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Interactive comment on "Validation of water vapour transport in the tropical tropopause region in coupled Chemistry Climate Models" *by* S. Kremser et al.

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Received and published: 18 November 2008

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Reply to the Anonymous Referee #2

We thank the referee for helpful comments.

Major Points

1. Validation *My main concern is related to the use of the term "validation" and the aim of the present paper. "Validation" usually deals with an external data set that is explicitly composed of measurements. The outputs of the two CCMs are only compared*

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to ERA40 parameters. At any stage of the manuscript, the CCM outputs (temperature and/or water vapour) are compared to measurements: e.g. for water vapour the space-borne sensors UARS/MLS, AURA/MLS, UARS/HALOE. Some intriguing Troposphere-to-Stratosphere Transport (TST) is calculated by DLR over Africa and South America, i.e. probably induced by continental convective systems that could indeed be consistent with TRMM/overshooting precipitation features (Liu and Zipser, JGR, 2005).

We agree to the referee's first comment that the word "validation" is misleading (c.f. also referee #1). Rather than strictly validating the water vapour transport in CCMs we study how the process is represented in the two CCMs that are part of our study. We feel that this is an important issue and justifies the publication of the paper. In the revised manuscript we have now completely avoided the term validation. E.g., the title now reads "Water vapour transport in the tropical tropopause region in coupled Chemistry Climate Models and ERA-40 reanalysis data". The purpose of this study is to test the ability of CCMs to realistically capture the water vapour transport from the troposphere into the stratosphere. We want to present differences in the transport schemata in CCMs in comparison to the ERA-40 reanalysis data. We do not want to validate the CCMs with the ERA-40 reanalysis data, but we want to assess the transport processes through the tropical tropopause region that lead to the modelled water vapour concentrations in the lower stratosphere.

2. Processes *The study is also based upon models with moderate horizontal resolutions (not better than 3.75° x3.75°). Consequently how well are reproduced convective systems (even large-scale systems) and up to which altitudes, certainly not above 150 hPa? Mesoscale models have shown their ability to transport upward air-parcels up to the lower stratosphere, with an associated rehydration of the ambient air masses through overshooting processes. This could alter the conclusions of the present manuscript (ERA40 does not include these subscale processes). In addition, how well the diurnal evolution and the continental vs. maritime convective systems behave in the CCMs, and do they agree with measurements from Liu and Zipser (2005)?*

This study relies on the common assumption that overshooting convection has a very limited influence on the water vapour transport into the stratosphere on a global scale. Convection certainly plays an important role for the vertical transport into the TTL but probably a very limited role for the transport into the stratosphere. We clearly mention this assumption in the paper (as a caveat) and justify it (page 11002). Thus the behaviour of the continental vs. maritime convective systems is not important for our study.

Does ERA40 accept supersaturation? If yes which amount? Since CCMs do not include supersaturation, how this could impact the overall results? Is it just a "flat scaling"? Will be the scale that "flat" because of processes that are by nature inhomogeneous in time (diurnal variation) and space (continent/maritime)? In my opinion, there are few chances the scaling to be "flat".

We are not using ERA-40 water vapour. We are using the ERA-40 wind and temperature fields for the trajectory calculations. The water vapour along these trajectories is determined by a simple microphysical scheme that does not account for supersaturation and is comparable to the schemes used in the CCMs.

The geographical distribution of the dehydration points will not be influenced by supersaturation. Since the supersaturation does not contribute to the total water vapour entry substantially and the absolute values of the water vapour entry produced by the CCMs are way too high, any small correction due to supersaturation would not change our results by much anyway.

3. Models *Could you specify the vertical resolution of DLR in the TTL? Could you please discuss the horizontal resolution vs. representation of convective systems and their maximum altitudes reached for both models? Very interestingly, FU better represents upper stratosphere whilst DRL TTL. Thus DRL should be closer to ERA40 in the present study. Nevertheless, FU seems to give results slightly closer to ERA40 than DLR. Could you explain this intriguing feature?*

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The vertical resolution in the tropical tropopause region is 600m in the E39/C model.

Again, the assumption in the present study is that convective transport does not play a significant role for the water vapour transport across the tropopause on a global scale.

We do not want to validate the CCMs with the ERA-40 data; we assess the transport through the TTL that leads to the observed or modelled water vapour concentrations. The purpose of this study is to test the ability of global CCMs to realistically capture the water vapour transport from the troposphere to the stratosphere and to present the differences in the transport schemata in both CCMs. There is clearly a need for further studies to examine the causes for the differences in transport in the CCMs. But this beyond the scope of the present study.

4. Monsoons *In NH summer, the paper deals with the Indian monsoon. But several recent studies have shown the impact of the Asian Monsoon Anticyclone (AMA) (lat > $30°N$) upon TST based on pollutants (e.g. CO) and H2O, together with the associated horizontal mixing in the tropics. In NH summer, an upper bound fixed at $30°N$ for the tropics is certainly too tight over Asia, i.e. not poleward enough. At least, this should be discussed since this could alter your conclusions.*

The end points of our trajectory calculations cover the geographical area from which air is then lifted up deep into the stratosphere with the upwelling branch of the residual circulation (i.e., +/- $30°$). Any material that is transported into the lowermost stratosphere outside this range has an important impact on the composition of the extratropical tropopause and lowermost stratosphere, but it will not be carried deep into the stratosphere.

The +/- $30°$ limit is only for the end points of the trajectories. It does not exclude air masses that are carried through the cold point elsewhere and reach the base of the upwelling branch of the residual circulation by equatorward transport in the tropopause region. But our figures show that this is not an significant process.

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5. Results *In general, you discuss areas of convection and ITCZ without showing their spatial extent on the associated Figures. Could you overplot OLRs on Figs. 2-13?*

For this study convective processes are not taken into account and thus maps of OLRs are not necessary.

You mention NH summers and winters. What are the actual periods you consider? How long? 3months?

The comment by referee #1 about the potential temperature of the cold point tropopause in the CCMs prompted us to carry out significant new calculations and analysis. We calculated two different sets of trajectories. The diabatic trajectories (based in ERA-40 wind and temperature fields) were calculated for 89 days. The kinematic trajectories (based on FUB-CMAM-CHEM and E39/C wind and temperature fields, respectively) were followed for 178K since the warm temperature bias leads to differences in the cold point temperatures compared to diabatic trajectories. All trajectories for the NH summer started on 30 August and for the NH winters the trajectory calculations begin on 28 February.

Neutral NH winter. Note that the South Pacific Convergence Zone (SPCZ) is detected in the DLR (Fig. 3c) outputs. Could you comment on that?

The SPCZ is indeed visible in Fig. 3c. But for the sake of a clear focus of this paper we do not discuss this in the paper.

El Niño NH Winter. Give Tmin and/or the bias in temperature Geographical distributions of fractional contribution (Figs. 5-7): Eastern andWestern Pacific with a maximum at $120W$ in neutral and DLR, but not in FU? Could you comment on that?

In this study we want to present the differences in the transport processes in different CCMs. The examination of the causes for these differences needs further investigations and is beyond the scope of this paper.

La Niña NH Winter. The TTL is measured slightly wetter than modelled (see e.g.

MOZAIC data), due to a lack of supersaturation in the models. Can that affect your conclusions? It is stated that: "Modelled H2O is in agreement with measurements from Schiller et al. (2007)". Over which areas and over which periods? La Niña period? There is only one reference published in non peer-reviewed literature. "Difference over the warmer regions of Africa and the Indian Ocean", could you please further explain?

Schiller et al. (2006) showed minimum observed water vapour of 1.5 ppmv (over Indian ocean) in January-February 1999 (La Nina period).

We agree that the observational basis is weak and we have changed "...is in agreement..." to "...is in rough agreement..." .

NH Summer. Monsoon: could you clarify about which monsoon you refer to? African/Asian/Indian?

We refer to the Indian monsoon.

"This value agrees well with ..."; Where? Clarify.

We refer to the mean water vapour value (averaged over all trajectories). Thus the area between 30°N and 30°S (all longitudes) contributes 3.8 ppmv to the total water vapour entry into the stratosphere in the reference calculation.

"As far north as 30°N": even farther north. See point 4. Need a discussion about the possible effect of the AMA.

See our response to point 4

Warm and wet points over Africa and South America from CCMs. This is interesting and needs further discussions including overshoots from TRMM (Liu +Zipser, 2005), see Point 2. It might also be possible that ERA40 is wrong over these two areas.

In agreement with a large fraction of the literature we assume that overshooting convection does not contribute a major fraction of the mass transport through the tropopause on a global scale. We clearly mention this assumption in the paper as a caveat.

Again, explain why FU represents so well the TST over the Bay of Bengal (compared to ERA40) whilst DLR is so poor over that region although FU should be more appropriate to reproduce upper stratospheric processes. This is very intriguing.

Indeed, it is intriguing and clearly justifies a detailed study about where the differences come from and which improvements of the models can be done. But such a study requires a number of additional model runs with each individual model and is well beyond the scope of this paper.

6. Residence times *Discuss the impact of the Asian Monsoon Anticyclone in NH summer, namely horizontal vs. vertical mixing. In general, you absolutely need to further discuss why the distribution of residence times is so different between CCMs and ERA40.*

The differences in the residence time mainly results from the fact that the advection schemes in the CCM are driven by the three dimensional wind fields and not by heating rates as it is the case in diabatic calculations with the ERA-40 data. This leads to excessive vertical diffusion (i.e. Wohltmann and Rex, 2007) and reduced residence times. We now show this in the paper by including results from kinematic calculations from ERA40 in Figures 14 and 15, which are similar to the CCM results.

7. Conclusions *Results from the CCMs are not "validated" against measurements. Compared to ERA40, all the outputs and diagnostics from CCMs (minimum temperature, water vapour, longitudinal distributions and residence times) are quantitatively highly biased. For some periods, CCM outputs are qualitatively consistent with ERA40. The numerous differences in the results from the two CCMs, and between the CCMs and ERA40 are not explained. Again, the key scientific point of the TTL studies is to explain and reproduce the processes occurring in that particular region to accurately forecast the evolution of climate. In that respect, the simulation outputs are scientifically weak and since no explanations are given, the outcomes of the study are negligible. Thus I do not agree when it is stated: "many important features of the geographical*

*distribution of dehydration and its variability with climate patterns are fairly well repro-
duced by the two CCMs chosen for this study".*

We agree that using the term "validation" in our original manuscript was misleading. In fact we do not validate the CCMs with the ERA-40 data or other observations. We rather examine the capability of CCMs to represent the water vapour transport in the tropical tropopause region and we present how different the results from different models (including ERA-40) can be.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10999, 2008.

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