

Interactive comment on “Cirrus, contrails, and ice supersaturated regions in high pressure systems at northern mid latitudes” by F. Immler et al.

Anonymous Referee #2

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This paper studies ice clouds and supersaturation in Lindenberg (Europe) during summer 2003. Ground-based lidar data are analyzed together with RH deduced from collocated radiosonde measurements, and ECMWF cloud fields. The methods used to analyze lidar, radiosonde, and ECMWF data are described. The results show cirrus and contrails occurrences, link between water vapour and cirrus/contrails, classification of cirrus and contrails, comparison of lidar data with cloud prognostic variables from opECMWF.

General Comment:

The topic of this study is interesting and important. The links between ice clouds, contrails, and water vapour in the upper troposphere need to be studied. The context

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is well described in the introduction. The remote sensing tool used is well suited: a lidar to detect cirrus and contrails.

At this stage, two points seem critical to me:

- the uncertainty on the relative humidity is not given. As a consequence, the impact of this uncertainty on the results is not discussed (it may be significant). It needs to be evaluated before the publication of the paper.
- the combination of lidar optical depth and ECMWF IWP to derive particle size and concentration is not fully convincing for instance (see detailed comments below).

I would suggest reporting this last section (combination of lidar optical depth and ECMWF IWP) in another paper, and focus this paper on the link between contrails, ice clouds and water vapour observations.

Detailed Comments:

Section 1

- The introduction is interesting, well referenced and described quite well the state of the art.
- Liou 1986 speaks about cirrus clouds in general but not thin cirrostratus in particular.
- The accuracy of humidity measurements by Vaisala-RS80 needs to be given in introduction, to convince the reader that this measurement is well suited to detect correctly supersaturation.

Section 2

- Give the typical footprint diameter of the laser spot and telescope on the ice cloud.

- Lines 20-25 page 1379 are not useful.
- Sect. 2, 4th paragraph: this typically gives an optical depth value of 0.0001 with an uncertainty of 0.0500. Does not sound correct ?
- In the same paragraph, last sentence: the retrieval of thin cloud optical depth in assuming S in thin clouds equal to S in thick clouds is not completely satisfactory, because S depends on the microphysic which can differ significantly between thin and thick clouds. The authors need to give the range of variability of S and the uncertainty induced by this variability on the optical depth value.

Section 2.1.

- This section is important because measurement of humidity in the upper troposphere is difficult to assess, and a large part of the results of the paper are based on the validity of these measurements. It needs to be improved and more quantitative.
- Is the paper by Treffeisen et al. accepted ?
- What is the uncertainty of humidity in the upper troposphere for RS-92 ?
- What is the uncertainty of RH derived from RS-80 after applying the correction algorithm ?
- Take into this uncertainty in the analyze (Sect. 3.2).
- The radiosonde balloon can fly far away from the lidar before to reach the altitude of the upper troposphere and cirrus. What means 'coincident lidar measurements' in the text. Does that refer to coincident in time or space? if it's in time, the distance in km between humidity and lidar data need to be discussed. How does that impact the results in the following sections?

Section 2.2.

Paragraphes 2 and 3 may be moved in the introduction.

It is confusing here, what is the objective of the paper?

Section 3

- The 1st IOP should not be mentioned (data are not used)
- The 'clear' term should be replaced by another expression like 'no low-cloud', here and after in the paper. This too confusing as it.
- Give the exact period of observations analysed : dates, number of days, number of hours, is it continuously (24h) day and night ?

Section 3.1.

- the word 'universal' should be removed, too strong : mid-latitude only and 2 papers is not universal.
- Is the exponential law really induced by geophysical properties or is it an artefact due to the method used to derive the OD: the maximum of the PDF corresponds to $OD=0.05$ which is the lower value for the first retrieval method and the higher value for the second retrieval method (given in Sect. 2).
- The sentence "The PDF of the OD in Fig. 1 can be simulated by assuming an exponential decay of spontaneously occurring ... model" is confusing. Needs to be clarified
- How are the "two clouds" cases identified. Does that correspond to 2 layers vertically overlapped, if yes what are the criteria (visual ? or typical distance between the layers?)

- 2nd paragraph: “isobars bends towards the high pressure” dose not sound correct.
- It is not clear in Fig. 2 that the “cloudiness was particularly high in the high pressure systems”. Maybe splitting Fig. 2 in 2 histograms (one for L and one for H) would help the reader.
- How are the 67% and 37% coverage obtained? the method should be better described

Section 3.2.

- The results given here are sensitive to the uncertainty on RH and the distance between the cloud seen by the lidar and the radiosond data. This needs to be discussed
- Paragraph 2: what means “By volume”?
- “The probability of encountering ISSRs is higher at 11-12 km the major cruising altitude of aircraft ... until the end of the paragraph”. It would be more convincing to discuss the contribution of aircraft vs. natural troposphere/stratosphere water vapour transition in showing a histogram of the tropopause altitude and another histogram of the probability to encounter ISSR as a function of altitude.

Section 3.3

- Fig.4: Add a cross at the location of Lindenberg in the upper panel to help the reader.
- Last sentence of second paragraph: Is the lidar less sesnitive than eye to detect clouds ?

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- Fig.5: legend and fig are not consistent 'sCi' and 'sCs'.

This section is quite qualitative (except Fig. 5), and leads to the conclusion that both cirrus and contrails require water vapour supersaturation conditions to occur. That sounds correct but not really new. A more positive way to present these results may be to exchange Sect. 3.3. and 3.2, and give more insight in the current Sect. 3.2.

Section 3.4

- Fig. 6. The white areas (no data?) in the upper panel should be white also in the lower panel (would be easier to compare)
- Why do the authors analyze only one week of data?
- The correlation coefficient (0.93) seems very good compared to the figure. Is that correct?
- Fig. 7 says 150 mic, whereas the text says 100 mic.?
- The value of the ice density depends on pressure and temperature. Is it taken into account in the computation ?

Retrieving the particle size and concentration from OD deduced from ground based lidar observations and collocated IWP simulated by ECMWF seems hazardous at that stage. If the equations given in the paper are correct and the approach is interesting, several uncertainties exist: the collocation needs to be carefully checked, cloud geometrical vertical thickness in observation and model need to be consistent, the uncertainty on the lidar OD has to be taken into account, it is also necessary to precise whereas the model cloud scheme contains (or not) some implicit hypothesis on the particle size or concentration or any other constrain that need to be properly considered in the retrieval. The method needs to be applied to a significant number of cases and

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the uncertainties need to be given in detail. Moreover several data set including in situ particle size measurements and lidar observations are available in the public domain to evaluate the method.

The effective particle size values obtained here for cirrus clouds ($r=100$ or 150 mic, give particle size of 200 or 300 mic) are very large compared to the in situ measurements available in the literature.

I would suggest to remove this Section of the paper and do additional work on this idea to submit a more complete and comprehensive study in a future paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13175, 2007.

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