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

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Referee #2 evaluates our manuscript rather negatively although the detailed comments can be easily rebutted or improved in the revised manuscript. I am grateful for the many detailed comments by Ref #2 which help improve the manuscript. However, I disagree with the remark that "the paper lacks scientific originality with respect to model validation and model development". This is untenable since for the first time we present a fully coupled and comprehensive lower-middle atmospheric chemistry GCM including point-to- point comparisons of model results with satellite data. Our manuscript is part of an ACP special issue in which both model development and validation are described in much more detail than usual, and we will be happy to provide additional details.

Referee #2 states that "With respect to process studies I have some doubts that a GCM is the appropriate tool", referring to the parameterization of convection and cloud microphysics in GCMs. Ref#2 continues "The presented model results and comparisons with observations are not enough...". If these were serious criteria, then a large number of studies, including trajectory analyses based on ECMWF data, would disqualify. I would argue that our model accounts for more details regarding TTL processes than previous GCMs, and furthermore our manuscript presents a comprehensive description of which processes are resolved or parameterized. This should be considered in view of recent studies that use coarser and less comprehensive models or meteorological analyses (the latter also produced by GCMs).

The example that underscores the "General problem of the manuscript" is ill- chosen. There is no "reversal conclusion" between our explanation of the model dry bias and the moistening effect of deep convection. Furthermore it is perfectly valid to use statements such as "this suggests" or "if correct".

One year model spinup is sufficient if the initial conditions are chosen appropriately (based on HALOE data). Moreover, we omit the first year of model results in the comparison with satellite data. HALOE and E5M1 show a very similar drying tendency, notably in the region where HALOE data are most reliable (middle stratosphere). The reasons have been discussed in section 6.4. We avoid strong conclusions about water vapor trends, firstly because the simulated period is too short, secondly because our model has a dry bias, and thirdly because measurement data sets are not conclusive.

The sudden drop of water vapor in 2001 is partly associated with inter-annual variability (2000 being humid), which may be underestimated by our model. Indeed, the HALOE data suggest the period after 2000 is relatively dry. Why is it "indispensable to show that the model reproduces this sudden drop"? The satellite data are sparse and not conclusive. Even though some of the details of our model results do not fit perfectly, it can hardly be denied that the TTL drying is simulated realistically (e.g. a humidity reduction of more than an order of magnitude between 200 and 100 hPa).

The use of ATTILA in E5M1 has been tested, similarly as Reithmeier and Sausen

(2002), and the trajectories remain well distributed. Ref#2 correctly addresses the sparseness of air parcels in the upper part of the model domain. However, this is not an issue in the TTL and lower stratosphere.

Accounting for convection with trajectory models is indeed a problem, which we are currently addressing with ATTILA. Note that only small scale convection is problematic; the larger scale motions are resolved. In section 6.5 we present air mass fluxes in the TTL and lower stratosphere. To the extent that the mean upward motion at 200 hPa is affected by small-scale convection, our method may underestimate the air mass fluxes. At higher altitudes this can be neglected.

The mass fluxes are calculated with ATTILA by accounting for the mass transports across pressure boundaries (200, 100 and 75 hPa). We have performed these calculations for air and water in its three phases. During transport time steps phase changes do not occur. A detailed description of the ATTILA transport scheme within E5M1 will be submitted to ACP soon. An additional paper in which ATTILA has been used in ECHAM4 is in press in Climate Dynamics by A. Stenke and V. Grewe.

Specific comments:

Our manuscript addresses "Stratospheric dryness". If it were a review article this would have to be mentioned specifically. It will be no problem to change the title if the current one gives rise to controversy or irritation.

p. 11250: The revised manuscript will include references to the studies mentioned, as requested.

The intention of section 2 is given toward the end of section 1: "In the next section we review some main aspects of stratospheric radiation and dynamics to provide a context for our AC-GCM simulation results and help understand the desiccation mechanism". Readers who are familiar with these fundamental concepts are welcome to skip this section, which we will explicitly mention in the revised manuscript.

p. 11253: The vertical resolution in the tropopause region is higher than in the stratosphere, which will be stated more clearly in the revised manuscript.

p. 11255: The intercomparison by Tost et al. (2006) also includes observations.

p. 1255: The revised version of the manuscript will mention more clearly that the cloud parameterizations only account for cloud bulk properties, not for microphysical details such as droplet and crystal number concentrations. Supersaturation does not occur in the model.

p. 11256: The ATTILA scheme will be described in much more detail in a manuscript by P. Jöckel et al., to be submitted to ACP soon (see above).

p. 11258: The QBO simulation of E5M1 has been compared to wind data in Jöckel et al. (2006) for a shorter period. Here we present an extended period; however, a comparison with measurement data is beyond the scope of the present work. For the SAO such data are lacking. Ref#2 may not be aware that many GCMs that generate a QBO have difficulties representing the period. In our experience this is very sensitive to the parameterization of gravity wave drag. Further, many GCMs do not include the full stratospheric water cycle. Indeed, most GCMs produce a tape recorder signal, but often the water vapor mixing ratios and upward velocities are wrong.

p. 11259: The MIPAS data presented have been processed especially for this work, and represent a period in which the instrument worked particularly well. Additional MIPAS data of this quality are not (yet) available.

p. 11260: As explained in the paragraph subsequent to l.16/17, our work indicates a smaller QBO signal in water vapor than previous studies. The reason is that previously a partly counteracting effect from water vapor produced by methane oxidation was not considered. We will refine the formulation in the revised manuscript. The relationship between the QBO and upward water vapor transport in our model has been extensively discussed by Giorgetta and Bengtsson (1999) and Giorgetta et al. (2006), and should



ACPD

6, S5991–S5997, 2007

not be repeated here.

p. 11260: A "correlation" that can be seen by the naked eye can be quite convincing. But actually, we do not address a correlation, as stated by Ref#2, but we mention that an influence by the SAO is manifest and that the westerly phase coincides with the most humid conditions. This is illustrated by Fig. 1. In the last paragraph we mention a "triggering" mechanism. We will reformulate this into "a causal relationship" (also in the conclusions). In this article we merely show that the model produces an SAO and a QBO since they are relevant for vertical velocities. It is beyond our scope to present a detailed account of these phenomena.

Fig. 4 and 7: Because at 100 hPa thin cirrus clouds are prevalent (see also figure 12), the MIPAS water vapor retrieval at this altitude is difficult (also for other IR spectrometers), and therefore the 100 hPa data are sparse. This is why we focus on 70 hPa for which the data set is larger and of higher quality. White areas indeed indicate out of scale data. As also indicated in our reply to Fueglistaler, we have requested additional AIRS data to perform a point-to- point comparison with model results.

p. 11262: The dry bias is only minor, on average 10-15%. This is not likely to have a major impact on the temperature distribution. It may have a small influence though, and therefore we state that the model is "realistic", not perfect.

p. 11262: We do mean 200-90 hPa because at 75-80 hPa the correlation is lower than 0.8 during summer.

Fig. 6: This is an interesting suggestion. We will check into this.

p. 11263: This refers to transport processes, which we will mention in the revision. If the tropical water vapor at 75 hPa is strongly influenced by transport from higher latitudes, then the correlation between water vapor and temperature within the tropics is likely to be lower, in contrast to periods where local temperature governs local dehydration within the tropics.



ACPD

6, S5991–S5997, 2007

Interactive Comment

p. 11263: The Asian monsoon does not affect water vapor in the SH extratropics, at least not directly. The Asian monsoon is associated with less effective dehydration (owing to higher temperatures). The consequent synoptic events of relatively high humidity broaden the PDFs toward higher mixing ratios.

p. 11264: The slightly lower correlations indicate slightly poorer agreement with satellite data.

Fig. 8: The bimodality indicates the relative importance of different types of transport processes or routes. Back-trajectory calculations to the locations involved might help identify these.

Fig. 9: White areas indicate no data. We only show model results (fig. 9a) at locations for which MIPAS data are available (fig. 9b). I suspect the model underestimates synoptic event-related variability, which might improve at higher resolution.

p. 11266: In the revised manuscript we will include a figure with model calculated net heating rates at 75 and 100 hPa.

Section 6.3: It is difficult to respond to the generic statement that this section is confusing. Our model does not produce supersaturation, so apparently this is not needed to reproduce the thin clouds and dehydration. We will reformulate this in the revision and remove a confusing remark about supersaturation. Some of the process details are "hidden" in parameterizations. For example, the model applies ice sedimentation rates based on observations. This means the model cannot help finding an explanation for the formation of ice crystals large enough to sediment from the TTL. The parameterization of process details is necessary in any type of model, and does not principally disqualify the results. We will also reformulate the confusing remark about the contribution of ice transport to thin TTL cirrus.

p. 11268: In the revision we will remove the speculation about the weakening monsoon.

p. 11269: We do not go into detail about the drying tendency since 2001, because this



ACPD

6, S5991–S5997, 2007

Interactive Comment

phenomenon has not yet been properly analyzed. We nevertheless acknowledge the possibility and that our model may not represent it accurately.

p. 11269: In the revision we will additionally emphasize that the solar cycle effect is a model result, although the formulation "model-based explanations of stratospheric humidity tendencies" is rather unambiguous.

Jos Lelieveld

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