Atmos. Chem. Phys. Discuss., 7, S3624–S3631, 2007
www.atmos-chem-phys-discuss.net/7/S3624/2007/

Atmospheric Chemistry and Physics Discussions

ACPD

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Interactive Comment

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

C. Ren et al.

Received and published: 2 August 2007

We thank the referees for their constructive criticism of our manuscript, and address the points raised in their reviews, in turn, below.

Both referees have requested further details regarding our sedimentation scheme, and we have changed the text to make our description clearer. Our modelling framework is that of a single Lagrangian air parcel. Using a single box undoubtedly makes the realistic incorporation of sedimentation difficult, because of the issues identified by the referees. Other studies have used 1-D (height vs time) formulations in order to incorporate sedimentation. However, using such a 1-dimensional 'curtain' is also unrealistic, because of wind shear. If the modelling is to be carried out along trajectories, the choice, then, is between (i) an assumption of vertical homogeneity from box to cloud-top, and (ii) an assumption of horizontal homogeneity on either side of a 'curtain'. Having chosen a single-box trajectory formulation, sedimentation out of the box will naturally be a first-order process, leading to an exponential function of cloud particle number with respect to time. However, we have sought to minimise the error in this treatment of sedimentation by considering that the number of particles in the box will decrease quickly near the top of a cloud and slowly at the base of a cloud. Our problem then becomes one of estimating a cloud-top reference, and we explain how we go about this in the text. In summary, then, our approach to sedimentation is a natural consequence of our choice of a Lagrangian framework, but takes account of information about cloud depth contained in the trajectories themselves.

1 Referee 1

The general remarks of Referee 1, particularly

"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 ... 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,"

have prompted us to look again at our description of the aims of this paper, and we have now clarified the text, particularly the last paragraph of the Introduction, which now opens

"In this paper, we describe the model we have devised to diagnose the processes which control water vapour in the TTL, and we give case-study results. Our diagnosis is made by pair-wise comparisons between four sets of results: the new model, the underlying meteorological anal-

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ysis driving the model, the simplest possible treatment of water vapour removal along trajectories, and in-situ total water measurements."

That is, the purpose of the paper is, first and foremost, to diagnose the processes which control water vapour in the TTL. We believe that the paper has achieved this goal, demonstrating the importance of (i) transport along trajectories, (ii) dehydration by cloud particles, and (iii) rehydration by convective injection. In carrying out this diagnosis, we have demonstrated that the LACM reconstructions can reproduce features of the observations (i.e., 'stripes' of anvil outflow, supersaturated air masses) that are not in the ECMWF analysis. We also show, in what we believe is an important subsidiary conclusion of the work, that the ECMWF analysis captures well much of the behaviour of water in the tropical tropopause region. To address the referee's specific comments:

1) See our comments on sedimentation, above.

2) See our comments on sedimentation, above. The only other choice open to us, would be to use a fixed cloud depth. Since the cloud-top assumption described above is conceptually more realistic, we don't compare these two choices.

3) Accepted. A more detailed account of the minimum-saturation-ratio method has been added.

4) Although our goal was to diagnose processes controlling water vapour in the TTL, rather than to conduct a competitive model-validation exercise, our initial visual inspection of the results (Figures 6 and 7) suggested that it was useful to show some objective statistics of model-data comparisons. The statistics show that all three methods capture the general behaviour of water vapour in the TTL. Our method is consistently better than the other Lagrangian method, and sometimes better than the full ECMWF analysis. When our model does out-perform the ECMWF system, it is because LACM includes important processes controlling TTL water vapour that are not addressed by ECMWF (i.e., cirrus cloud evolution in the TTL). We think that the comparison of Figures 2, 4, and 5 is particularly telling in this regard; the statistical measures provide a

more detailed appraisal of the merits and demerits of the respective modelling methods. With respect to the interpretation of the correlation coefficient, we apologise for our slip: "the variance" has been changed to "the standard deviation".

5) We believe that the comparison with MODIS data is appropriate, as it is a demonstration – against an independent data source – that we have chosen the right processes in our model. Some consideration of phraseology is needed, and we have re-written the text accordingly. Section 3.3 on page 5533 (previous version) now reads: “MODIS retrieval is not available over lands for comparison”.

6) Accepted.

7) We apologise for a leftover, which misleads the reader. The misleading sentence ‘Trajectories used in this paper are all from FLEXTRA runs’ has been deleted.

8) We have clarified the last sentence of paragraph 2 in section 3.1. It now reads “Lagrangian models are likely to simulate air in the TTL with dry bias”.

9) This comment is about what initial values should be given to trajectories starting in the stratosphere for the instantaneous dehydration method. The paragraph has been rewritten to clarify.

10) We have clarified this point. The penultimate sentence on page 5526 (previous version) reads “numerical diffusion in ECMWF” in the new version.

11) We have checked and corrected the references to figures on page 5527 (previous version). We apologise for the confusion caused.

12) Theoretically, according to the relevant documents, the ECMWF analysis cannot produce ice supersaturation. However, we found that the ECMWF model of 2005 did produce a small amount of ice supersaturated air (see figure 7 of the manuscript). This could not be because of the minor difference in water vapour saturation pressure w.r.t. ice used in ECMWF and our study. Our values of saturation pressure are quite close to, but not smaller than, those used in ECMWF according to Simmons et al. (Q. J. R.

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Meteorol. Soc. 125, 353-386, 1999).

13) We have clarified the first sentence in the Conclusions. It now reads: “In this paper, a Lagrangian air-parcel cirrus model (LACM) is developed to describe the processes controlling total water in the TTL”.

14) Accepted. We apologise for difficulties in reading figures in the previous version and hope that we have corrected this to everyone’s satisfaction in this version.

2 J. Nielsen

We agree, of course, with the referee that this is a legitimate exercise, trying to simulate the major processes which control TTL water vapour. We would like to keep trying other ways of validating our model. However, we feel our results are encouraging, rather than discouraging, in that our model results are in agreement with flight observations and satellite observation, meaning the model captures the major responsible processes, and that the ECMWF analysis did surprisingly well.

To major comments: **P 5519 L 17-21 (previous version)** We agree that the vertical velocity, hence the cooling rate, controls cirrus nucleation. The precise influence of vertical velocity on results from our cirrus parameterisation has been discussed in Ren and MacKenzie (2005). In this paper, the vertical wind from ECMWF has been used directly in generating trajectories with FLEXTRA. Uncertainties regarding vertical wind fields are, of course, substantial, and we have added sentences acknowledging this in our revised manuscript, which read,

“In generating trajectories, the vertical velocity from ECMWF is directly used. Although the model-output vertical velocity fields, especially the assimilated fields, are noisier than the diabatic-heating-driven vertical velocity fields, this should not be a problem, as the noise produces effects similar to those produced if sub-grid vertical velocity fields were included. The precise influence of vertical velocity on results from our cirrus parameterisation has been dis-

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cussed in Ren and MacKenzie (2005), and the sensitivity of trajectory models to vertical transport method discussed in Harris et al. (2005)."

To our knowledge, no data from TroCCiNOx was assimilated into the ECMWF analyses that we used. The analyses assimilated, therefore, the standard set of sonde, civil aircraft, and satellite data in this region; in general, the southern hemisphere and the tropical upper troposphere are data-poor regions. We have added a sentence to say that SSM/I total water vapour of satellite observations has been assimilated into the ECMWF analysis.

P 5520 L 20 (previous version) In calculating τ_q, we assume spherical particles and that the accumulation of latent heat in the particles is negligible, which is a good assumption for tenuous clouds in the tropical upper troposphere. Superficially, it appears that we have oversimplified the calculation. However, a negative feedback mechanism exists between consecutive steps. The feedback diminishes the error introduced in the previous step, meaning that results are not sensitive to the subtle inaccuracy in τ_q.

P 5523 L 5 (previous version) For the comment, please see our note on sedimentation, above.

P 5524 L 16 (previous version) Our intention in implementing a rehydration scheme is not to enable the model to capture the TTL composition close to individual convective clouds, but rather to capture large increases in water in the TTL - i.e., the effect of large areas of convection (whether those areas are mesoscale convective systems or unorganised tropical convection). Neither model can be expected to capture the variability close to convection, as measured, e.g., on the "Golden Day".

P 5530 L 2 (previous version) eq. 15. The standard form of the correlation coefficient is dominated by the very large change of water with height in the upper troposphere. We wished to develop a statistical metric that put more emphasis on the variability of water at a given altitude, since we are more concerned about the subtleties of water's behaviour close to the tropopause than its general exponential dependence on altitude.

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We believe eq. 15 is an appropriate metric, therefore, for the very reasons that the referee describes. We have inserted a sentence to make clearer our intentions in using eq. 15: “This indicator emphasises on smaller values of qt, and is hence useful in measuring which method is better for estimating the entry-level of stratospheric water vapour.”

P 5532 L 12 (previous version) Dr Nielsen is correct: the instantaneous scheme does not include rehydration. Note that the difference between the LACM results in Figs 6 and 8 is that there is no rehydration in the LACM runs for Fig 8 (we have now made this explicit in the caption text for Fig 8). The primary purpose of Figure 8, therefore, is to demonstrate the importance of including a rehydration parameterisation. One could calculate water fields using instantaneous dehydration and our rehydration scheme, but we prefer to show results from a simple instantaneous-dehydration-only scheme, since this is what has been used in previous studies (e.g. Bonazzola and Haynes, 2004; Fueglistaler et al., 2004, 2005). It is not especially useful to think of the effect of the rehydration scheme as “smearing” the water signal, since it fills in minima, rather than moving both extremes towards a central value.

P 5533 L 5 (previous version) We agree with Dr Nielsen that further comparisons against satellite data sets would be beneficial, but it is simply the case that these very recent papers came too late to be used in our work. Section 3.3 provides a global context to our work, gives firm qualitative support to our approach, and shows regions where the rehydration process – hitherto neglected in Lagrangian studies – is important.

P 5535 L 2 (previous version) The conclusion that the temperature history of the air parcels is not accurate, in the central part of the sortie on 5 February, does not depend on our statistical measures or cirrus scheme; it is the conclusion we come to by elimination of other possibilities.

p 5524 l 3 (previous version) "saturation ratio 0.8": This number does indeed not

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influence the results.

p 5527 l 10 (previous version) We have corrected the figure citations - our apologies for the confusion.

p 5528 l 18 (previous version) We have amended this sentence, which now reads "The 5th of February is chosen for a case study, because the model-measurement comparison on this day is the worst for all except the 'golden day'".

p 5542 (previous version) We apologise for difficulties in reading figures in the previous version and hope that we have corrected this to everyone's satisfaction in this version.

p 5548 (previous version) fig 7 We are also puzzled as to how the ECMWF interpolations can show saturation ratios above unity. Nevertheless, these are our results. See also our comment (12) to referee 1, above.

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