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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.

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We like to thank referee #1 for his comment on our paper. He has raised two major issues, the first of which is related to the reliability of the water vapour data used in our study. Water vapour was measured with Vaisala RS-80 radiosondes and a special correction algorithm was applied in order account for known problems of this sensor at cold temperatures. We have not discussed in detail why we used this algorithm because this was already done elsewhere and it is not the topic of our paper. However, we will demonstrate below that the data we have used are suitable for our study and that our conclusions stand on solid ground.

It is a well known fact that RS-80 humidity data is dry biased at cold temperature. To handle this problem different correction algorithms have been developed, the most well known are the one introduced by Miloshevich et al. (2001, MCA, hereafter) and by

Leiterer et al., (1997, LCA, hereafter). We have tested both methods by applying them to the raw RS-80 data and found that above roughly 8 km the MCA yields consistently larger humidities than the LCA. For our study we choose the Miloshevich correction algorithm for different reasons. One is, that the good performance of the MCA has been documented in a number of studies where RS-80 data were compared to the data from other instruments, including the NOAA frostpoint hygrometer, which is considered the reference instrument for this kind of measurements (Miloshevich et al.2004, Miloshevich et al.2006, Treffeisen et al. 2007, Wang et al., 2002 and references therein).

Further support for the MCA comes from a comparison with water vapour data obtained with our Raman Lidar. This instrument detects Raman scattering at 407 nm which is characteristic for the water vapour molecules. This kind of water vapour measurements works to about 8 or 10 km altitude with good accuracy but the statistical error rises rapidly at higher altitudes due to low signal. It is calibrated using the radiosonde at an altitude of 2-3 km, where the radiosonde data is reliable no matter which correction algorithm is used. A comparison of Lidar and radiosonde data using the MCA from 4-10 km altitude shows a good agreement (Correlation coefficient of 0.87). The LCA on the other hand shows a poorer agreement (Correlation coefficient of 0.73) and is clearly dry biased in particular at high humidities (RH$>$ 70%) when compared to the lidar results.

The third reason that convinced us that the MCA data is suitable for our study is the following: In a large number of cases the radiosonde data corrected with MCA yields values above 100% in the upper troposphere, while saturation was not reached with raw or LCA data. As we demonstrate in our study, these ISSRs (according to MCA data) correspond perfectly to our observation of cirrus clouds. The in-cloud distribution of the relative humidity shown in fig.3 of the manuscript was calculated based on the humidity data using MCA and the lidar observation of clouds. It agrees well with in-situ measurements of in-cloud RH demonstrating the good performance of the MCA. The discrepancy between our observations and the results published by Spichtinger at al.

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(2003) concerning the frequency of ISSRs in the upper troposphere is to some extent due to these differences in the correction algorithms. Spichtinger at al. (2003) used the LCA and therefore obtained lower frequencies of ISSRs than we observed. However, the lidar observations of cirrus and the MCA radiosonde data yield a consistent dataset where a high coverage with cirrus clouds (about 60%) corresponds to a high frequency of ISSRs in the upper troposphere. If we had based our study on the LCA or raw RS-80 data, there was a large number of cirrus dwelling entirely in subsaturated air. There is no convincing reason why this should occur, unless one doubts that the particles that the lidars detects in the upper troposphere are ice particles. This issue touches the second point brought up by the referee which was concerning the definition of cirrus clouds.

Cirrus clouds, from the lidar perspective, are layers of particles with a well defined upper and lower boundary and a large vertical and temporal variability, that depolarize light and have a close to zero colour index. The latter expresses the wavelength dependence of the backscatter coefficient. White clouds (color index =0) scatter equally efficient at all visible or near visible wavelengths. This definition holds for all clouds that were observed and evaluated for our study. The depolarization and colour index does not significantly change with the optical depths of clouds. As shown in the Fig.1 of our manuscript the PDF of the OD is a smooth function. There is no indication whatsoever from our observations that optically thin layers that fulfil the specifications given above differ substantially from visible cirrus clouds. Therefore we assume that optically thin clouds are composed of ice particles and we believe it is justified to label them clouds. The optical depth is certainly not a good parameter to distinguish between clouds (condensed water) and other types of particles. We have observed optically rather thick (about 0.1) layers in the middle and upper troposphere that do not match our cirrus definition, e.g. that do not depolarize light and/or have a significant non-zero colour index. Those layers were excluded from our cirrus study and were identified as either Saharan dust or biomass burning aerosol (see Immler et al, 2005).

Based on our comparison with the ECMWF data we showed that the ice particle number concentration does not change very much with optical depth and is of the order of 10 / liter for all stratiform cirrus clouds. The optical depth of the clouds is mainly a function of the effective particle size. Contrails seem to occur embedded in all kinds of cirrus, visible as well as subvisible clouds. The referee makes very interesting suggestions for a more detailed analysis. However, since the retrieval of particle size and number is associated with large errors, we don't think it makes much sense at this point to work these relations out in more detail. More information and a much larger dataset are required for such an analysis. Also the retrieval of number and size should be validated by in-situ measurements. A measurement campaign that focuses on thin 'high pressure'-cirrus and contrails with lidar and in-situ observation would be a very useful exercise in this respect.

We hope that this comment addresses the major concerns of the referee and convinces that our basic conclusions are well supported by our observations. Some points we have described here are not, or not clearly enough, worked out in our manuscript. We will amend it accordingly.

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