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Atmospheric Chemistry and Physics Discussions

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

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Ref #3 has some doubt concerning our contribution to the debate of TTL dehydration. It is unfortunate that this referee does not appreciate the novelty of our atmospheric chemistry - general circulation modelling approach. It is not clear to me why the referee considers the combination of model development and the improvement of process understanding as problematic, and interprets it as a lack of focus. I do not share this opinion.

Major issue 1:

Figure 11 presents the requested seasonal variation of the 100 hPa temperature and water vapour. Since the figure is printed rather small the details are difficult to discern. This should be improved in the next version of the paper. Furthermore, we present extensive comparisons with datasets.

In the first paragraph of section 6.2 we argue why a quantitative comparison between

HALOE and E5M1 in Figure 2 is difficult, so we agree with the referee that this figure cannot be used for that purpose. Therefore we do not make such a comparison, and I am puzzled by the critical remark. Since the HALOE dataset is sparse, especially in the lower stratosphere, and biased towards cloud-free conditions, it is most useful to test the model at locations where the satellite data were taken (see Figures 7 and 8).

Figure 4 presents a rather quantitative impression of the seasonal cycle of 70 hPa temperatures (years 2002/3), not the TTL. The comparison of TTL temperatures is presented in Figure 5 (100 hPa).

We are not avoiding any type of comparison. Our model results are available on request, open to all comparisons. The PDF statistics, showing e.g. non-gaussian distributions, provide rather powerful tests of stratospheric transport characteristics (see also the interactive comment by Tuck and references therein). For reasons mentioned above and in section 6.2 we avoid over-interpretation of the HALOE data.

Major issue 2:

Please check Tost, H., P. Jöckel and J. Lelieveld, Influence of different convection parameterizations in a GCM, Atmos. Chem. Phys. 6, 5475-5493, 2006. From this work we know that the convection scheme in our model often does not reach sufficiently high. We do not state that ozone measurements or model results could present unambiguous evidence of convective overshooting plus mixing. We simply combine the facts that overshooting happens in the real world and not in our model. We agree that overshooting events are rare above 100 hPa, although they do occur (see Alcala and Dessler, 2002). It is furthermore conceivable that convective penetration of the TTL occurs below 100 hPa and that the signal is carried upward in the large scale circulation.

For a comparison with SHADOZ data (balloon soundings) we refer to Jöckel, P., H. Tost, A. Pozzer, Ch. Brühl, J. Buchholz, L. Ganzeveld, P. Hoor, A. Kerkweg, M.G. Lawrence, R. Sander, B. Steil, G. Stiller, M. Tanarhte, D. Taraborelli, J. van Aardenne and J. Lelieveld, The atmospheric chemistry general circulation model ECHAM5/MESSy:

ACPD

6, S5173–S5176, 2006

Interactive Comment

Consistent simulation of ozone from the surface to the mesosphere. Atmos. Chem. Phys. 6, 5067-5104, 2006. Unfortunately, combining SHADOZ with satellite data (e.g. HALOE or MIPAS) is not trivial.

True, convective overshooting was originally invoked as a dehydration mechanism. Meanwhile, numerous studies have shown that convective outflow locally moistens the air. Dehydration subsequently takes place after cirrus clouds have formed, from which the largest ice particles sediment fastest. The ice particles formed by water vapour from the deepest convection must be relatively large to contribute to dehydration, since they need to fall large distances in order to leave the TTL. Therefore, the deepest convection may not dehydrate very effectively.

Other comments:

- We will check and improve the consistency of geometric and pressure heights.

- We will check the semantics of the ozone layer. It should be ok to relate the ozone layer with stratospheric dryness in the introduction.

- The hygropause is still an issue, and our results (also of Richard et al., 2006; see interactive comment by Tuck) indicate that drying can continue above the thermal tropopause. Nevertheless, most of the drying occurs at lower altitudes (in our model between 100 and 200 hPa). I would use a different formulation: The tropical tape recorder is a manifestation of the seasonality of the hygropause (not vice versa).

- In the introduction we present a brief historical overview, referring to Rosenlof (2003), and we agree that the simplified fountain concept is no longer taken seriously. Note, however, that the paper by Newell and Gould-Stewart (1981) is still often cited.

- We do not claim that radiative heating inside cumulonimbus clouds is relevant in this context. We refer to the “presence” of cumulonimbus anvils and cirrus clouds, as discussed by Hartmann et al. (2001) and Corti et al. (2006). We will rephrase these sentences to avoid confusion.

ACPD

6, S5173–S5176, 2006

Interactive Comment

- The model nudging is used because we aim to represent realistic meteorological conditions with our AC-GCM. We do not use analyses because we are attempting to compute the relevant processes with our own model rather than rely on ECMWF and NCEP. Indeed, analysed meteorological fields do not include QBO/SAO, important for the speed of the tape recorder signal.

- You mean page 11263? A "mechanism" can include a number of processes.

- We will reformulate the sentence on p. 12266 to emphasize that radiatively induced descent can locally supersede wave driven ascent.

- "Injection of water" should be replaced by "injection of sulphur species" (see also line 10-12).

- We will highlight the radiative nature of our "drain" in comparison with the drain-concept of Sherwood (2000).

Jos Lelieveld

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