Atmos. Chem. Phys. Discuss., 7, S5934–S5937, 2007
www.atmos-chem-phys-discuss.net/7/S5934/2007/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

Received and published: 12 October 2007

The authors have combined and analysed lidar and radiosonde data obtained during the summer 2003 heat wave at the meteorological station Lindenberg, Germany. In the focus of the study was the relation between ice supersaturation, cirrus clouds and contrails. The paper reports very interesting results that should be published if they prove correct. The results I mean are: a very high proportion of ice supersaturation in the corrected radiosonde profiles, the fact that they almost always contained cirrus clouds, and the finding that contrails are almost always embedded within cirrus clouds. The latter two findings are, of course, related.

1 Major comments

My biggest concern about this paper is the finding of ice supersaturation in more than 60% of the radiosonde profiles. A similar study by Spichtinger et al. (2003) with radiosonde data from the same location found supersaturation only in 28% of the profiles. One might argue that both studies have considered special periods in time, but I do not believe this is sufficient. To my view, a number of tests must be performed in order to substantiate these results. First, check whether the RS data are already provided with a correction applied, as it might be that the Lindenberg observatory applies the Leiterer et al. normalised frequency method automatically. Second, the RS data have been corrected with the Miloshevich method, while Spichtinger et al. have used Leiterer's FN method. It is absolutely necessary that your raw data are fed through the FN method, in order to see whether there are substantial differences between the two correction algorithms. Third, one could also look into the MOZAIC humidity data of Sept. 2003 to see whether there are indications of a enhanced ice supersaturation during that period.

My second big concern is of a more general nature, touching on the definition of a cloud. As the lidar is able to see very small optical thicknesses of less than 10^{-4}, it is justified to ask whether such a highly diluted ensemble of ice crystals should still be labelled a cloud. The authours have mentioned this difficulty in the paper, but I think that several statements they make do not properly reflect this difficulty. When they say that ISSRs almost always contain ice crystals and that contrails are almost always embedded in cirrus, it might be true, but I would like to see more differentiation in such statements. In particular I would like to see estimates of the probable ice number concentrations that are implied in such statements. It might be that half of their contrails are embedded in "cirrus clouds" with one crystal per m^3. I suggest that a table or figure be prepared that shows as a function of ice number concentration classes (or, optical thickness classes) the fraction of contrails embedded. Also for better judging the ISSR

ACPD

7, S5934–S5937, 2007

vs. cirrus relation I would like to see pdfs of the ice number and mass concentrations found with the Lidar. I know that the translation from optical thickness to crystal number densities is not unique. But one can make some simplifications. You could take the pathlength from the RS profile of from the Lidar profile, and if you then assume two different effective radii (10 and 100 μm), and furthermore identify the effective radius with the mean radius (which is of course incorrect, but for an estimate it might do) you can compute number and mass concentrations.

2 Minor comments

Page 13178, line 16: formation conditions of contrails are known very well.

Section 2: the first par should have a heading, e.g. "2.1.1. MARL"

P. 13180, l.9: How can optical thicknesses of 10^{-4} be measured when the uncertainty is 0.05? Needs better explanation.

End of Section 2: The new ECMWF model explicitly represents ice supersaturation (Tompkins et al., QJRMS, 2007), so some of the problems mentioned should be mitigated.

Section 3.1, 3rd sentence: This sentence does not belong to its current environment.

Equation 1 and associated text and Figure 1: Although I like such probabilistic games, I find that the one presented here does neither explain much nor is it convincing. First, when a function is smooth then so is its log, and vice versa. Second, the equation contains a plus-minus sign that I do not understand. Third, there should be transitional states between the two exponential lines. How do they look like? And what is the "integration time" in line 1 of page 13184? I suggest to delete this part of the paper.

P. 13184, l. 25: delete "climatological" because it is wrong.

ACPD
7, S5934–S5937, 2007

P. 13190, l. 14: check unit of effective radius.

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

ACPD

7, S5934–S5937, 2007

Interactive Comment