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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General comments:

The study of Lelieveld et al. presents results of a model simulation with the atmospheric chemistry GCM E5M1 focusing on stratospheric water vapor. The simulation covers the years 1996-2005 and applies a model nudging technique, assimilating ECMWF operational analyses within the troposphere. The analysis is based on a comparison of simulated water vapor and temperatures with different observations and aims at gaining new insights into the dehydration mechanisms in the TTL.

As I have already mentioned in my referee quick-report, I think this paper does not provide any substantial new insights into the dehydration mechanisms in the TTL. According to my opinion the paper is a mixture of model validation and process studies, but both aspects are presented in a sketchy way. With respect to model validation or model development the paper lacks scientific originality. Indeed, the study applies the nudging technique, but the benefit of this approach does not become clear. For example, model nudging is not necessary to simulate a QBO or tape recorder signal in stratospheric water vapor. With respect to process studies I have some doubts that a GCM is the appropriate tool. Due to the model resolution several sub-grid processes have to be parameterized in a GCM, e.g. convection or cloud micro-physics. The parameterization are just as good as our knowledge on the processes which have to be parameterized. The authors themselves state that "for the TTL the picture is less unambiguous" (p 11252). The presented model results and comparisons with observations are not enough to convince the reader that the role of deep convection and tropopause cirrus for dehydration in the TTL is realistically captured.

I think the authors should focus on a specific scientific question which is appropriate for this kind of model simulation. However, this probably requires a complete review and re-structuring of the current manuscript which means more or less to write a new paper. Therefore, I suggest to reject the submitted manuscript, although I think that the model simulation provides several interesting results, e.g. with respect to the model nudging approach and the possibility of direct comparison with observations, which could be worth publishing later on.

I detail below some additional major points:

- A general problem of this manuscript is that the conclusions seem to appear from nowhere. For example: The authors attribute the simulated dry bias to a lack of convective penetration deep into the TTL. That might be the case, but in a further step they come to the reversal conclusion that deep convection moistens the tropical lower stratosphere. I can not find any indices for this conclusion in

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the paper. Statements like “this suggests” (p 11265, l 14) or “If correct,” (p 11272, l 21) show the uncertainty of these conclusions.

- The study is based on the period 1997-2005. The first year of the model simulation (1996) is omitted, because it is too close to the initial conditions (p 11258). However, looking at Fig. 2, one year spin-up time seems to be not enough. Stratospheric water vapor (above approx. 20 hPa) seems to decrease continuously in the model simulation, at least until 2000/2001. In the HALOE data a similar decrease is not apparent. The reason for this behavior has to be identified and at least discussed.

- Several observational data sets indicate a sudden decrease of stratospheric water vapor after 2001 (e.g. Randel et al., 2006). From Fig. 2, it is not clear whether the model reproduces these extreme low water vapor values after 2001 (a plot showing water vapor anomalies at a certain pressure level, e.g. 80 hPa, see Fig. 1 of Randel et al. (2006), might be useful). However, I think it is indispensable to show that the model reproduces this sudden drop in stratospheric water vapor. Since the simulation applies the nudging technique, I would expect that the model is able to reproduce such striking observations. Otherwise, I have strong doubts that the model captures the dehydration mechanism realistically.

- Trajectory calculations using ATTILA:
  Reithmeier and Sausen (2002) applied the Lagrangian transport scheme ATTILA in the ECHAM4 GCM with a horizontal resolution of T30 and a vertical resolution of 19 levels, the top model layer centered at 10 hPa. For the given resolution the model atmosphere was divided into about 187 000 air parcels. In the present study a middle atmosphere GCM (top layer centered at 0.01 hPa) with 90 vertical layers and a horizontal resolution of T42 was applied. Here the model atmosphere is divided into about 1 700 000 air parcels. An important question using this kind of transport scheme is whether the air parcels remain well distributed

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throughout the integration period or accumulate at certain places. Reithmeier and Sausen (2002) showed that the trajectories remain well distributed in their model runs with the reduced resolution. Is this conclusion also valid for the middle atmosphere GCM E5M1? I could imagine some problems with the upper model layers becoming devoid of air parcels. Furthermore, it would be interesting to see some results of comparison with the trajectory model of Stohl and Trickl (1999) using ECMWF data. Unfortunately, the study of Traub (2004) is only available as PhD thesis.

As stated by Reithmeier and Sausen (2002) ATTILA does not consider vertical transport of air parcels due to sub-grid scale convection, but only due to large-scale winds. ATTILA considers convective tracer mass fluxes as calculated by the ECHAM convection scheme, but the location of the air parcels is not affected by convection. How is it possible to calculate meaningful trajectories and transport paths without convective transport of the air parcels?

It is not clear to me how the vertical air mass fluxes across certain pressure levels are calculated. Are the mass fluxes calculated from the ATTILA trajectories? Or are they calculated from the residual circulation stream function as described in Rosenlof (1995)? In the latter case, the use of ATTILA would not be clear to me. How are the upward water fluxes calculated? Do you map the water vapor mixing ratio at a certain pressure level to the ATTILA air parcels? Does your method consider any phase changes? I would like the authors to explain the applied method in detail. Otherwise, it is not possible to follow the argumentation and conclusions. Overall, I am not sure whether the use of ATTILA is suitable in this study.

Specific comments:

The manuscript is not very well structured. In many cases the chosen figures are not

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appropriate to confirm the conclusions drawn in the text and the argumentation is hard to follow. The quality of the manuscript additionally contributes to my negative position. Below I have listed several points:

- The title “Stratospheric Dryness” somehow implies a review article on dehydration mechanisms in the TTL. This is definitively not given in the present study. In fact, the paper shows a comparison of simulated stratospheric water vapor with different observations. Therefore, I recommend to change the title to a more precise term.
- p 11250: I miss a discussion of the studies of Fueglistaler et al., 2004, and Fueglistaler and Haynes, 2005, JGR. I think these studies are a major contribution to the subject of troposphere-to-stratosphere transport and stratospheric water vapor, and should be mentioned here. Overall, the given summary of different possible dehydration mechanisms is not up-to-date. Fundamental studies of Holten and Gettelman (2001) and Gettelman et al. (2002) discussing the impact of horizontal advection on dehydration are also not mentioned.
- section 2: What is the intention of this section? I think the readers of ACP are certainly familiar with the fundamental dynamics and radiative processes in the stratosphere and the given overview on stratospheric dynamics is unnecessary. I suggest to omit the whole section or, at least, to shorten it substantially. A short paragraph motivating the choice of certain geographical regions and pressure levels for the analysis could be added to the results section.
- p 11253, l 12 and l 16: There are two different values for the vertical resolution in the lower stratosphere, 500 m and 700 m. Which is correct?
- p 11254, l 5: citation: Landgraf and Crutzen (1998)

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- p 11255, l 4: An intercomparison of different numerical schemes does not provide information about the reality of the results unless the model results are compared to observations.

- p 11255, description of cloud micro-physical processes: I think the cloud parameterizations play a crucial role for this kind of model study. Maybe the authors could give a more detailed description of the cloud micro-physics in E5M1. For example: Does the cloud scheme consider the number concentrations of cloud droplets and ice crystals as prognostic variables? Does the cloud scheme consider supersaturation?

- p 11256, l 7 et seq: The description of ATTILA and the applied method (forward trajectory model) should be more detailed (see above).

- p 11258, last paragraph section 4: Fig. 1 shows E5M1 model results, but it does not show whether the simulated SAO, QBO and tape recorder signal are realistic or not. Where is the link between Fig. 1 and model nudging? QBO and tape recorder can be realistically simulated without model nudging, too. The intention of this paragraph is not clear to me. I suggest to omit this paragraph as well as Fig. 1.

- p 11259, l 11: What's about MIPAS data after November 2003? There should be more MIPAS data available?

- p 11259, l 27: Divakarla et al., 2006

- Fig. 2: Title should be 5°N - 5°S instead of 5°N - 5°N

- p 11260, l 16/17: I totally agree with the authors that from Fig. 1 the impact of the QBO on water vapor is not clearly evident. I would like the authors to add a figure clearly showing the role of the QBO, maybe a correlation plot between stratospheric water vapor and zonal wind.

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- p 11260, l 21-25: Is there any evidence for this statement? The QBO modulation of water vapor is mainly apparent in the tropics, the downward transport of moist air masses from the mesosphere occurs in high latitudes. Therefore, I can not see an interaction. Please clarify this point.

- p 11260, last paragraph, SAO: Again I think that Fig. 1 is not appropriate to show the correlation between SAO and water vapor, or the impact of the SAO on the QBO. Furthermore, it would be interesting to calculate the amplitude of the QBO and SAO induced water vapor anomalies.

- Fig. 4 and 5: Why do you show AIRS temperatures for July 2003 and January 2004? Best agreement between model results and observations? Why do you show AIRS at 100 hPa and MIPAS at 70 hPa? White indicates temperatures above 230 K in Fig. 5? Please clarify.

- p 11262, l 21/22 "... the model realistically simulates dynamic and radiation processes": On page 11260 (l 5/6) the authors stated that the model has a dry bias which should have an impact on the model's radiation. However, the simulated temperatures are in good agreement with observations. That seems to be inconsistent.

- p 11262, l 28: 200 - 75 hPa, not 200 - 90 hPa

- Fig. 6: As obvious from Fig. 3, the water vapor distribution in the tropics shows large zonal differences. It might be interesting to show regional correlations, e.g. for the Asian area, and not only zonal mean values.

- p 11263, l 7/8: "... processes that control water vapor are to a large degree located in the outer tropics". Please clarify. Which processes?

- p 11263, l 27/28: How does the Asian monsoon affect the extra-tropics of the southern hemisphere? Please explain. Furthermore, the impact of the Asian

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monsoon on the width of the PDF is not clearly evident from Fig. 7.

- p 11264, l 4: Any idea for the lower correlation coefficients in 100 hPa?
- Fig. 8, lower panel: The model data show a bimodal PDF also for 10°N - 30°N. Is there an explanation for this result?
- Fig. 9: What do the white areas mean? Any idea for the lower variability in the model results (p 11264, l 19)?
- p 11266, l 9-11: I think a figure showing simulated net heating rates within the TTL would be helpful.
- section 6.3: The whole section is very confusing. Model results and observational studies are mixed up and it is not clear whether the model results reproduce the observations. For example, as far as I know, supersaturated regions are not considered in the model. Some conclusions seem to be a pure invention. For example, I can not find any evidence for the statement that the model results indicate that in situ tropopause cirrus formation contributes most to dehydration (p 11267, l 4/5).
- p 11268, l 22-27: This paragraph is somehow speculative. Are there any indications for a weakening of the monsoon since the 1990s?
- p 11269, l 2: The water vapor decrease after 2001 is also apparent in other observational data sets, e.g. the Boulder balloon soundings.
- p 11269, l 13-20: Is the drying tendency since 1997 due to the solar cycle a pure model result? Please clarify.

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