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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnosis of processes controlling water vapour in the tropical tropopause layer by a Lagrangian cirrus model" *by* C. Ren et al.

Anonymous Referee #1

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General

This paper describes a method to treat cirrus microphysics along trajectories, the Lagrangian air–parcel cirrus model (LACM). The purpose of the method is rather to get a better representation of the water balance along trajectories than available in the basic ECMWF vapour and ice fields. The method has been tested by comparison with measurements of total water on research flights in the TTL during the TROCCINOX campaign. Surprisingly, although the figures indicate that LACM total water is closer to the measurements than the interpolated ECMWF fields, objective statistics finds better correlation in the ECMWF data. So it might be justified to not publish the paper,

but I still find the method interesting enough to warrant publication. In any case, the presentation must be improved substantially, as detailed below.

Major comments

1) Treatment of sedimentation: If the assumed cloud is vertically homogeneous and without an assumption on size distribution (implying a distribution of terminal velocities), the vertical flux of ice through the trajectory should be constant until the ice that was originally at cloud top has fallen down to the trajectory altitude. Then the flux should become instantaneously zero. In the model there is some exponential law that seems to be inconsistent with the assumptions in the model. I need more explanation.

2) Choice of the cloud top: This could be a topic of a sensitivity study.

3) The three methods: A new subsection 2.5 should be written that introduces and explains the other two trajectory methods: interpolated ECMWF (relatively clear) and minimum saturation ratio (not clear).

4) Statistical indicators: This discussion is not very convincing. It looks as if the ECMWF interpolation performs better, even after introduction of Dev. It can also be noted that even the simplest method seems to reach quite high correlation coefficients. The comparison might be more convincing if it would not only look at the total water but also to the ice mixing ratio, for instance. But that would, of course, need validation with other instruments. The interpretation that a correlation coefficient of 0.76 means that more than three quarters of the variation is explained by the model is wrong. In fact, it is the sqare of the correlation coefficient that has to be used here, so only about 58% of the variation are represented by the models.

5) Comparison with MODIS data: This section detracts from the main theme in the paper. Is is also not convincing, e.g. "...this can not be confirmed by MODIS data...". So the section should be deleted.

Minor points

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6) some references in the text should be outside of brackets.

7) The first par. in sect. 2 contains unnecessary information since only FLEXTRA is used. So one sentence and one reference is enough.

8) Sect.3.1. last sentence in par 2: What is too dry here. I assume it is the TTL in the model, not the TTL itself. Please clarify.

9) Sect.3.1. last par: I could not catch the meaning of this text. Should be rewritten.

10) 2nd but lat line on page 5526: “numerical diffusion”, in ECMWF or in LACM?

11) First par on page 5527: Check figure numbers.

12) First par on page 5531: It should be noted that the ECMWF model of 2005 was not able to reproduce ice supersaturation.

13) First sentence in Conclusions: what is meant with “conservation” here. Since there is dehydration and rehydration, there is no conservation. Please clarify.

14) Some figures are too small and should be enlarged in a revised version.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 5515, 2007.

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Interactive Comment