We thank the reviewer for helpful comments which improved the manuscript significantly. Especially, by adding more explanations the revised manuscript will be easier to understand for the reader. The detailed replies on the reviewers comments are given below and structured as follows. Reviewer comments have bold letters, are labeled, and listed always in the beginning of each answer followed by the author's comments including (if necessary) revised parts of the paper. The revised parts of the paper are written in quotation marks and italic letters.

1. **However, in addition to the comments and suggestions listed below, my main concern with the paper is the lack of a clear statement on what new we were supposed to learn at each step of both (the one-point and the two-points) analyses provided in the paper. What is the main message the authors want us to take home after reading it? I got a feeling that the paper is much more descriptive than conclusive. I'd like to see a list of bullets/statements, at least, in the 'Summary and Conclusion' section.**

The reviewer is right. So far we had been too focused on the feasibility of the study and missed to point out the conclusions of our analysis clearly. We did not use a list of bullets, but we revised the conclusion part to make the most important results more clear.

"[…] Furthermore, the results from the 2D analysis showed that for the observed cloud cases the subtropical cirrus was more homogeneous than the Arctic stratus. This result was not available from the investigation of the commonly used 1D inhomogeneity parameters. Therefore, using 2D methods in future studies for the characterization of cloud inhomogeneities is advisable, since their information content exceeds the information content of the commonly used 1D inhomogeneity parameters. Nowadays, 2D images of cloud fields are widespread by e.g., measurements of all-sky cameras or satellite observation with high spatial resolution. Applying the presented methods to such continuous measurements would provide detailed views into the climatology of cloud inhomogeneities. […]"

"[…] We found such differences for more than the half of the observed cloud scenes. Therefore, the directional structure of cloud inhomogeneities should be taken into account, when cloud inhomogeneities are characterized. Clearly some clouds will have less directional dependence of cloud inhomogeneity, but this does not mean it is not useful to measure it. It is expected that the information content derived from the directional analysis of cloud inhomogeneities can clearly improve sub-grid scale parametrizations in weather and climate models. For this, depending on the application, the decorrelation length (size and structure of cloud inhomogeneities) or the scale breaks (horizontal photon transport, 3D radiative effects) may provide better proxies compared to commonly used 1D inhomogeneity parameters.

However, so far only two cloud types were investigated. To build up a better idea on cloud inhomogeneity of different cloud types, more high definition observations of cloud fields are needed. Beside dedicated field campaigns, continuous observations by all-sky cameras or satellites with high spatial resolution such as LandSat (15-90 m resolution) or ASTER (Advanced Spaceborne Thermal Emission and Reflection Radiometer, 15-90 m resolution) may provide the required data.

The 1D and 2D autocorrelation functions and Fourier analysis in conjunction with the derived decorrelation length and scale breaks are a helpful tool to verify cloud resolving models in terms of typical horizontal cloud geometries."

2. **A) The list of references is very rich but, as always, is incomplete. I would definitely add two more very relevant papers. The first one is**

Davis, A., Marshak, A., Gerber, H., and Wiscombe, W., 1999: Horizontal structure of marine boundary-layer clouds from cm– to km–scales. J. Geophys. Res. 104, 6123-6144.

In this paper the authors discuss the structure of marine stratocumulus clouds down to 4-cm scale using both spectral and structure function analyses.

We added the reference at the point where we are talking about the small-scale break. Please check comment number 3 for our changes .

B) Another paper is

Barker, H.W., B. A.Wielicki, and L. Parker, 1996: A parameterization for computing grid-averaged solar fluxes for inhomogeneous marine boundary layer clouds. Part II: Validation using satellite data. J. Atmos. Sci., 53, 2304–2316.

(May be also the Part 1). This paper, I believe, was the first to use the ratio $\nu=(<\tau>/\sigma\tau)2$ to quantify cloud inhomogeneity.

We have also included a reference to this paper in the current manuscript

"Therefore, similarly to the studies by Barker et al. (1996), who used ratios between mean τ and the variance of τ, Davis et al. (1999) and Szczap et al. (2000) utilized the normalized inhomogeneity measure ρ_τ to quantify the horizontal inhomogeneity of τ."

3. **Small-scale break $\xi\tau,S$. I wonder if the small-scale noise can be reduced by averaging over all cases or over all columns (or rows) in one case. Also, please, compare the location of your small-scale breaks with the once reported by Davis et al. (1999). It was not clear for me what could be learned about cloud structure from the reported small-scale breaks. How does your conclusion depend on pixel size and uncertainty in observations? Please summarize.**

If we average over all columns as proposed by the reviewer the result looks like in Fig. 6 from the original submitted manuscript. The overall appearance of the lines is more smooth then, but their distribution on the y-direction is relatively broad. Therefore, we decided to use the power spectral densities along and across the prevailing direction only for the discussion of the small-scale break. We have also compared the results to the values given by Davis et al. (1999), from which it becomes clear that the small-scale break may have a physical explanation (for scales larger than the pixel size) and is not only related to the white noise, which of course is the reason for the flat power spectral signal for scales below the pixel size. However, since there is a directional behavior we like to keep this paragraph, although it is not possible to fully explain the reason for the small-scale breaks, which are in larger size ranges than the pixel size.

"[…] Furthermore, the ranges of the derived small-scale breaks $\xi_{\tau,s}$ are found to be close to the ranges of the small-scale breaks reported in literature. Davis et al. (1999) derived small-scale breaks for a broken-stratocumulus/towering cumulus cloud complex from LWC measurements with a particulate volume monitor probe (4 cm resolution) at ranges of about 2-5 m. They proposed that those small-scale breaks are related to extreme values in the detected LWC, which appear on small horizontal scales. Besides Poissonian fluctuations of the cloud optical thickness τ and the white noise related to power spectral signals at scales below the pixel size this might be a further explanation for the derived small-scale breaks in the current study and needs to be investigated in further studies."

4. A) Large-scale break $\xi\tau$,L. After Fig. 7, I'd recommend to mention that CARRIBA $\xi\tau$,L << VERDI $\xi\tau$,L especially, for the most homogeneous cases of C-02 and C-03. This is partly because $\xi\tau$,L, as the radiative smoothing scale, is the harmonic mean of the cloud geometrical thickness and the transport mean free path. Both factors are much smaller for Arctic stratus than for cirrus.

We thank the reviewer for this advice. We used the reviewer's suggestion and included this information at the end of the relevant paragraph.

"[…] Furthermore, the resulting large scale breaks $\xi_{\tau,L}$ confirm the results from the derived decorrelation lengths ξ_τ that the subtropical cirrus observed during CARRIBA is more homogeneous (larger ξ_τ and $\xi_{\tau,L}$) than the Arctic stratus from VERDI (smaller ξ_τ and $\xi_{\tau,L}$). This is related to the fact that $\xi_{\tau,L}$, which is the radiative smoothing scale, is a function of the cloud geometrical thickness and the transport mean free path. For Arctic stratus both parameters are significantly smaller than for subtropical cirrus."

B) I'd also recommend comparing the theoretical values of the radiative smoothing scale with the observed ones, $\xi\tau$,L.

We have now included a comparison to theoretical values reported by Marshak et al. (1995):

"[…] Especially the values for the Arctic stratus are in the size range, which was also reported by Marshak et al. (1995), who found scale breaks for fractal clouds in the range of 200-500 m. […]"

5. A) Retrieval of τ. I know that several references on the retrieval processes are given. However, the way τ-field has been retrieved is important for understanding the analysis provided in the paper. The main question is how much the retrieved τ-field is influenced by 3D radiative effects. I'd recommend to briefly describing here the retrieval processes.

We have now included the most necessary information on the retrieval technique we applied in this study:

"Simulations are performed with the radiative transfer solver DISORT 2 (Discrete Ordinate Radiative Transfer). Input parameters such as cloud optical properties, aerosol content and spectral surface albedo are provided by the library for radiative transfer calculations (libRadtran, Mayer et al., 2005). The required profiles of thermodynamic parameters are derived from measurements from radiosondes and/or dropsondes. Despite of assuming plane-parallel clouds in the simulations, the investigation of 3D radiative effects is still possible using the retrieved fields of τ, but directional features related to the scattering phase function are avoided. $I_\lambda^{\downarrow}$ and $I_\lambda^{\uparrow}$ were simulated as a function of values of τ_{ci} and τ_{st}, respectively. The simulations were performed for all scattering angles within the FOV of AisaEAGLE. Thus, simulated grids of possible $I_\lambda^{\downarrow}$ and $I_\lambda^{\uparrow}$ and corresponding τ_{ci} and τ_{st} are available for each time step of the measurements and each spatial pixel. The retrieved τ_{ci} and τ_{st} are derived by interpolating the simulated $I_\lambda^{\downarrow}$ and $I_\lambda^{\uparrow}$ to the measured value for each spatial pixel using a linear interpolation. More detailed descriptions and sensitivity tests of the applied retrieval procedures are reported by Schäfer et al. (2013) for subtropical cirrus and by Bierwirth et al. (2013) as well as Schäfer et al. (2015) for Arctic stratus. […]"

B) Another point, I was not convinced that from analyzing the structure of the retrieved cloud optical depth fields for inhomogeneity, one can learn something new compared to the analysis of the measured fields of radiance. I'd recommend, in parallel to, say, Figs. 5 or 6 (or even 7), showing some results of the analysis of energy spectra for the radiance fields.

There is one major reason for using cloud optical thickness fields instead of radiance fields in this study. If radiance fields are used instead of cloud optical thickness, the features of the scattering phase function would contaminate the analysis of cloud inhomogeneity structures performed with 2D methods. For one-dimensional analysis using autocorrelation functions or Fourier transformations cloud parcels are observed within the same narrow scattering angle range. Changes in the observed radiance field are then most probably related to the cloud and not to the features of the scattering phase function. However, for 2D images this is different since they cover a wide range of scattering angles. Please find below in Fig. 2 a measurement example extracted from Schäfer et al. (2013), which shows a measured radiance field (left) and the corresponding cloud optical thickness field (right). The radiance plot clearly shows features of the scattering phase function, namely the increase of radiance closer to the Sun (located on the right side of the image). This signature of the radiance field would be imprinted into the autocorrelation or Fourier analysis. In other measurement cases e. g. also halo events or cloud bows could be imprinted in the radiance field. Therefore, we have used the corresponding fields of cloud optical thickness, where those features of the scattering phase function are not included as can be seen in the image on the right side.

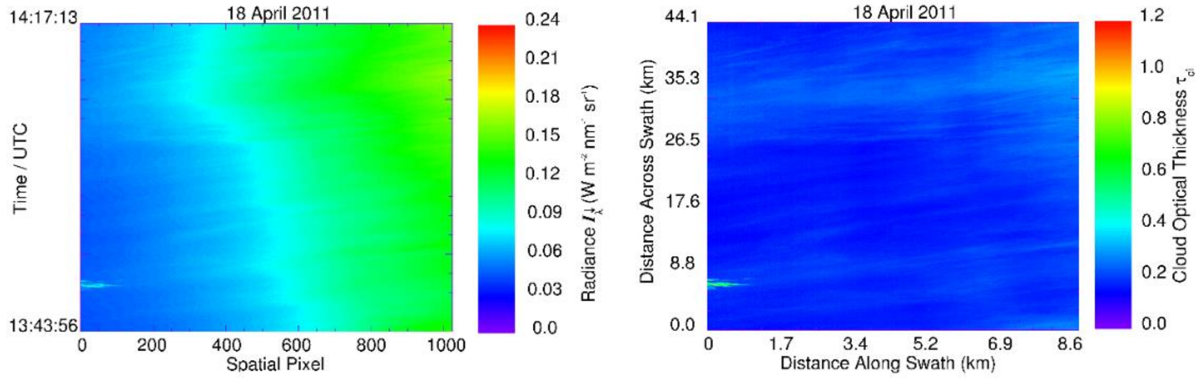

Fig. 2: Radiance field (left) and corresponding cloud optical thickness field (right) (Schäfer et al., 2013)

We have also included more information and extended the relevant paragraph to make it more understandable to the reader.

"As proposed by Marshak et al. (1995), Oreopoulos et al. (2000), or Schröder (2004), horizontal cloud inhomogeneities are studied by scale analysis of cloud-top reflectances. However, radiance measurements include the information of the scattering phase function (e.g., forward/backward scattering peak, halo features) in the measured fields of radiance (Schäfer et al., 2013). To avoid artefacts in the scale analysis resulting from such features, parameters that are independent on the directional scattering of the cloud particles have to be analyzed. The cloud optical thickness τ *does not include the fingerprint of the scattering phase function. Therefore, the ground-based and airborne measured fields of* $I^{\downarrow}_{\lambda}$ *(CARRIBA) and* $I^{\uparrow}_{\lambda}$ *(VERDI) were used to retrieve horizontal fields of* τ *with a spatial resolution of less than 10 m. The retrieved fields of* τ *were then applied to investigate horizontal cloud inhomogeneities of subtropical cirrus (index ci) and Arctic stratus (index st)."*

"[…] Despite of assuming plane-parallel clouds in the simulations, the investigation of 3D radiative effects is still possible using the retrieved fields of τ*, but directional features related to the scattering phase function are avoided. […]"*

6. **Decorrelation length ξτ**. I wonder why did you use 1/e for the squared autocorrelation function rather than $1/e^2$.

We thank the reviewer for this advice. Now, we have used this new threshold to recalculate the decorrelation lengths, revised the relevant figures, tables, and text parts. However, the overall conclusion we made on behalf of the decorrelation lengths has not changed. Only the magnitude of the derived values for the decorrelation lengths have changed. Therefore, here in this reply, we only like to show the revised Figure 4. The remaining changes to the manuscript with regard to new decorrelation length values are marked in the additionally submitted author’s response document.

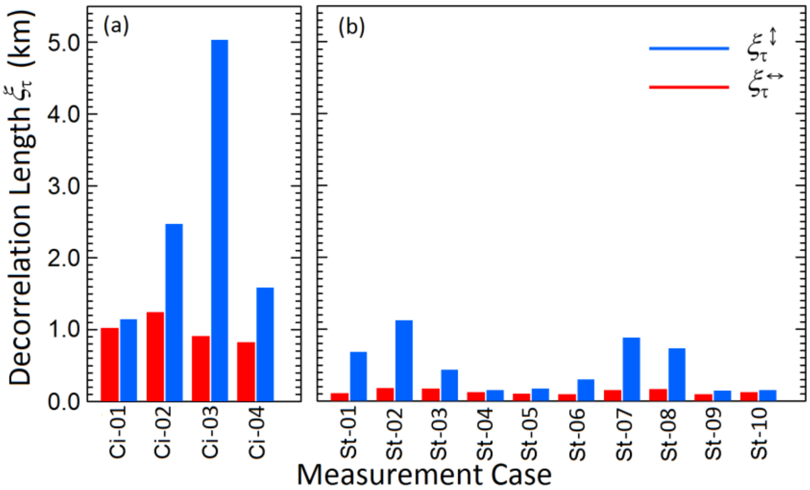

Fig. 1: Revised Fig. 4 using the new threshold for calculating the decorrelation length.