We thank the reviewer for the helpful comments which improved the manuscript significantly. The detailed replies on the reviewer's 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.

<u>Comments:</u>

1. **Lines 46-60. A very incoherent paragraph. Cloud inhomogeneity effects on gridded fluxes are mixed with effects on satellite retrievals. Lines 49-50 talk about retrievals, and the next sentence talks about GCMs. Moreover, the first problem facing GCMs is not the lack horizontal photon transport, but the absence of subgrid variability, i.e., the unavailability of PDFs of cloud condensate for each layer. If such PDFs were available at least IPA calculations would in principle be possible (still no horizontal transport). The dissemination of confusion continues later on. In lines 55-58 the limitations of IPA compared to 3D are followed in lines 58-60 by an irrelevant example of errors found by Shonk and Hogan when comparing PPH and IPA.**

We agree on that. Especially, the reference to GCMs were confusing and we agree that the reference to Shonk and Hogan (2008) is irrelevant and confusing at this point. Therefore, we removed it and revised the paragraph as follows:

"Several independent studies investigated the influence of the plane-parallel assumption on cloud retrievals (e.g. Cahalan, 1994; Loeb and Davies, 1996; Marshak et al., 1998; Zinner et al., 2006; Varnai and Marshak, 2007). They found that the magnitudes of model biases are related to the degree of horizontal photon transport. In 1D radiative transfer simulations clouds are divided into separate vertical columns with horizontal homogeneous optical and microphysical properties (independent pixel approximation, IPA). However, horizontal photon transport cannot be neglected in case of inhomogeneous clouds. Additionally, multiple scattering due to 3D microphysical cloud structures smooth the horizontal radiation field. On small scales, this limits the accuracy of IPA. For example, Cahalan (1994) and Marshak et al. (1995) revealed discrepancies for individual pixel radiances exceeding 50 % due to a plan-parallel bias."

"In many remote-sensing applications clouds are assumed as plane-parallel (Francis et al., 1998; Iwabuchi and Hayasaka, 2002; Garrett et al., 2003), which may introduce biases into the modeled radiation budget (Shonk et al., 2011). For example, in the cases of cirrus, Carlin et al. (2002) found a plane-parallel cirrus albedo bias of up to 25 % due to spatial cirrus inhomogeneity. For Arctic stratus over variable sea-ice surfaces, Rozwadowska and Cahalan (2002) reported a plane-parallel albedo bias of less than 2 %, but an absolute value of the transmittance bias that can exceed 10 %."

2. **Lines 61-68. Here, cloud overlap is mixed into 3D effects and Monte Carlo discussion. Misleading. You can account for overlap perfectly, but still ignore 3D effects by performing IPA calculations on the perfectly overlapped cloud field.**

The reviewer is right. Talking about cloud overlap schemes at this point is misleading. Therefore, we removed those parts, which are related to cloud overlap schemes, in the resubmitted version of the manuscript.

3. **Lines 75-77. I'm sure that ECMWF models do not need the two point statistics of cloud structure (as derived by autocorrelation and power spectrum analysis), but some information on the PDF, i.e., the inhomogeneity parameters of section 3 derived from one point statistics, so invoking "spatial features" "below the meter scale" is again inappropriate.**

In conjunction with the comment above and comments by the other reviewers we removed the reference to ECMWF models in the resubmitted version of the manuscript. We further revised the statement "spatial features below the meter scale" to clarify that this is related to clouds in reality.

"General circulation or numerical weather forecast models require sub-grid scale parameterizations of, e.g., cloud structures, liquid water content (LWC), and/or ice water content (IWC) (Huang and Liu, 2014). In reality, cloud structures reveal spatial features down to distances below the meter scale (Pinsky and Khain, 2003). Therefore, measurements with appropriate spatial and temporal resolution have to be conducted in order to derive the needed parameterizations. […]"

4. **Lines 141-142: "However, the fact that rho_tau can exceed values of unity and depends on the average value might lead to misinterpretations." Why? I don't see anything wrong with values greater than unity.**

That is true. There is nothing wrong with values greater than unity. This wording belonged to a former version of the manuscript, where we wanted to say that there is no upper limit for ρ_τ where the clouds can be found to be inhomogeneous to 100 %. We revised this part by the following:

"However, ρ_τ has no predefined upper limit, which might lead to misinterpretations in a variability analysis. This renders ρ_τ not as a quantitative, but qualitative measure only."

5. **Lines 157-158: It would be simpler to say that chi is the scaling factor with which mean tau needs to be multiplied to approximate the IPA albedo.**

We have used the suggested wording from the reviewer to simplify this sentence.

"Thus, the logarithmically averaged τ provides a way to account for cloud inhomogeneity effects in plane-parallel radiative transfer calculations using χ_τ as a scaling factor with which τ needs to be multiplied to approximate the IPA albedo."

6. **Page 7: One has to be careful when comparing inhomogeneity parameters across publications. First, pixel size matters. Second, and most importantly, the domain size matters. The bigger your reference domain, the wider the PDF, the larger the inhomogeneity. So this is by no means a trivial comparison.**

We thank the reviewer for this advice. Indeed, a comparison of the results is complicated for different pixel and domain sizes. However, we still like to show and refer the results from other studies. Therefore, we kept this comparison, but we included a paragraph, which points out the restriction of a comparison. The reader has to keep in mind that the results from the different studies are related to different pixel and domain sizes. Furthermore, we included the pixel and domain size of the investigated cases in Table 1 and added the pixel and domain sizes for the literature cases at the relevant parts.

"The three 1D inhomogeneity parameters ρ_τ, S_τ, and χ_τ are calculated for each retrieved field of τ_{ci} and τ_{st} from the CARRIBA and VERDI campaigns. The results are listed in the right three columns of Tab. 1. When comparing them to literature values one has to keep in mind that cloud inhomogeneities appear on different spatial scales. E.g., cloud fields may change on synoptic scales (~ 100 km) or dynamic scales (10 – 100 m) depending on the cloud type. Therefore, inhomogeneity parameters depend on the pixel and domain size of the analyzed cloud fields. The larger the domain size or the smaller the pixel size is, the broader the probably density function of the cloud parameter may become. Therefore, a comparison of different cloud cases is only valid when pixel size and cloud domain are in the same range."

We also revised this statement in the conclusion part of the manuscript:

"Considering the pixel and domain size of the analyzed measurements, the results from the calculated 1D inhomogeneity parameters ρ_τ, S_τ, are in agreement with values given in the literature for similar cloud types."

7. **Lines 188-190. It's not a matter of directional dependence only. It's mostly a matter of spatial coherence of cloud condensate, in other words how the variability is distributed across scales.**

That is true. Therefore, we have included this statement to the relevant sentence.

"Therefore, not only the horizontal inhomogeneity, but also the spatial coherence of cloud inhomogeneity parameters and their directional dependence need to be investigated (Hill et al., 2012)."

8. **Lines 204-205: Why are negative autocorrelations ambiguous?**

The reviewer is right. The use of the word "ambiguous" is not suitable for the statement we wanted to give here. We revised the paragraph by the following lines to clarify what we wanted to say at this point:

"Here, only the degree of correlation matters; the positive or negative sign of the autocorrelation result is of less importance. To avoid misinterpretations with the sign, the squared autocorrelation function $P^2_\tau(L_x,L_y)$ is used here.

9. **Eq. (5): If the scale length is typically defined as the distance at which the autocorrelation drops to 1/e, shouldn't the scale length of squared autocorrelation be defined as the distance where it drops to 1/eˆ2?**

We thank the reviewer for this advice. It is true that $1/e^2$ should be used as a threshold when the squared autocorrelation function is used. We recalculated 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.

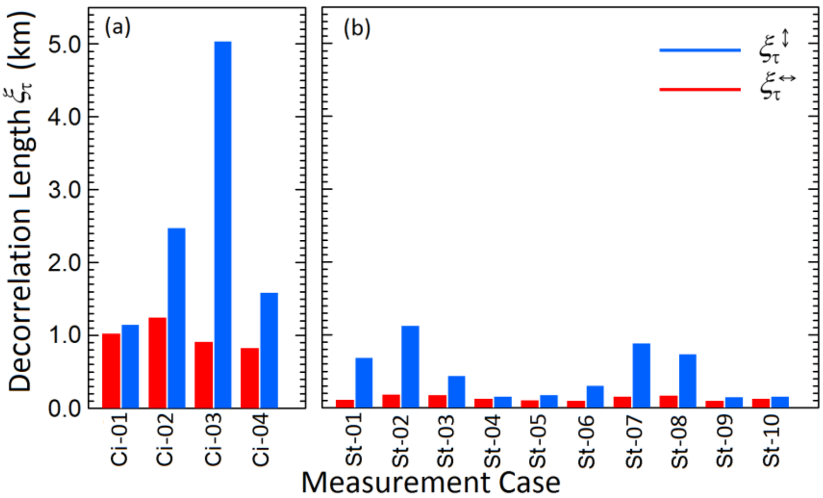

Fig. 1: Revised Fig. 4 using the new threshold for calculating the decorrelation length.

10. **Lines 255-256: It's not that they are not well-suited, it's that by themselves they provide incomplete information, i.e., not the whole story.**

We revised this sentence by the reviewer's suggestion.

"This reveals that the 1D inhomogeneity parameters ρ_τ, $S\tau$, and χ_τ just provide incomplete information for a comparison of different types of clouds as they are not able to consider the horizontal structure of cloud inhomogeneities."

11. **Power spectrum scale break analysis: Have the authors given any thought on whether the comparisons of scales in terms of physical units (m) makes sense when the pixel sizes are different? With pixel size varying, the extent to which the smoothing is resolved is also different, so I was wondering whether defining the scale lengths in terms of pixel number would bring the results closer together.**

Yes, we had a thought on this. It is right, a comparison of scales in terms of physical units (m) may be difficult, when the pixel sizes are different. However, here in this case they differ not much. Indeed, along the swath (pixel width) the pixel size for the subtropical cirrus cases is twice as large as for the Arctic stratus cases. This is due to the geometry and different distances between sensor and cloud. However, the pixel size across the swath direction (pixel length), which depends on the integration time, cloud/ aircraft velocity, is almost the same for the measurements during CARRIBA subtropical cirrus) and VERDI (Arctic stratus). The pixel length is about 5±2 m for both cases. Furthermore, the differences are far below the detected scale breaks. Therefore, a comparison is possible in this case. To better clarify this issue, we added the following lines to the manuscript:

"[…] Furthermore, both cloud cases, subtropical cirrus and Arctic stratus, exhibit a similar pixel length along L_y (5±2 m), which results from the chosen frame rate (subtropical cirrus: 4 Hz, Arctic stratus: 30 Hz) and given cloud (20 m s^{-1}) and aircraft velocity (70 m s^{-1}). This allows a direct comparison between these two different cloud types with different observation geometry."

Typos and other minor stuff:

1. **Line 10: "VERtical" instead of "VERical". Also Line 97.**

Changed to *"VERTical"*

2. **Lines 120 and 272: I think you wanted to use "fingerprint' rather than "footprint". The term "footprint" in remote sensing indicates the resolution, i.e., pixel size.**

Changed to *"fingerprint"*

3. **Line 328: "inhomogeneity".**

Changed to *„inhomogeneity"*

4. **Line 337: "too small".**

Changed to *"too small"*