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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric dryness" *by* J. Lelieveld et al.

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We are grateful to Referee #1 for many comments and valuable suggestions, which help improving the manuscript. Ref#1 criticizes that our conclusions are not compelling enough and to some degree speculative. The revised version of the manuscript will more strongly highlight the new scientific findings based on our modeling results, and add arguments to reduce the level of speculation. We will include additional references to separate more clearly the interpretations from the literature and the present work.

In our defense I would like to mention that several recent studies based on GCM results (notably ECMWF analyses) have addressed transport pathways and temperature histories within the TTL. Apart from the fact we are using a different model, there may be some overlap with these studies. However, the actual dehydration mechanism has not been discussed in much detail because it is difficult to retrieve from ECMWF analyses.

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In some studies dehydration was recalculated from ECMWF moisture and temperature analyses. We are the first to describe GCM simulations of thin cirrus forming within the TTL in regions with frequent cumulonimbus occurrence, and we discuss the interplay between dynamics, convection and radiation processes.

In the revised manuscript the analysis will be strengthened by presenting radiative heating rate calculations and transport fluxes across isentropic surfaces. We feel this comprehensive view is new even though aspects of TTL transport and dehydration have been published previously. Albeit that ice cloud formation and crystal sedimentation are parameterized in the model, we show the role of thin cirrus in dehydration, and explain in more detail than previous GCM studies how the freeze drying depends on temperature.

The discussion of QBO and SAO is relevant for the annual and inter-annual variability of stratospheric water vapor and the vertical propagation of the tape recorder signal. It is appropriate to show that the model realistically reproduces these phenomena whereas most models have problems in this respect (see Eyring et al., 2006). I agree though that the relation between QBO and SAO is far-fetched in view of the current discussion, and will be scaled back in the revision (and removed from the conclusions). We will emphasize more strongly the differences with previous work.

In recent literature much attention is devoted to TTL supersaturation. Our model does not simulate supersaturation but nevertheless seems to produce realistic TTL clouds. Possibly, cloud formation in terms of the amount of condensate may not be critically dependent on the level of supersaturation. However, the microphysical details are influenced by ice nuclei abundance, i.e. how the condensate is distributed over the particles. This implies that the cloud microphysics and ice sedimentation rates are sensitive to changes in aerosol particles. We will emphasize this new angle in the revision.

I acknowledge the lighthearted use of the terms “fountain” and “drain”, and we will correct this in the revision. By calculating isentropic transport it is possible to distinguish the diabatic (radiation) from adiabatic (dynamics) components. Therefore, we are currently performing cross-isentropic flux calculations. Nevertheless, it is quite customary to describe vertical transport across isobars, and by also presenting the net radiative heating rates (planned for the revision) it will be easier to show the role of local radiative tendencies in vertical transport.

Detailed comments:

p.11292, l.2: The reference to Mlynczak et al. (1999) was based on their figures of vertical heating profiles. Indeed, CO_2 should also be mentioned, which will be corrected in the revision.

p.11249, l.8: The remark about the moistening tendency within the stratosphere may be confusing in view of the role of the lowermost stratosphere. It will be removed in the revision.

p.11249, l.20: In the revision we will mention both the discovery and the explanation of the hygropause more clearly within a historical context.

p.11249, l. 22: In the revision of the introduction we will emphasize dehydration in convective processes rather than convective overshooting, as suggested.

p.11251, l.1: In the revision we will refer to a pressure gradient (rather than force) and circumpolar flow (rather than vortex), and avoid the discussion of forces.

p.11251, l.20: This will be rephrased to avoid the misleading implication.

p.11252, l.9: The sentence will be adjusted accordingly.

p.11255, l.26: The confusing sentence will be removed.

p.11258, l.5: The QBO phase evolution in the model is quite close to observations. We will state this explicitly in the revision.

p.11258, l.8: We will remove the sentence about the graphics tool.

p.11262, l.14-21: Since we have not performed comparisons of temperature averages and climatologies between model and satellite data, we cannot answer this question. We argue that such averages can be biased by sampling procedures, and that this can be avoided by point-to-point comparisons, as we have performed. One important problem is that water vapor observations may be biased by cloud contamination, most strongly in the lower stratosphere, which limits the number of data points in these regions. We are the first to show satellite observations of very low water vapor (1-2 ppmv) in the tropics, as reproduced by our model. Obviously, if the point-to- point comparisons are satisfactory, the comparison with long-term data sets should also be favorable. Our strategy has been to use the nudging to represent actual conditions as realistically as possible so that limited satellite data sets can be used most efficiently. The model comparison of temperature with MIPAS data indicates good agreement for the actual state of the atmosphere, including spatial and temporal variability in the period considered. We are in the process of extending the comparison to AIRS data and hope to include these in the revision. The sentence “We interpret it as confirmation that the model realistically simulates dynamic and radiation processes” seems fair to me. We will see to what extent it holds up after comparing with additional satellite data.

p.11262, Fig 6: We have performed the correlation calculation for zonal mean water vapor and temperature only, which will be specified in the revised manuscript. Indeed, it will be interesting to study regional differences. In the revision we will recommend such studies by using simultaneous water vapor and temperature measurements from satellites (e.g. MIPAS, AIRS), as well as model results.

p.11264, l.7: This sentence will be deleted from the manuscript. I highly appreciate the dynamics view by Ref#1, in particular because this is a weakness in our team. Nevertheless, a rapid cooling as a result of dynamics, as indicated by the reviewer, must exclude the large-scale dynamics because it is very slow in the TTL. Interestingly, the TTL may be conceived as a region where direct influences of convection, large-scale

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dynamics and net radiative heating/cooling are all weak. Therefore, if one of these influences changes regionally (e.g. the net heating term changes sign) the effects on transport may be significant. We have not analyzed the role of Kelvin and gravity waves (mostly owing to a lack of expertise). It was my impression that the influences of these waves are larger at higher altitudes.

p.11265, l.14: In the revised manuscript we will clarify our hypothesis of the moistening influence by overshooting convection in greater detail. We have deduced it from the work by Alcala and Dessler (2002) and our own results, albeit that our model does not reproduce the deep convective intrusions. In section 6.3 we will add the following text: "We hypothesize that the ice particles formed in the deepest convective outflow are less likely to grow into the size range needed to escape the TTL. Firstly, the ambient air in the upper TTL is already relatively dry, and secondly the particles would have to grow to relatively large sizes to fall a large distance and leave the TTL. Since transport of the particles is both vertical and horizontal, the likelihood that they meet subsaturated conditions is relatively high. Thus ice particles formed in the outflow of the deepest convection are the most likely to evaporate within the TTL".

p.11265, l.24: In the revision we will add that the hygropause is caused by the upward transport of the water vapor minimum in the tape recorder, and refer to Kley et al. (2000).

p.11266, l.7-12: In the revision we will change the formulation into "the ascent through the TTL is accompanied by net cooling, which maintains a high relative humidity and sustains water condensation". The subsidence, at least partly driven by radiative cooling, will be illustrated in the revision by showing net radiative cooling e.g. over the tropical Indian Ocean in summer. We will change the formulation into "If the radiative cooling would be stronger it could balance the wave driven ascent and even contribute to subsidence" thus leaving room for a dynamical explanation. A similar point was raised by Fueglistaler and Fu (2006), and we will add this reference.

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p.11266, l.23: The thin tropopause cirrus indeed coincides both spatially and temporally with maxima in deep convection and anvil clouds in the model (also with convective precipitation). The convection and cirrus cloud maxima in figure 12 also coincide with observed convection (check e.g. ISCCP or the NCAR cloud atlases). Further upward (100-80 hPa) these patters maintain coherence (e.g. maxima over the Indo-Pacific region, Africa and S-America in January, and in addition over SE Asia in July.

p.11267, l.17: Unfortunately, figure 11 is not very clear because it was compressed onto 0.5 page. This should be improved in the revision, since it contains much interesting information about the temporal relationship between temperature and humidity. The remark on l.17 is supported by the results presented in figure 11.

p.11267, l.27: This has been deduced by comparing the temperature profiles in our model results, which we will mention in the revision.

p.11268, l.24: The model results indeed suggest such a link, however, I am not sure if this is consistent with observations, which are rather scarce. In China the monsoon rainfall seems to have changed little in the 1990s. This will need more research, and in the revision we will reformulate into "On the other hand, much of the inter-annual variability, as also displayed in Figure 1, coincides with water vapor anomalies in the outer tropics, especially in the NH, suggestive of a link with the Asian monsoon".

p.11267, l.4 (not l.9): We have computed the vertical fluxes of water in three phases, and note that the upward transport of ice is very small. This indicates that in the model the TTL ice clouds are mostly a result of in situ formation. We will reformulate this in the revision. We acknowledge, however, that a more detailed analysis e.g. of isentropic transport will be helpful to distinguish diabatic processes.

p.11269, l.10: In the revision we will mention that although we have not used our present model for the period directly influenced by the Mt Pinatubo eruption, in prior studies with the previous model version (Steil et al., 2003) we discovered that the enhanced flux of water into the stratosphere is a consequence of radiative heating by

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SO_2 and sulfate aerosol particles in the TTL, which allows more water vapor to enter the stratosphere. Further, we will remove the misleading remark on the efficiency of the dehydration process.

p.11271, l.8: As indicate above, in the revised manuscript we will remove the discussion of fountain and drain regions. In the revision we will add figures to show model calculated net radiative heating rates and vertical fluxes in pressure coordinates. In cases where upward or downward vertical fluxes have a high degree of correspondence with net heating or cooling, respectively, it seems reasonable to assume a diabatic component. We are in the process of re-calculating the vertical fluxes across isentropes, which will support further analysis.

p.11272, l.3: In the revision we will remove the conclusion about a link between the SAO and QBO.

p.11272, l.17: In the revision the discussion/conclusion on the role of convection in the dry bias will be refined. I cannot answer the question about the influence of convection on temperature (the model temperature comparing favorably with MIPAS and AIRS data). I assume that the convective intrusions are rare enough and the water vapor bias is small enough (10-15%) to have negligible influence on temperatures at 70-100 hPa.

p.11273, l.29: The role of the monsoon has been discussed in previous studies, partly suggesting a bypassing of the regions of dehydration. If this were true then tropospheric water vapor mixing ratios should occur in some regions of the lower stratosphere, which is not observed. Some studies mention a bypassing of the coldest regions, implicitly referring to a relationship between temperature and dehydration rather than explaining the process. Bonazzola and Haynes (2004) make the distinction of convective and non-convective dehydration. Our study goes a step further by describing the mechanism and concluding that monsoon desiccation occurs similarly as in the inner tropics through thin cirrus formation and ice particle sedimentation, however, that

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this process is less efficient at higher temperatures. We will remove the word “bypassing” to avoid confusion with earlier work.

p.11274, l.8: The comment about volcanic eruptions in the conclusions will be removed in the revision.

Jos Lelieveld

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11247, 2006.

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