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Atmospheric Measurement Techniques Discussions

Interactive comment on "Observation of Cirrus Clouds with GLORIA during the WISE Campaign: Detection Methods and Cirrus Characterization" *by* Irene Bartolome Garcia et al.

Anonymous Referee #2

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Referee Report on "Observations of Cirrus Clouds with GLORIA during the WISE Campaign: Detection Methods and Cirrus Characterization", by Bartolome et al.

General Comments

This paper analyses and characterises cirrus cloud occurences observed during a suite of scientific flights observing the limb in the infrared range with the GLORIA spectrometer. This paper is written with care and provide an in-depth analysis of the characteristics of cirrus clouds using two different retrieval methods, based on the extinction coefficient and on a cloud index, respectively. Although I am not sure about the validity of the approach neglecting the scattering in the radiative model, the clear quantifica-

tion of the expected biases is highly valuable and provides to the reader the necessary insight to make an own judgement.

Hence, I recommende the publication of this paper, after addressing minor issues.

Specific comments

L. 8, p.1: "cloud top bottom height": Do the authors mean: "cloud bottom height" ? Otherwise, what is the difference between "cloud top bottom height" and "vertical extent" ?

L. 9, p.1: What do the authors mean by "fraction of cirrus clouds" ? Is it an estimate of covered area referred to the global coverage ? Is it restricted to some altitude range with respect to the tropopause level ? Is there some time reference in terms of annual mean ? The estimate of 13 to 27 % is different from what is mentioned in L. 18, p.1: is it a number derived from the GLORIA measurements ?

L. 12, p.1: What do the authors mean by "unattached cirrus layers" ?

L. 17, p.1: Do the authors mean "one or banks of small white flakes" ?

L. 20, p.1: "due to the low temperature of their environment": I guess the scattering due to the presence of the cirrus is the main driver, and not the temperature: I guess an even cold, dry, cloud-free atmosphere is not equally absorbing than cirrus clouds in the same temperature conditions. Please clarify.

L. 21, p.1: "they influence the amount of solar radiative energy received": Is this statement not in contradiction with L. 19-20, p.1: "cirrus clouds are rather transparent to incoming solar radiation" ?

L. 21-22, p.2: "2% of stratospheric cirrus . . . 4-5% for MIPAS in middle latitudes": To which quantity do these percentages refer ? To the global cloud occurrence frequency ? on an annual basis ?

L. 35, p.2: What do the authors mean by "long light of sight" ?

L. 2, p.3: What do the authors mean by “nature of the cirrus” ? Do they mean the optically thin/thick or subvisible character, or the (microscopic) structure of the clouds, or something else ?

L. 14-20, p.3: There are some repetition of information provided in p.2. The authors might consider remove them.

L. 23, p.3: Except saving memory space, does such averaging present any advantage in better visualizing the vertical structure, by getting rid of the local variability ? Table 1, p.4: This table seems to provide properties of both detectors (Fourier-transform spectrometer and 2D detector). It might be important to specify which one is concerned by each property. For instance, are both instruments covering the same range 780-1400 cm-1 ?

Figure 1: The choice of map does not render very well the location of land, and specifically of countries of importance, like Ireland and UK. Has the colour code (e.g. with all range of blue colours at the level of the sea and oceans) any importance for the present study ? If yes, the meaning of the colour code and a colour map should be provided. If not, I suggest to change the choice of map to make the information of importance (I guess: the geolocation) clearer and more visible.

L. 4-5, p.5: It seems useful to give some more insight into what static stability is, by giving some value of the static stability for extreme cases and/or by giving some equations that would also be useful to explicit the temperature dependence mentioned in l. 6, p.5. The quantity N should be defined. This paragraph indicates that the the static stability is an important parameter, but it is not clear why, and for which purpose the stability of the atmosphere has to be computed.

L. 10, p.5: If possible, I suggest to add some reference in English providing a description of JURASSIC2 for the more general readership of the journal.

L.10, p.5 - l.5, p.6: In view of the very scarse documentation provide about JURAS-

SIC, I suggest to extend a little bit the description, and make it somewhat less cryptic. E.g., concerning the retrieval technique, what is the retrieved quantity, and from which quantity-ies ? If the inversion technique has some similarities with the one used for limb sounding measurements or any other techniques, this might be mentioned. A reference is needed about the Schwarzschild equation.

L. 11-12, p.6: What do the authors mean ? Cloud index and extinction coefficient are physical quantities, and not methods.

L. 16-17, p.7: “The difference (. . .) is 21%”: please specify the metric used.

L. 12-19, p.7: From this paragraph, it appears that the present case is a rather unfavourable case with respect to the study by Höpfner and Emde (2005). From runs on test cases that are not described, a quite large difference is found between the retrieved extinction coefficient using no-scattering approximation, and the retrieved extinction coefficient using single scattering (which is a simplified case with respect to the actual multiple scattering case). In the no-scattering case, quite high values are found for the most extreme cases (P5 and P95). From all these data, it is difficult to convince (at least the non-expert) that a non-scattering approach is sufficient. The authors might provide the values of the difference at 2 x the standard deviation (P16 and P84) or interquartile values or another useful metric. They also should specify how the test cases were chosen (using different typical cirrus cloud configurations ? Was the distribution of the different kinds of situations representative for real atmospheric conditions at the considered latitude?). Finally, an important and more convincing argument would be to provide an estimate of the uncertainty on all target macrophysical quantities and on the detected cirrus cloud fraction, possibly in function of the latitude.

L. 27, p.7: What do the authors mean by “elevation angle offset” ? A reference to a paper about the instrument might be useful.

L. 1-2, p.8: Should this error of 125 m be added to GLORIA’s 140 m-vertical resolution ? How does it affect GLORIA’s ability to detected small-scale structures mentioned in

§2.1 ? Caption Figure 3: “First filtering of optically thicker regions”: Do the authors refer to the aerosol contributions ? It would be useful to specify.

L. 12-13, p.8: “Above the clouds”: please specify the vertical range affected by this regularization effect. Which method is supposed to be affected by this problem ?

L. 18-19, p.8: A reference is needed for this choice of criteria for clear sky conditions. “CI always greater than 2”: What do the authors mean by “always” ?

L. 18-20, p.8: In which extend is this pre-selection coarse ? Does it miss cloudy conditions, does it identify too much clear-sky conditions as cloudy ones, or randomly fails to distinguish clear-sky and cloudy conditions ?

Caption Figure 4: “Clear sky profiles of the vertical gradient (. . .)” looks strange. Wouldn’t it be better to write “vertical gradient of the extinction coefficient in the case of clear sky conditions” ?

Figure 4: I guess the reason why the width of the scattering cloud is quite constant down to $\sim$8 km, and then rapidly increases with decreasing altitudes (linked afterward with the water vapour influence), is related to the presence of the tropopause level. The authors could usefully add some line representative for the tropopause level for the sake of clarity.

L. 23-24, p.8: Do the authors expect to miss many cirrus events by using this apparently very conservative criteria (following Fig. 4)? ‘ L. 28, p.8: PDF should be defined.

L. 30, p.8: Where is the value 2 x 10-4 km-1 coming from ? Figure 4 provides a value of $\sim$1.1 x 10-4 km.

L. 31-32, p.8: It would be useful that the authors provide here the estimates by Sembhi et al (2012) and bu Griessbach et al. (2020).

L. 32-34, p.8: Where are the estimates of the CI threshold and considerations about “low number of count shifts” coming from ?

L. 34, p.8-L.1, p.9: Similar to comment on L. 31-32, p.8.

L.3, p.12: It would be interesting to mention the percentage of profiles showing cloud occurrence detected by both methods, to see in which extend these methods have similar performance, or can be considered as complementary. This information would usefully complete the distribution shown in Figure 7.

L. 10-11, p.12: Is there any possible confusion between the occurrence of cirrus clouds and of polar stratospheric clouds ?

L. 14, p.12: Same as comment on L.3, p.12.

Caption Figure 7: The authors might consider adding, for Fig. 7c, that this plot is discussed in Section 4.3.

Figure 8: If the distribution shows the number of estimated CTH, I guess it is the same distribution as the distribution of detected cloud occurrences. The integral of this histogram should be about 100%, since all not considered values (values < 0 or > 6 km) are unrealistic, thus supposed to be at worst marginal. However, a first estimate gives a total of $\sim$36% in the case of the extinction method and even significantly less in the case of the CI method. What is wrong ? The estimates in L. 18-19, p.12 (31% [20%] of the clouds detected by the extinction [CI] method) look consistent with Figure 8.

Figures in supplement, L. 5, p.4: The supplement includes 15 figures seemly aimed at providing the data for each individual flight. However, flight 1 is missing, and flight 3, already illustrated in Figure 3, is duplicated. Is it what the authors want ? Also, the last figure, i.e. Figure S15, corresponds to the 16th flight although a total number of 15 flight is mentioned in L.5, p.4. This should be corrected for the coherence.

Caption Figure 10: “all cloud top heights (CTHs) for the extinction method”, for 10a and 10b; “with color code as equivalent latitude”.

Technical corrections

L. 2, p.2: “their detection”

L. 21-22, p.2: “4-5% for MIPAS at middle latitudes”

L. 4, p.4: “the data (. . .) were. . .”

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