Thank you for your helpful comments and for taking the time to point out options to improve our manuscript.

We have revised the manuscript following both reviewers' suggestions. In the following, we reply (in standard font) to your comments (cited in italics).

General Comments 1. What is the primary purpose of the paper? I don't get a good indication of the direction of the paper. Is the primary purpose to: a. Retrieve Arctic cloud properties, b. Intercompare a 2-wl or 5-wl cloud retrieval method with 'closure' provided by comparison to in situ measurements, or c. Introduce the AisaEAGLE hyperspectral imaging camera?

Short answer: Mostly **c**, to which **b** is a prerequisite; and **a** as the ultimate goal of our studies, yet the presentation of extensive data sets is not the focus of this particular paper.

Long answer: It is good that you (and also Reviewer #2) point out the need to better describe the goals of the paper. We appreciate that you distinguished three scenarios with a certain set of appropriate comments for each. After reading your comments to the three 'scenarios', we find that none of the comments should be disregarded no matter what the answer is – a, b, or c. In fact, we find it not easy to pick one answer and rule out the other two entirely. It is (or should be) rather clear that **a** is not and cannot be the focus of the paper, as it contains only one measurement case. Still, we consider the presented data interesting (yet not ground-breaking) due to the scarcity of observations in the Arctic. The 'newest' aspect of this study was to make the cloud retrieval work with the hyperspectral data in combination with 'established' spectral nadir reflectance measurements, hence, **c** is the main topic of the paper. The retrieval from the hyperspectral data are, however, based on the established procedure of the cloud retrieval from spectral nadir radiances (SMART-Albedometer). But even that is not fixed forever, and recent work such as that by Coddington et al. (2010, 2012) and Werner et al. (JGR, submitted 2012) has made it clear to us that there is more potential in the 'established' retrieval. As a first step toward an improved version of the 'established' procedure, on which the hyperspectral retrieval then relies, we extended our algorithms to the 5-wl version. We understand well that a thorough analysis of the information content as outlined by the 2012 papers by Coddington et al. and King and Vaughan is an essential tool to advance further in retrievals from where we are now. Yet we feel that the combination of established spectral measurements and the new hyperspectral technology, which we present in this paper, is a different (technological) step of its own. While the implementation of the methods published by above authors in 2012 into our algorithms seems promising and ultimately necessary, we prefer to finish this particular manuscript as it is within its time line rather than trying to adjust and re-run the code hastily.

To present the goals of the presented study more clearly, the following paragraphs have been added to the introduction:

In this paper, we demonstrate the combination of spectral and hyperspectral data in the retrieval of cloud properties (optical thickness, effective radius) for one flight scenario during SoRPIC. In that field campaign, that radiance was measured by several instruments, including a nadir spectrometer and an imaging spectrometer. Our goal in this study was to combine these measurements into a novel retrieval procedure, based on the classic retrieval by Nakajima and King (1990). The classic retrieval uses two wavelengths; it can, however, be improved by including the information from more wavelengths which is available from modern spectral radiometers. In this study, we proceed in that direction by extending the retrieval algorithm to five wavelengths, as suggested by Coddington et al. (2010). Further retrieval improvement is possible by including more spectral information, which was recently shown by Coddington et al. (2012). This paper covers the combination of spectral and hyperspectral data sets, which each have their own strengths and weaknesses. Meanwhile, the analysis of information content as demonstrated by Coddington et al. (2012) and King and Vaughan (2012) is currently being developed as an important tool for coming airborne cloud retrievals. Other questions we will address in ongoing and future studies include the problem of cloud observations over bright surfaces or a detailed comparison to satellite data.

In this paper, we proceed as follows: First, the retrieval of cloud optical thickness and effective radius is applied to spectral nadir radiance (350–2100 nm), comparing the results of the two-wavelength (2WL) to the results of the five-wavelength (5WL) approach. Second, a field of cloud optical thickness is retrieved from the hyperspectral data set from the imaging spectrometer (400–1000 nm). This requires a constraint to the cloud effective radius which cannot be retrieved without measurements well beyond 1000 nm. Therefore, we took an intermediate step to find out how much the choice of that constraint matters. Finally, the retrieval results are compared to cloud properties derived from in-situ sampling of cloud particles, highlighting some of the endemic problems of single-aircraft cloud observations. This study is performed for a single case (one measurement flight during SoRPIC) where the conditions were sufficiently well defined to focus on the retrieval performance rather than complications and ambiguities in the cloud field. In particular, the presented case is a cloud layer with known vertical extend, and the warm temperatures made sure that there was no ice in the cloud. While ice particles in clouds were a primary objective of the SoRPIC campaign, their clear absence works in the favour of this particular study.

The measurement set-up is presented in Section 2; the meteorological situation during the chosen flight in Section 3. Section 4 deals with the comparison of the 2WL and the 5WL approach for nadir reflectance. The retrieval is then geometrically extended to the hyperspectral data in Section 5. Conclusions are given in Section 6.

I find that the authors present histograms of retrieved optical thickness and droplet effective radius (Figure 7), yet no discussion is given as to what was learned from this addition of data to the 'data base of the Arctic climate system' (line 11, p 7755).

As outlined in the reply to the previous comment, the addition of vast arrays of optical thickness and effective radius to the 'data base of the Arctic climate system' is not the focus of this paper, which is more about technological aspects and their implementation in retrievals. With those steps finished, we can re-examine the rich data set which we collected during SoRPIC (and a follow-up campaign, VERDI) to evaluate those with better tools than we had before. In any case, all we would add is a single cloud case in rather peculiar meteorological conditions (warm front with extraordinarily high temperatures).

Many of the important issues in the role clouds play in Arctic climate touched on by the authors in the introduction were not discussed further in the text. Namely, validation of satellite cloud retrievals for measurements over a bright surface, mixed-phase clouds, and verification of retrieved products impacted by instrument errors (this topic is discussed by authors) and forward model errors.

It is true that there is a discrepancy between the introduction and the remaining paper with respect to the listed topics. It arose because our general line of research aims at targeting those issues, but we need to remove the discrepancy from this particular paper. Bright surfaces were hardly present during SoRPIC, which was mostly conducted over the open ocean; but we are currently working on data over sea ice with similar analyses. The retrieval data will be compared to satellite data, but later; and mixed-phase clouds were excluded from this study to avoid ambiguities (see reply to comment 1). The introduction of this paper has been re-worded to reflect these circumstances more accurately.

A more developed description of the experimental data will help. Were the measurements only over ocean and not a bright surface?

Only open ocean, no snow or ice. This has been included in the manuscript.

Of the 13 research flights, is it correct you are presenting results from 1 flight and do the other 12 research flights add new results for the Arctic data base?

Yes and yes, but this paper is more focused on the principles of the retrieval. Other cases are interesting in themselves, but were not necessarily suitable for the purposes of this study. Additionally, the hyperspectral imager was operative only on 3 of the 13 flights, and the presented case was the most suitable one for this first detailed analysis.

In the introduction, mixed-phase clouds are emphasized; are your results for water and ice phases? Is there the possibility of comparison with satellite results?

Due to the temperatures, all cloud particles were liquid (for the presented case).

The authors, by propagation of measurement error, make determinations of retrieval performance for two different retrieval methods. However, as they discuss in the introduction (page 7755, line 4) the assumptions made in the radiative transfer model also impact retrieval error. The authors do not reference recent work that has been done to investigate the impacts of both measurement and model errors on retrieved cloud properties. Such impacts should be included for an attempt to 'provide closure' between the retrieval methods. L'Ecuyer et al. [2006] and Coddington et al. [2012] are two examples of the objective assessment of retrieval errors on retrieved cloud properties. Recent work by King and Vaughan [2012], use a similar form of objective assessment in the retrieval of the vertical profile of cloud droplets, which is a topic that is mentioned in this manuscript to be the reason for discrepancy between in situ and retrieved cloud optical properties.

Thank you for the useful references. They have been included in the paper. As outlined by L'Ecuyer et al. (2006), we calculated the forward model error for reasonable variations of the input parameters temperature, humidity, and surface albedo. For temperature and humidity, the measurement accuracy of 2 % R.H. and 0.2 °C, respectively, was used, and the (ocean) surface albedo was varied by 10 %. We found that the total forward model error is dominated by the radiance measurement uncertainty of 9 %. For most cases, only 0.1 % or less is added by said variations. Only for very thin clouds (the tau=0 branch of the grid) the total error increased up to 13.5 % (at 1625 nm).

The next step outlined by L'Ecuyer et al. (2006) is the retrieval sensitivity to tau and reff. This is already part of our manuscript in form of the propagated retrieval uncertainty (somewhat an inverse representation of the same issue).

We added a discussion of the forward model error to the manuscript. That replaces the subsection 'Influence of Atmospheric Profile', as the forward model error covers the temperature and humidity effects much better. The forward model error is included in a new short section that describes the radiative transfer model and its boundary conditions with more detail, as suggested by the reviewer in several instances. This section should also answer the questions which the reviewer had in other comments with respect to the radiative transfer model. It reads as follows:

> The cloud properties are retrieved from the spectral and hyperspectral radiance measurements aboard the Polar 5 aircraft. The measured data are checked against look-up tables (LUT) of simulated radiances.
>
> These look-up tables are produced with the radiative transfer package libRadtran (Mayer_2005a). The radiative transfer calculations are initialised so as to represent the environmental parameters during the SoRPIC campaign. That includes the relative geometry of the Sun, the cloud layer, and the aircraft; the aerosol optical thickness obtained from the Sun photometer; and the cloud-top height which is obtained from the AMALi lidar and the flight altitude. The atmospheric profiles are provided by libRadtran (subarctic winter, Anderson et al., 1986) and were modified with the meteorological profiles from drop-sonde launches (the standard profiles were scaled to ensure a continuous transition from the standard table to the drop-sonde data). Drop-sonde data were discarded down to the level where the temperature reading had dropped from cabin to ambient temperature and starts to rise again as the sonde falls into warmer layers. The surface albedo was set to that of open ocean, as all measurements presented here were performed over ice-free water. The optical properties of the cloud particles were calculated from Mie theory, as the warm temperatures of this case ruled out the existence of ice crystals.
>
> The look-up tables then contain the calculated values for the spectral upward radiance. They are given as a function of optical thickness tau and droplet effective radius r_eff of a plane-parallel cloud. tau is varied from 0 to 38, r_eff is varied from 4 to 18 nm. From the look-up table, the most likely combination of tau and r_eff is obtained by interpolating the measured radiance at different wavelengths into the simulated radiance grid. In general, the retrieval runs through five iteration loops. In each step, either tau or r_eff is retrieved with the other quantity fixed. For instance, if r_eff was determined to a certain value in cycle *n*-1, then in cycle *n* the optical thickness is retrieved along a (newly interpolated) branch of the retrieval grid that corresponds to that value of r_eff. The quantities are swapped in the subsequent step, and so on. With respect to wavelength, different approaches are possible and are described in more detail in the following sections.
>
> The forward model error was calculated as suggested by L'Ecuyer et al (2006). For each value of cloud optical thickness and effective radius, uncertain model input parameters were varied to a reasonable degree (typically the measurement uncertainty of that parameter). The forward model error is defined as
>
> epsilon = sqrt{ \sum_i (delta I_i / I_ref)^2 },

where delta *I_i* is radiance variation caused by the variation of error source *i*, and *I*_ref is the reference radiance. epsilon is a measure of the “noise'” generated by the model uncertainties. The error sources were quantified as follows: The atmospheric profiles of humidity and temperature, as determined by the drop sondes, were varied by the measurement uncertainties as specified by the manufacturer, Vaisala; i.e., 2 % relative humidity and 0.2 °C, respectively. The surface albedo was varied by 10 %. Another error source is the uncertainty of the radiance measurement.

The calculated values of epsilon are almost all less than 1 % and much lower than the measurement uncertainty of radiance (9 % for the SMART-Albedometer). Only for cases with tau=0 the forward model error epsilon was significantly increased to 9.3, 10.8, 11.9, 12.8, and 13.5 % at 515, 745, 870, 1015, and 1625 nm wavelength, respectively. The main contributors apart from the radiance uncertainty were the surface albedo and the temperature at almost equal parts. However, already at tau=2 those contributions to epsilon are reduced to less than 0.1 %.

The references you provided give a roadmap which might eventually lead to an optimized choice of wavelength channels for maximum retrieval sensitivity and accuracy. This will be very useful in our future studies on this subject, but is beyond the scope of the current manuscript. Nevertheless, thanks for pointing us to these publications.

In addition, the authors discuss that time delays between in situ measurements and the remote measurements prevent providing closure between the 2wl and 5wl methods. It isn’t clear in the authors discussion that the in situ data presented was obtained in such a manner (such as statistics of vertical profiles of in situ measurements by Minet al., 2003] to provide a rigorous intercomparison to the remote measurements. What is the maximum time difference allowed between comparing in situ measurements and remote measurements?

That is an excellent question, and we’d be glad to know the answer. We plan a measurement campaign in 2014 (funding pending) with two aircraft, separating the platforms for in situ measurements and remote sensing. Those flights should give us much more concrete ideas about the impact that this time delay has on the closure quality.

The case presented in our manuscript does not have in situ data obtained in a statistical manner comparable to Minet et al. Especially in the southern section, the cloud deck was too low to fly profiles (not enough vertical extent above the minimum flight altitude in what was essentially fog). Therefore, horizontal variability was sampled rather than vertical.

The authors also did not discuss the non-orthogonality in cloud optical thickness and droplet effective radius for the thin clouds presented in this study. For water clouds of thickness less *than around 40, it's well known that errors in optical thickness can propagate into errors in effective radius (or vice versa) [for example the Nakajima and King, 1990 paper that is cited].*

In order to take the non-orthogonality into account, the retrieval was performed in an iterative manner. Optical thickness and effective radius were adjusted in turns in five cycles providing good convergence. This information was added to the retrieval description. Also, this problem is discussed in the section about the impact of choice of effective radius constraining the retrieval from hyperspectral data.

Finally, more description should be included regarding the inputs used in the radiative transfer model: What is the spectral resolution of SMART, and the AisaEagle?

Below 1000 nm, the resolution (FWHM) is 2–3 nm for both instruments, beyond that 12–15 nm. The information has been added to the text.

Was the surface boundary condition assumed to be ocean?

Yes, as the entire flight leg in consideration was above ocean.

Was the Sunphotomer measurements from ground or air, and what was the relationships of aerosols with respect to clouds (under, above)?

The sun photometer was on the aircraft (added to text).

Was their knowledge of the aerosol absorbing and scattering properties?

No, the aerosol instrumentation was unfortunately rudimentary. The aerosol type was set to the OPAC Arctic type (Hess et al., Bull. Am. Met. Soc., 1998). Added to the text.

What assumptions were made in the meteorological profiles above the level of the dropsonde?

Standard subarctic winter atmosphere, reference added to text.

What cloud scattering properties did you use and how were they developed (i.e. model for Mie scattering for water clouds, or what ice crystal model for ice clouds)?

Mie, as all particles were liquid (added to text)

You refer to the benefit of the off-track pixels with the hyperspectral imager data, yet do not (I believe) include relevant discussion or support for this statement. Please expand on this important point. For example, further discussion regarding the data shown in Figure 10, and implications of added potential information available from the hyperspectral imager for heterogeneous clouds.

More explanation of Fig. 10 has been added to the section "Retrieval Results", and a discussion of the benefits of hyperspectral cloud observations to the "Conclusions" section:

> Hyperspectral imaging was used to retrieve the cloud optical thickness in a 40° field of view across the flight track at 4 m resolution. This extends the application of the hyperspectral camera AisaEAGLE to airborne cloud research, and shows the potential of this rapidly developing technology to this purpose. Fig. 10 demonstrates how the two-dimensional cloud statistics become available with imaging spectroscopy. First, it allows us to compare the statistics along the flight track (as observed by a single radiance sensor, such as the SMART-Albedometer) to the statistics across the flight track. This will be particularly useful in the case of non-stratiform or undulating cloud fields. Furthermore, such a highly-resolved spatial distribution of the cloud optical thickness gives us the opportunity to feed a very realistic cloud field into a Monte Carlo model of radiative transfer. The spatial extend of the model domain can be increased by a meandering flight pattern to cover a large rectangular area (if the flight speed is faster than cloud evolution). Additionally, just imagine a single ice floe in the middle of Fig. 10. While the retrieval of cloud optical thickness would become more complicated, such radiance observations would be very valuable to study the horizontal propagation of radiation reflected by the ice floe in the cloud. In a similar fashion, cloud edges can be observed at high spatial resolution. With the added benefit of a full spectrum, imaging spectroscopy of clouds can also be a powerful tool to study adjacency and cloud-edge effects.

Page 7755, line 4; Instrument uncertainty and forward model assumptions occur with all remote sensing platforms (ground, air) and not just satellite. As it currently reads, it sounds like the air and ground-based measurements/retrievals do not have this problem.

Good observation. The corresponding paragraph has been rephrased as follows:

> Problems in retrievals of cloud properties from remote sensing arise from both instrument uncertainty and the assumptions made in the radiative transfer models used for the retrieval algorithms (Brest_1997, Marshak_2006). Thus, credible verification of these retrievals requires (i) direct comparison to in situ measurements; and (ii) comparison of spectral

radiances simulated by radiative transfer models to measured quantities (Formenti_2008, Barker_2011). This is how airborne observations can assist space-borne retrievals, as the aircraft can go back and observe what is inside and underneath the cloud that was remotely sensed.

Page 7755, line 27-30: I like that you have defined 'hyperspectral'. The given definition does confuse me a little as a hyperspectral imaging cube has two spatial dimensions (x and y) and a spectral 'z' dimension. The added time dimension you include would result in 4-dimensional dataset. Is this in line with your definition?

Please excuse the confusion. Of course our 'hyperspectral' data set is three-dimensional. The time dimension corresponds to the along-track spatial dimension, to which it is connected by $s = vt$. This is now clarified in the text.

Page 7758, line 11 and Figure 4: The figure and text would be easier to interpret if you added an altitude (km) scale.

Figure 4 does have an altitude scale, so I'm not sure where the confusion arises from. The sounding level 975 hPa that is mentioned in the text has been replaced by 400 m to make the scale clear.

Page 7758, Description of RT calculations: Please see request for more details (above) under the paragraph beginning, if the answer is 'b'

The description has been expanded, see above.

Page 7759, line 26 through end of paragraph: first, change 'reflectance' to upwelling radiance. I believe that is your measured quantity that you want to discuss; if you are normalizing by the downwelling please define as such in your paper. (Check for consistencies throughout paper to make sure you aren't bouncing back and forth between reflectance and radiance unless that is your desired intent).

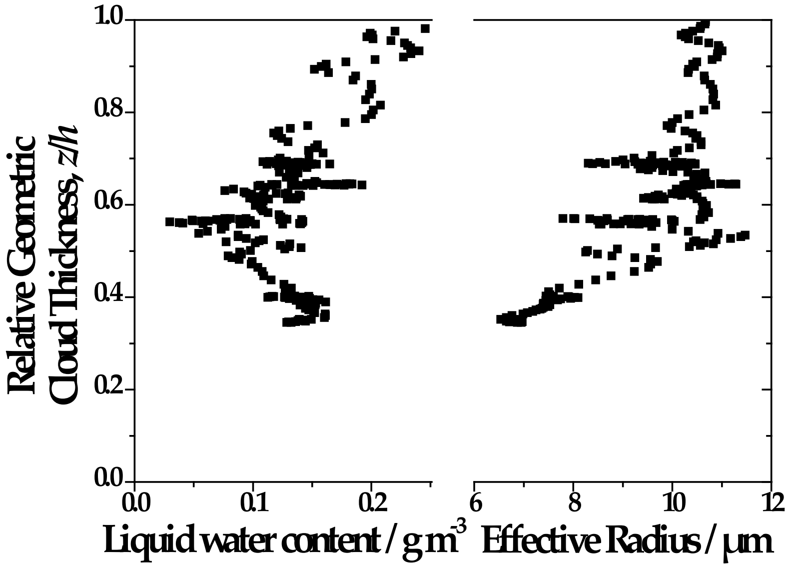
Correct, the intended usage is 'radiance' throughout the manuscript. Thanks.

Page 7760, line 1-16: Coddington et al. [2012] investigated the added information in adding retrieval wavelengths. They found, even for optical thickness, that information with additional wavelengths was gained due to a reduction in radiometric uncertainty. The improved knowledge in optical thickness, could propagate over into improved knowledge in *effective radius because, for clouds of optical thickness less than approximately 40, the lines of constant effective radius and cloud optical thickness are not orthogonal. For example, your figure 5 (bottom) shows larger uncertainty in optical thickness (from measurement error propagation) north of 75 degrees. Including a reference to Platnick, 2000 or similar would be suggested in discussing the change in cloud droplet size with height within a cloud. Both 2-wl and 5-wl would be expected to reach same penetration depth within the cloud, being that their longest retrieval wavelength is near 1600 nm. King and Vaughan (reference available above) use information content in the retrieval of the vertical profile of cloud droplet. In addition, what references can you provide that show in situ measurements can be used to validate remote sensing measurements?*

While a full implementation of the information content analysis and GENRA would be beyond the scope and time line for this manuscript, we included reference to Coddington et al. (2012) in the introduction, and we will keep improving our algorithms according to this very promising approach. As to the vertical size change, we have profile measurements at one location (75.8°N), see the figure below. The liquid water content was observed to increase linearly with height within the cloud, indicating adiabatic conditions, for the upper half of the cloud. At lower altitudes it seems to decrease linearly with a similar rate, although sampling is too low in that range to give that section too much weight. As the cloud bottom was right at the ocean surface, and the cloud was affected by the warm front advancing northward, the top and the bottom section of the cloud layer may have evolved differently. In situ measurements of the effective radius in the only profile that was flown indicate a quite constant effective radius in the upper half of the cloud. Based on that, we assume rather small errors in the retrieved particle size due to the particle size profile. This was included in the manuscript:

> The vertical profile of the cloud droplet size and its impact on remote sensing was studied, e.g., by Platnick (2000). For our case, the FSSP observations during the one profile flown at 75.8°N indicate that the liquid water content increased linearly (from 0.07 to 0.2 g m^(-3) in the top half of the cloud). The effective radius was rather constant for the top half of the cloud (10–11 um), dropping to 6 um in the lower half (about one third of the geometric cloud thickness; lower levels could not be sampled due to flight safety concerns). This indicates that contributions from different penetration depths would not skew the apparent effective radius as seen from above, as long as contributions from the lower cloud half are negligible.

As to your last question, in situ measurements are the most direct measurement of cloud particles, with other methods being other remote-sensing strategies, active or passive.

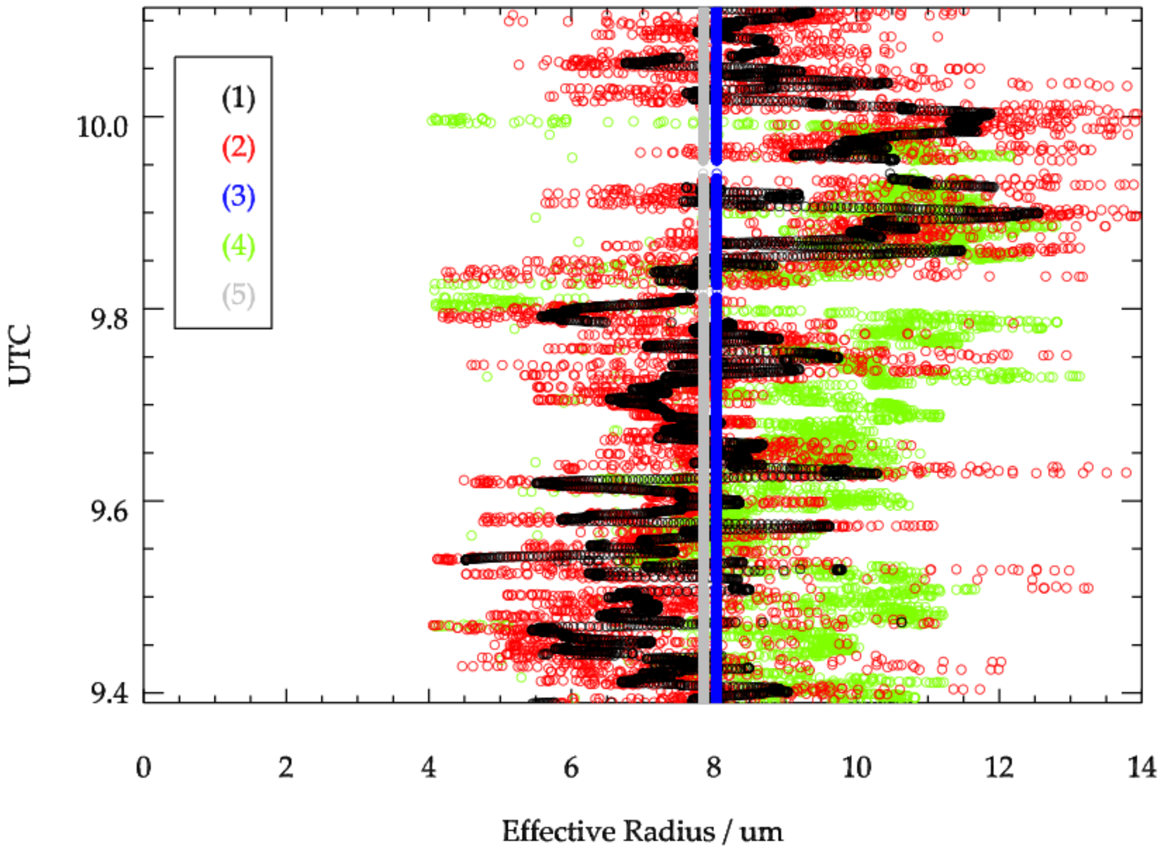


Lastly, please provide a more in-depth synopsis (perhaps add a cartoon or figure, or build on figure 4) showing a) the time delay between the remote and in situ measurements, b) the location of cloud, and location within/above cloud for remote sensing and in situ measurements.

The times and flight altitudes for remote sensing and in situ observations have been added to Fig. 4.

Page 7762, line 12: What maximum time difference do you allow between in situ and remote sensing measurements? In addition, with these different choices for determining effective radius, what is the size of the entire field? It would also be nice to know the spread/distribution in the different five options for effective radius.

The values of the effective radius, used a constraint for the retrieval, have been included in an additional plot:



Page 7763, line 5: It is difficult to understand why the in situ values of effective radius, when considered only at the same aircraft location of the remote sensing measurements, would be a poor choice, yet the average of all in situ effective radius over entire field would give such a good choice. Please expand.

While the other four choices yield distributions of optical thickness that are consistent to each other, this one produces significantly fewer low values (tau < 8). We therefore trust the other four more than this one, without being able to single out one of those four. In addition, the definition of choice #4 is the one where it seems most difficult (or unlikely) to encounter the very same conditions (the same piece of the cloud, if you want) during remote sensing and in situ measurements. While GPS makes it relatively easy to actually come back to the same location, we cannot assume that the cloud is still the same–has not evolved or shifted. Local inhomogeneities therefore become relevant, which they are not when average values are considered; and their influence would be particularly strong for thin cloud patches where entrainment might take place, where the cloud might be forming or dissolving in local updrafts or downdrafts.

The paragraph in question was rephrased as follows:

> The histograms of the entire field of optical thickness retrieved with the five different constraints of effective radius (Fig. 8) shows that the choice matters only when the optical thickness is less than 8. One choice, r_eff(4),

> leads to a significantly lower retrieval of those low optical thicknesses, while the other choices are consistent among each other. One possible explanation lies in the definition of r_eff(4) which implies that the cloud is still there and the same when it is probed the second time (the first time being the remote sensing, the second time the in situ flight leg). In the time between both visits, the cloud may have evolved or moved, and local inhomogeneities gain importance. Thin parts of the cloud (with low optical thickness) would also be particularly affected by local turbulence, which is in line with Fig. 8. Therefore, we conclude with the possible exception of r_eff(4) the choice of the effective radius has little significance to the retrieval of the cloud optical thickness.

Page 7763, line 20: Can you provide some indicator of spatial scale (lat/lon or km) on Figure 10?

Added to text (the depicted cloud is 2 km wide and 1 km long).

Page 7764, line 15 through 20: The discussion of the difference in histograms shown could be more developed. Are conclusions contradictory? For example, the shape of cloud property distribution of SMART albedometer is 'the same' as AisaEAGLE (12) yet on line 19, differences in distributions are attributed to off-track deviations.

We apologize for this oversight in internal review. The paragraph has been rewritten to describe the retrieval results more consistently:

> The histograms for the entire field of view and for the ES pixels do not differ significantly, which justifies the assumption that the cloud statistics are the same in flight direction and across (on the scale of the field of view). The distribution of optical thickness as retrieved from the imaging spectrometer is similar to that of the nadir values retrieved from the SMART-Albedometer radiance. While the AisaEAGLE has about 20 times more data points, the shape of the distribution is roughly the same as for the nadir optical thickness of the SMART-Albedometer. Only the ES pixels can be directly compared to the simultaneous retrieval by the SMART-Albedometer in nadir. A time series is shown in Fig. 12. The optical thickness retrieved from nadir radiance (red line) agrees within the grey-shaded range of uncertainty with the optical thickness retrieved from the ES pixels of the imaging spectrometer.

Page 7756, line 8: "Parts of the instrumentation. . ."; replace Parts with Some.

Corrected.

Page 7756, line 16: "The AisaEAGLE covers the . . ."; replace covers with measures.

Fixed.

Page 7756, line 25: replace 'fibre' with 'fiber'.

Fixed.

Page 7757, line 5: "have an own Inertial. . ."; replace an with their.

Fixed.

Page 7758, line 8: "On the aircraft,.."; replace On with From

Fixed.

Page 7759, line 26: The LUT discussion indicates gridded values are in radiance units. Please define reflectance that you are using, or perhaps change reflectance to reflected radiance, as reflectance typically means the reflected light has been normalized by incident light.

The term 'reflectance' has been removed from the text (see also the comment above).

Page 7761, line 19: The Use of the symbol :=, what does it add to the text? What do you lose by simply using the '=' symbol?

The symbol answers the questions 'Where does the superscript of *r*_eff come from?' as it is shorthand for 'The authors define the symbol on the colon side of the = symbol to be equal to the expression found on the opposite side of the = symbol.' Nevertheless, we understand that too specific mathematical symbols may be not universally recognized by a non-mathematical audience, and therefore removed the colons.

Page 7761: line 20 and 23: You have used the variable 'd' in two defintions. First as the width of the observed strip of cloud in AisaEagle's filed of view. Second, as the distance covered by the radiance spot on the cloud top.

It was our intention that these two quantities be equal. However, the wording was confusing. The paragraph in question has been rewritten as follows:

> Here, the averaging period Delta *t* is obtained as follows: First, the width *d* of the observed strip of cloud is determined from the height *h* between cloud top and the aircraft (from lidar) and the AisaEAGLE's field of view (alpha_E = 40°). Then the nadir effective radii r_eff^{S}(t) from the SMART-Albedometer are averaged over the time in which the radiance spot on the cloud top covered a distance that is equal to *d*, so
>
> *d* = *v* * Delta *t* + 2*h* * tan(alpha_S/2) ,

> with alpha_S being the viewing angle of the radiance inlet, Delta t the averaging time, and v the aircraft speed (ignoring any cloud motion with the assumption v >> v_cloud). The last term in Eq. X adds the radiance field of view behind and in front of the nadir point. Hence, the averaging time interval is [t – Delta t/2, t + Delta t/2] with
>
> Delta t = 2h* (tan(alpha_E/2) – tan(alpha_S/2)) / v.

Page 7762, line 2: Incorrect relational operator (should be « to support your assumption).

Corrected.

Page 7763, line 13: Include pointer to Figure 9 somewhere in the discussion.

Figure 9 has been removed, as its topic is better covered with the forward model error discussed in an earlier part of the manuscript (see above).

Page 7764, line 16: "that are more pronounced."; awkward end to the sentence allows for ambiguity in interpretation. Pronounced with respect to. . .?

This awkward phrase was deleted while editing the text for one of the above comments.

Figure 2: "marked with crosses at exemplary wavelengths."; Please explain as the data shown is comparisons of radiances at a single wavelength, 870 nm.

Wrong wording, thanks. Has been corrected, replacing 'wavelengths' with 'data points'.

Figure 7: I would suggest swapping the order of the plots, for consistency with Figures 4, 5 and 6.

Fixed.

Figure 12: I may have missed it in text, but are the results shown for 2wl or 5wl?

2WL (added to text)