<u>We thank the reviewer for his helpful comments which certainly have helped to improve the manuscript significantly.</u>

The reply is structures as follows. Reviewer comments have bold letters, are numbered, and are listed always in the beginning of each answer. The reviewer comments are followed by the authors comments with an explanation if necessary and revised parts of the paper. The revised parts of the paper are written in quotation marks and italic letters.

<u>General comments:</u>

1.) **Although the instrument was hyperspectral, by most versions of that definition (there are several), only a single wavelength was used in the analysis. I think the authors should reconsider the use the word "hyperspectral" in their title. At the very least, they need to point out that the results in this paper do not capitalize on the hyperspectral capabilities of the instrument but that future analyses may better exploit the wavelength dependencies of cloud radiation.**

➔ Thanks for pointing this out. We agree that "hyperspectral" is not the appropriate wording for our study and might have caused much confusion. This is mentioned more clearly now (at the end of the Introduction and in the beginning of chapter 3 "Retrieval of cirrus optical thickness").

"It needs to be mentioned that this study does not fully capitalize on the hyperspectral capabilities of AisaEAGLE. Here only one wavelength (530 nm) is used. Thus, the paper is regarded to be a first feasibility study to show the potential of AisaEAGLE for ground-based measurements of downward solar spectral radiances and for retrievals of cloud microphysical properties like the cirrus optical thickness from the spectral measurements. In future studies the wavelength range used for data evaluation will be extended to increase the number of retrieved cloud optical properties."

"In a first feasibility study the simulations were performed for 530 nm wavelength only, which was chosen with regard to the wavelength of the LIDAR measurements at BCO."

➔ To avoid any confusion in the revised manuscript we omit to call the measurements "hyperspectral". The title do not includes the word hyperspectral anymore.

"Retrieval of Cirrus Optical Thickness and Assessment of Ice Crystal Shape from Ground-Based Imaging Spectrometer Measurements"

2.) **The relative lack of sensitivity to particle size is rather surprising but if it is correct, the authors can show this rather simply by comparing the asymmetry parameters for the three particle sizes they analyzed in the simulations. They must be about as close as the relative differences in retrieve tau for those cases. After all, what is retrieved in these cases is tau*(1-g). This is one example where I would prefer to see the authors provide better physical insight into their results. And because they are retrieving something close to tau*(1-g), that explains the potential ambiguities in their results with respect to crystal habit. They have explored only a small subspace of possible crystal habit and the Yang models are only a small sample of possible realizations of ice crystal scattering. In fact it**

may even be beneficial to present results in units of tau*(1-g), particularly when interpreting parameter sensitivity because it will become immediately apparent to which parameters tau is relatively sensitive (habit) or insensitive (size). A little more physical insight will be very helpful to the general readers of this paper.

- ➔ We agree that there was a little less physical insight. In this regard we improved the revised manuscript significantly as will be explained below. However, we think using tau*(1-g) is not an option for the retrieval described in the manuscript. The first reason why not using tau*(1-g) is that the scaled optical thickness applies only for optical thick clouds and for reflected radiance above the cloud. Both does not hold for our measurements where transmitted radiance is measured and cloud optical thickness is less than two. Additionally, for our directional measurements with different scattering angles, the optical thickness would have to be normalized with the scattering phase function. Here we think that the asymmetry parameter (Henyey-Greenstein function) is not the best way to describe the scattering phase function of ice crystals.

- ➔ However, to better explain the physics behind the measurements and retrieval results, we included a plot of the scattering phase functions instead to explain the sensitivity to the ice crystal shape and the effective radius. Looking at the scattering phase functions it is easy to figure out why the results are less sensitive to the effective radius, but highly sensitive to the different ice crystal shapes. In the range of scattering angles observed in the four cases (highlighted grey in Figure 9), the phase functions are mostly similar when comparing different effective radii but show significant different values for different shapes. A discussion on the connection between scattering phase functions and sensitivities is given in the revised manuscript.

  "Comparing the scattering phase functions in Figure 9b it can additionally be seen that, except in the maximum of the halo region and below 20°, over most of the captured scattering angle range they are quite similar. Since the differences between the scattering phase functions calculated for different reff appear mostly in the forward and backward scattering range but not in the captured scattering angle range, this explains the small variation found in the sensitivity study."

3.) This is less of a comment than a question: didn't the cloud lidar provide optical thickness information? Of course, habit assumption would be implicit in those retrievals as well. If that data exists, can they be compared with the results here? The lidar appears to have been used more as a ceilometer.

- ➔ That is true. For this study the lidar has been used more as a ceilometer since the cloud top was close to the detecting range of the lidar. Having no reference clear sky measurements above the cirrus, a reliably retrieval of tau from the lidar signal was unfortunately not possible. For future studies hopefully this can be improved.

<u>Minor comments:</u>

1.) p. 1203, l. 7-9. The second sentence in this paragraph, on spatial inhomogeneity, has nothing to do with the first sentence, on crystal orientation. I recommend a reorganization of this paragraph which seems to jump over various topics. Also, the 25% reference needs to be better qualified. I doubt that this represents some upward limit in albedo bias but it may be misinterpreted as such.

➔ First sentence on the orientation of ice crystals has been removed because of the given reason. The Sentence was restructured to avoid misunderstandings.

> *"Carlin et al. (2002) found changes of cirrus albedo as derived from millimeter cloud radar datasets and independent column approximation of up to 25% due to the spatial cirrus inhomogeneity (over weakly reflecting surfaces and at high solar zenith angles)."*

2.) p. 1204, l. 3-6: The mismatch between remote sensing and in situ measurements probably has very little to do with "enhanced absorption" and almost everything to do with the second part of this sentence, the mismatch in sampling volumes between in situ and remote sensing measurements.

➔ Thanks for pointing this out. The part with "enhanced absorption" has been removed.

> *"This disagreement has not been resolved yet, partly because it has been extremely difficult to collocate remote sensing above the clouds and concurrent in-cloud microphysical measurements."*

3.) p. 1204, l. 13-15. In the last sentence of this paragraph, "These issues may only be solved…" I don't think it is explained how these issues may be solved by the two methods listed.

➔ It is said that cloud inhomogeneities and the surface albedo can introduce high uncertainties to the retrieved cloud microphysical properties. To avoid this it is necessary to capture also the small scale inhomogeneities of the cloud and to reduce the impact of the surface albedo. Therefore, measurements with a high spatial resolution are necessary. To avoid the impact of the surface albedo, the best way is to perform ground-based measurements of the downward directed radiance. Especially for thin clouds, measurements of the upward directed radiance will always be contaminated by reflected radiance from the ground, transmitted through the cloud. We reworded this part to clarify our reasoning.

> *"These issues can be mitigated by highly resolved measurements to cover cloud inhomogeneities in case of ground-based measurements less affected by surface albedo."*

4.) p. 1206, l. 26: Should be "tangent"

➔ Changed to "tangent"

5.) p. 1208, l. 8: need to say "U.S. National Institute…"

➔ "U. S." inserted

6.) p. 1208, l. 16-17: Instead of "larger" and "smaller" wavelength use "longer" and "shorter".

➔ Changed to "shorter" and "longer"

7.) p. 1209: l 21: The spectral range from 400-970 nm covers more than one octave, meaning that the range from 800-970 nm requires order sorting. Actually, it will be required for wavelengths longer than 2*shortest wavelength. Did the detector array include order sorting filters? If not, this comment needs to me moved to the category of "major"! If no order sorting was used, a major correction to the data will be required.

➔ AisaEAGLE has a second order depression using order blocking filters near the detector. We added this to the instrument description.

> *"For each spatial pixel the radiance is measured spectrally between 400 nm and 970 nm with 488 wavelength pixels. The spectral resolution is 1.25 nm FWHM. Since this spectral range covers more than one octave, the range from 800-970 nm requires order sorting. For this, AisaEAGLE has a second order depression using order blocking filters mounted near the detector."*

8.) p. 1210, l 1: see comment number 6.

➔ Changed to "shorter" and "longer"

9.) p. 1210, l 1-14: I think I follow the discussion on wavelength range and smear correction but it was a struggle. I suggest rewriting this paragraph to simply the discussion.

➔ The paragraph on smear correction has been revised to make it better understandable.

10.) p. 1211, l 1-2: for what cases and when do the MODIS size retrievals correspond?

➔ See comment 15

11.) p. 1211, l 25-26: It is not explained how the all-sky images provide thickness information. After all, if they do provide this, why do you even need the spectral imager? (!) In other words, this sounds completely qualitative, which I am sure it is, but a little more discussion is warranted.

➔ The reviewer is right, the all-sky images have been used qualitatively only. This is mentioned more clearly now.

> *"During CARRIBA, this supplementary information was provided by all-sky images and LIDAR measurements. The all-sky images do not give a quantitative value of τ_{ci} but were evaluated qualitatively. By eye, an experienced observer is able to judge of the retrieved τ_{ci} are in the range left or right of the maximum of the retrieval curve presented in Figure 6."*

12.) p. 1213, l 13: What is meant by "azimuthal position is rectangular…"?? Perpendicular?

➔ Changed to "perpendicular"

13.)p. 1213, l 26: halo should not be capitalized.

- ➔ Changed to “halo”

14.)p. 1214, l. 1: Should be “irregular”, not “unregular”.

- ➔ Changed to “irregular”

15.)p. 1216, l 8-11: It is still confusing how MODIS retrievals are implemented. Please explain better here and in comment number 10.

- ➔ MODIS data are not implemented directly in the retrieval. The data were just used as a guideline to estimate the range of possible effective radii in the surrounding area of Barbados. This is pointed out more clearly now.

  “For the simulations a fixed r_{eff} has to be defined as no direct retrieval from AisaEAGLE is possible. For this, a value of 20 µm was assumed. The assumed r_{eff} was taken from Moderate Resolution Imaging spectro-radiometer (MODIS) data collection 5 as best estimate for the area close to BCO.”

16.)p. 1219, l. 20-22: This last sentence in the second to last paragraph is confusing to me. I don’t think they can ever remove habit assumption. After all, as I said in the general comments, they are only considering a small subset of possible habits. There will always be residuals between measurement and model. How can they verify they are not habit-dependent? I think it is better to state that angular information provides additional information on habit. That is different than saying it removes all ambiguity of crystal habit, which this seems to say.

- ➔ We agree that the wording of this sentence have been misleading. We intended to say that due to the multi-directional measurements an estimation on the dominating crystal habit can be derived independently. This makes a fixed assumption of the crystal shape unnecessary. The sentence is restructured now to avoid misunderstandings:

  “The results will then be implemented in the retrieval algorithm. The additional angular information might allow developing a cirrus retrieval technique independent on any guess of ice crystal shape.”