phase layer. We added the following:

<u>From the conceptual model of cloud particle size profiles inside a DCC (e.g., Rosenfeld and Woodley, 2003) it might indicate the bottom of the mixed phase layer, when cloud particle size starts to increase. However, this increase is less pronounced than presented in Rosenfeld and Woodley (2003).</u>

p13,L12-14: Here we get some potentially important conclusions, which should be expanded an elaborated on. What is the significance of this finding? What can the satellite-based ensemble method do, and what not? Do in-situ and remote sensing observations from the aircraft tell two different stories?

It's hard to give a general conclusion on the validity of the ensemble method due to the limitations of studied cases. For such a statement another study would be required using data from several measurement campaigns comparing in situ with satellite observations. However, for our data set we can conclude the following:

<u>Comparing the glaciation height from MODIS with NIXE-CAPS in situ data and results from specMACS observations shows a deviation of about 1.0 - 1.5 km between the different retrieval techniques and observation strategies. However, the mean profile over the entire cloud cluster derived from CAS-DPOL measurements exhibited a similar glaciation height (of about 8.7 km) as found from the MODIS data. This shows that the satellite-based ensemble method may be representative for a large cloud field.</u>

But for individual clouds NIXE-CAPS and specMACS measurements have shown lower glaciation heights. The most likely reason is related to the fact that the ensemble method relies on cloud top observations of growing clouds in different stages of evolution. As shown in Fig. 9g mainly particle sizes between 22 and 27 µm were derived indicating that profile is dominated by cloud measurements in mature stage. At this stage the particle phase may be altered by up- and downdrafts within the clouds as was shown in Fig. 9e. This leads to an enhanced horizontal variability of the cloud phase state which cannot be resolved by passive remote sensing from cloud top observations. Another, but minor reason of the discrepancy between ensemble method and NIXE-CAPS / specMACS measurements is related to the retrieval uncertainty of the effective cloud particle radius. While scattering properties are well defined for liquid water particles, they are variable for ice particles due to differing habits and crystal shapes (Eichler et al., 2009). This gets even more complicated for cloud tops where phase transition starts. Additional retrieval uncertainties of the particle size directly contribute to the derived profile of r_{eff}.

p14,l5: Does this explain the discrepancy between in-situ obs and remote sensing?

We added the following:

Also local strong downdrafts can transport ice particles into lower levels, which will be interpreted as mixed phase layer from the cloud side observation perspective. Due to the horizontal variability of cloud phase inside a cloud cluster for example caused by up- and downdrafts, in situ measurements may only reveal liquid phase particles. A direct comparison between the observation strategies is subject to restrictions because of temporal and spatial variability of cloud properties in convective systems.

*p14,l16/17: distinctive *change* in gradient, or simply "significant gradient" (change of gradient is a gradient of a gradient ...)*

Changed as suggested.

p14,L21: Here the question is again why this was chosen over IR imaging.

As mentioned already, an IR imager was not available during the campaign.

p14,L31: Earlier, the authors said that the aircraft measurements are not statistically significant to prove/disprove theory. This statement here is not meant to be the main finding of the manuscript, is it? Wouldn't satellite data be more suitable to put this on a statistical basis? If so, what would then be the purpose of the aircraft measurements? This may be obvious, but it would help the reader to understand this point.

In this part of the conclusions we summarized the findings of the comparison between the three flights (AC10, AC13 and AC18) with respect to aerosol impact on the mixed phase layer height. Conclusions about the comparability is given later (see reply to comment p15,L13).

For moderate aerosol conditions, only few cases exhibited liquid water, mixed phase, and ice phase, which limited the statistical significance of the comparison with AC13. However, comparing the glaciation heights of AC10 (6.8±0.2 km) and AC13 (7.4±0.4 km) we found an indication of an increase of glaciation height and a decrease of glaciation temperature for polluted aerosol conditions. With respect to the occurrence of first ice particles, the lower boundary of the mixed phase layer was derived with 6.0 - 6.5 km for polluted conditions, whereas for AC18 the altitude was shifted down to 5.5 - 6.0 km, which agrees with theory.

p15,L38(top): The results from remote sensing and in-situ are not really consistent (as discussed earlier by the authors, and noted by the reviewer).

This sentence was referring to the comparison between NIXE-CAPS and specMACS measurements. We changed the sentence for clarification:

<u>Consistent results of mixed phase zone levels were found from specMACS and NIXE-CAPS measurements, for the flight AC13 with most individual cloud cases showing pure liquid, mixed phase layer and pure ice phase.</u>

p15,l3: "invariance of space and time" does not seem an appropriate way to describe the assumptions of the ensemble method. It's really spatial statistics vs. temporal evolution, isn't it? Secondly, the manuscript now divulges that it did seek to study the aerosol effect on deep convection - or is this a statement that this was done (by others) using MODIS? Clarification is needed here what was done in the manuscript vs. prior work. Is it fair to say that the manuscript got closer to observational evidence for the validity of the interchangeability of spatial statistics and temporal evolution?

The reviewer is right when describing the ensemble method assumptions by spatial statistics vs. temporal evolution. But we will use here the term "time-space-exchangeability" as it was named in several other publications (e.g., Lensky & Rosenfeld, 2006, Yuan et al., 2010). The main intention of this is short paragraph was to summarize the results of the ensemble method. It was not intended to study aerosol effects on DCCs on the basis of cloud top observations. We rephrased the first sentence for clarification as follows:

<u>Additionally to in situ and cloud side measurements, the glaciation temperature was derived applying an ensemble method based on MODIS data, which assumes time-space-exchangeability for a cluster of clouds with different states of evolution.</u>

p15,L13: This seems like a fair statement, but how do we interpret it? For which purposes is the satellite-based method good enough, and for which problems do we need to use airborne or tower-based observations (as later suggested by the authors)?

We made a final conclusion on this as follows:

<u>It is concluded that the assumed time--space--exchangeability used in the ensemble method can give a simplified picture of the vertical distribution of the phase within a field of convective clouds of different stages of evolution. Particularly, cloud tops where phase transition (from liquid to ice) starts and ends needs to be observed by the satellite to profile the thermodynamic phase. The number of these observations has to be significant, since the particle sizes are averaged over a larger domain. So, in general the ensemble method can give an indication when phase transition arises for the first time. However, for estimation of the cloud phase profile at a later stage of the DCC evolution, in situ and also cloud side remote sensing might be the better observation strategy, when phase distribution is altered for example by up- and downdrafts.</u>

p2,L17: 1) Why is there a new paragraph? 2) Suggest re-wording "In particular ... " as "The phase transition ... is especially relevant for ... " without a preceding indent/new paragraph.

Changed as suggested:

<u>The phase transition from liquid water to ice is especially is relevant for the development of precipitation.</u>

p2,L25: remains > remain

Done.

P3,L30: “In further development of the scanning ... ” Something wrong with the language here and the conclusion of this sentence.

Changed as follows:

Different from the scanning-point-sensor measurements as presented by Martins et al. (2011), this paper introduces airborne measurements of an imaging spectroradiometer called specMACS (spectrometer of the Munich Aerosol Cloud Scanner, Ewald et al., 2016). These observations were used to derive vertical profiles of the phase state of DCCs during the HALO (High Altitude and Long Range Research Aircraft) campaign ACRIDICON (Aerosol, Cloud, Precipitation, and Radiation Interactions and Dynamics of Convective Cloud Systems) - CHUVA (Cloud processes of tHe main precipitation systems in Brazil: A contribUtion to cloud resolVing modeling and to the GPM (GlobAl Precipitation Measurement)) in 2014 (Wendisch et al., 2016).

p4,L12: add comma after “September)”

Done.

p4,L21: The “degree” characters should be superscripts.

Done.

p5,L7: “by measuring monochromatic radiation from a monochromator” - revise language?

Changed as follows:

The spectral characteristics were deduced by using monochromator output at selected wavelengths.

p8,l17: “inlay” > “inset”?

Done.

p9,l31 (top of page) “ grid cell in” > “grid cell at”

Done.

p9,l2: ranging between > from ... to?

Changed as suggested.

p9,l3: What is the “first cloud case”? At this point, the table suggested above would really be helpful.

The first and the second cloud case are not related with the measured cloud cases which will be presented later in the manuscript. To omit confusion, we called the two cases in this section “cloud scenarios” and modified the introduction of the two scenarios as follows:

Two simplified cloud scenarios with different profiles of cloud effective radius and water content are assumed. In both cases the clouds ranged from 4.0 to 11.0 km altitude with a mixed phase layer between 6.4 and 7.0 km. While the first scenario uses constant values of cloud effective radius (r_{eff} = 20 µm for liquid water and ice) and water content (0.7 gm^{-3}), the second scenario assumes variable profiles of the microphysical parameters. These two cases are chosen to identify effects on the I_P-profile caused by changes of (i) the phase state itself (scenario 1), and (ii) the cloud particle size and water content (scenario 2).

p9,l6: “originated” > “originating”?

Done.

p9,l8: The phase index is significantly shifted to positive values > either it assumes positive values or not - what is the meaning of “significantly shifted to positive values” How about “shifted to positive values”?

We rephrased the sentences as follows:

In the mixed phase layer the phase index shows a steep increase to values larger than 0.15. The absolute difference of the phase indices between mixed phase layer and pure ice phase layer is less pronounced than between liquid and mixed phase layer.

p9,l9: Why “obviously”? Perhaps “apparently”? Meaning unclear.

We modified it:

This might be caused by the fact that the contribution of ice particles within the mixed phase layer leads to an increased absorption of radiation resulting in an increase of the phase index.

p9,l14: “related” > “relative”?

Changed.

p9,l15: move “also” to after “is”

Done.

p9,l20-21: Too hard to understand. Try to improve language.

Changed as follows:

As concluded in Jäkel et al. (2013), the phase index becomes less variable for a water content of more than 0.4 g m^{-3} (variation lower than 7 %). This holds true for most of the DCCs when cloud edges are excluded, which are optically thinner than the inner regions of the cloud.

p9,l22: “as can be concluded” > how can this be concluded?

We combined the two sentences as follows:

Less impact is attributed to the change of the sensor elevation angle, since the variability of the phase index with respect to the viewing geometry for each phase state in the first cloud scenario with fixed cloud microphysics is lower than the variability of I_P due to the changed cloud properties in the second cloud scenario.

p10,l1: "showing" > "with"

Done.

p10,l2: "need to be taken" > move to right after "images" on l1.

Changed as follows:

To estimate the distance to the observed cloud element (C) two images from different positions (P1 and P2) with a projection of the observed point in both images need to be taken (C1 and C2, so-called tie points) as illustrated in Fig. 5a

p10,l2: explain "epipolar plane"

For easier understanding we removed the two sentences mentioning the "epipolar plane". For the following equations this term is not needed.

p10,l3: What's the "world" coordinate system?

A world coordinate system is independent of the camera and aircraft coordinate systems. In fact, the spatial location of the camera/aircraft is given in the world reference system (world coordinate system) with :"The x -and y-axis of the world coordinate system (not shown) are pointed to the east and to the north, respectively, while the z-axis is perpendicular to the x-y plane (pointing upward)." as was stated in the manuscript.

We changed the sentence as follows and added another reference:

The geometric problem comprises three coordinate systems: for the camera, the aircraft, and the world coordinate system (longitude, latitude and altitude) for the observed point C (Biter et al., 1983).

p10,l6: Usage of the word "exemplarily" seems out of place throughout most of the manuscript. How about "for example"?

Changed as suggested:

For example, a positive pitch angle of the aircraft …

Here "exemplarily" was removed from the sentence:

The theoretical background on photogrammetry is given in Hartley and Zisserman (2004), while Hu et al. (2009) applied these techniques for cloud geometrical reconstruction.

In Section 3.1 we changed the sentence as follows:

The procedure is applied for an example cloud scene observed during ACRIDICON-CHUVA from 19 September 2014.

In Section 4.1 we exchanged "exemplarily" by "for example":

For example, a closer look at the asphericity is taken for the time range between 18.28 and 18.34 UTC (Fig. 9e).

p10,l6-7: Unclear what this means - wrong axis perhaps?

The sentence is extended as follows:

For example, a positive pitch angle of the aircraft (associated with rotation around the aircraft y_a-axis) rotates the camera (image) around the camera’s x_i-axis as can be deduced from Fig. 5b.

p11,L25: 6b: are the distances in km?

We added the unit in the text:

From stereographic analysis of these tie points the distances to the cloud points (in km) are determined (Fig. 6b).

p11,L27: “quite *a* homogeneous” (add “a”)

Done.

p11,l14: “scientific” > “science”

Done.

p11,l16: “which” > “because it” ?

Changed as suggested.

p12,L30 (top): “have been” > “were”

Done.

p12,l32 (top): “phase states. Mainly” > “phase states, mainly”

Done.

p15,L15: ATTO - introduce acronym somewhere.

The Amazon Tall Tower Observatory – was already introduced in Section 2.1 Field campaign.