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Interactive Comment

Interactive comment on "Balloon-borne match measurements of mid-latitude cirrus clouds" *by* A. Cirisan et al.

Anonymous Referee #1

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General:

The paper presents balloon based observations of mid-latitude cirrus performed by the 'cirrus match technique' together with detailed microphysical modeling along air mass trajectories to explain the observed high supersaturations inside of a thick cirrus. The authors state that the measured supersaturations can not be reproduced, irrespective of the choice of meteorological or microphysical model parameters and conclude that either an unknown physical process or a measurement error can explain the observations.

The study is interesting and of scientific content that is in the focus of recent research in the field and is thus suitable for publication in ACP.

I have read the report of referee 2 and agree with his comments with respect to the depth of details described in the manuscript.

However, I do not agree with his comment that 'The manuscript is a very comprehensive presentation of the current understanding of cirrus microphysics ....'. Quite the opposite, my major criticism at the study is that the scientific frame the study is embedded in is not at the present state of the art and should be reassessed (see specific comments below).

Specific comments:

1) Abstract

'Observations of persistent high supersaturations with respect to ice inside cirrus clouds are challenging our understanding of cloud microphysics and of climate feedback processes in the upper troposphere.'

I recommend to reformulate this statement, since persistent high supersaturations are not yet observed in mid-latitude cirrus, only in very cold tropical cirrus. In addition, persistent high supersaturations in mid-latitude cirrus are theoretically understood and it is demonstrated by model calculations (e.g. Spichtinger and Gierens, 2009, ACP: Modelling of cirrus clouds – Part 2: Competition of different nucleation mechanisms) that the in-cloud supersaturation strongly depends on the number of available ice nuclei (IN) that freeze heterogeneously.

To my understanding (?) the goal of this study could be to provide measurements of the evolution of supersaturations in mid-latitude cirrus using the match technique and explain them by microphysical modeling since it is very difficult/impossible to observe the evolution of cirrus from aircraft.

2) Introduction, Page 4, line 11 ff:

'... very high in-cloud supersaturations are difficult to comprehend, since ice crystals should readily deplete the supersaturated water vapour leading to a fast decrease of

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supersaturation. Even though several potential theoretical explanations for persistent supersaturations have been alluded to, this phenomenon and the frequency of its occurrence remain difficult to understand (Peter et al., 2006, 2008).'

It is known that in case heterogeneous freezing occurs first and only few ice crystals form, the supersaturation decreases only slowly. In addition, in a new study of Cziczo et al.(2013, Science: Clarifying the dominant sources and mechanisms of cirrus cloud formation) it is shown from observations in mid-latitude cirrus in the US that heterogeneous freezing dominates the formation of cirrus, at least in the field campaigns investigated in this study.

I suggest to include both -the influence of heterogeneous freezing on cirrus evolution and the new findings of Cziczo et al. (2013)- into the paper.

3) 4.4 Microphysical analysis based on COSMO-7 trajectory fields without small-scale temperature ïňĆuctuations

and

4.5.1 Small-scale temperature fluctuations

The need of small-scale temperature fluctuations to reproduce the microphysical properties of cirrus clouds is already demonstrated in two publications coming from the same group of scientists as the actual manuscript (Hoyle et al., 2005, JAS and Brabec et al., 2012, ACP). I don't think that it is necessary to repeat this type of model study here - especially since it shows the same as the other studes, namely that it is difficult to reproduce measured cirrus properties without superposing small-scale temperature fluctuations to the air mass trajectories. I recommend to mention the earlier studies and remove the sections from the manuscript.

4) 4.5.2 Mass accommodation of H2O on ice

To my opinion this section is far too long. In addition, it is shown by an earlier model study which is comparable to this one (Gensch et al., 2008, ERL: Supersaturations,

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microphysics and nitric acid partitioning in a cold cirrus cloud observed during CR-AVE 2006: an observation–modelling intercomparison study), that the very low mass accommodation coefficient reported by Magee et al. (2006) can lead to unrealistic cirrus microphysical properties.

5) 4.5.3 Heterogeneous nucleation of ice

As already mentioned, the in-cloud supersaturation depends quite strongly on the number of IN (see e.g. Spichtinger and Gierens, 2009, ACP) with a higher number of IN can cause higher in-cloud supersaturations (homogeneous freezing might be entirely suppressed). Thus, I recommend to introduce one or two additional case studies with varying IN number. This would give a broader view on the possible conditions for the cirrus evolution and the statement that the observed supersaturations could not be reproduced by varying the microphysical model parameters would be more robust.

6) Table 4:

it would be desirable to see more model results than only backscatter, e.g. ice number and supersaturation.

7) Figures 9 - 12:

Only the model results including pure homogeneous ice nucleation are shown. I suggest to also show the results including heterogeneous freezing with varying IN concentrations.

I also suggest to show additional model parameters such as ice number and size (mass mean diameter).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 25417, 2013.