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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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Review of the paper entitled " Validation of water vapour transport in the tropical tropopause region in coupled Chemistry Climate Models " by Kremser et al.

The paper shows a study of back trajectory calculations into the tropical Upper Tropospher-Lower Stratosphere in order to assess the quality of the outputs from two chemistry climate models (CCMs) to reproduce the minimum temperature, the amount of water vapour, the residence time and the location of air parcels reaching the lower stratosphere compared to the ERA-40 ECMWF standards. The two German CCMs are: the ECHAM4.L39(DLR)/CHEM (hereafter: DLR) and the Freie Universität Berlin Climate Middle Atmosphere Model (hereafter: FU). The authors consider three different periods during the NH winters: 1995-1996 (El Niño Southern Oscillation (ENSO) neutral phase), 1997-1998 (El Niño conditions), and 1998-1999 (La Niña conditions), extended towards NH summers. Whatever the periods considered, the two CCMs have a warm bias in the minimum temperatures (3-7 K) producing much wetter air (positive bias of 1.5 ppmv) parcels entering the lower stratosphere compared to the ERA40 atmosphere. The longitudinal distributions of dehydrated/rehydrated areas are not fully consistent between CCMs and ERA40. Dehydrated areas are most of the cases found above Western Pacific but Africa (and to a lesser extent South America) area shows excessive water vapour fluxes from CCMs compared to ERA40. Finally, the times of residence in the tropical UTLS are in average shorter by 1-2 days in the CCMs than in ERA40 but, above all, the majority of air parcels in the ERA40 stays 9-10 days in the UTLS whilst in the CCMs they never exceed 3 days.

The manuscript is well written, well structured, with an abstract consistent with the core of the article. Figures are also very clear although it is almost impossible to discriminate low temperatures within a range of 10 K (blue colour), and low amounts of water vapour within a range of 2 ppmv (blue colour) on Figs. 2-13 due to the use of a constant colour scale (that absolutely needs to be constant).

The "validation" of CCMs is indeed an important exercise in order to assess our potentials to predict the temporal evolution of the Earth atmosphere within a century, and above all, to understand and model the processes that affect the water vapour content in the UTLS, particularly the Tropical Tropopause Layer (TTL), and its direct radiative link to climate change. The term "validation", as it appears in the title of the manuscript, refers to an external data set, namely another modelled data set from ECMWF ERA40, that is supposed to represent the true atmosphere. Authors refer to a selection of articles using Lagrangian trajectory studies all using ECMWF forcings for vertical winds, either from heating rates or vertical velocity. The term "Validation" should refer to comparisons with actual measurements that can furthermore highlight processes that are not inserted in the global-scale models (e.g. overshootings), CTMs

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and CCMs. But, above all, even considering ERA40 as the real atmosphere, the results provided by the two CCMs in terms of minimum temperature, amount of water vapour, dehydrated/rehydrated areas, residence times cannot be considered as good at all. At least, quantitatively the differences between the two CCMs and ERA40 are huge (7-10 K in temperature and 1.5 ppmv in water vapour). It seems the CCMs do not track the same ECMWF atmosphere (e.g. longitudinal variations) and no plausible explanations are actually given in order to interpret these differences (between CCMs, and with respect to ERA40). These results seem to show that the scientific community is far from 1) reproducing the real atmosphere, and 2) forecasting its long-term evolution. Consequently, I do not consider this manuscript to be acceptable for publication in the ACP journal. I detail below the major and minor points of my arguments.

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 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).

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 airparcels up to the

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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)?

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".

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?

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.

5. Results

In general, you discuss areas of convection and ITCZ without showing their spatial ex-

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tent on the associated Figures. Could you overplot OLRs on Figs. 2-13? You mention NH summers and winters. What are the actual periods you consider? How long? 3 months?

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

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

- 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?

- NH Summer. Monsoon: could you clarify about which monsoon you refer to? African/Asian/Indian? "This value agrees well with ..."; Where? Clarify. "As far north as 30°N": even farther north. See point 4. Need a discussion about the possible effect of the AMA. 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. 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.

6. Residence times

Discuss the impact of the Asian Monsoon Anticyclone in NH summer, namely hori-

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zontal vs. vertical mixing. In general, you absolutely need to further discuss why the distribution of residence times is so different between CCMs and ERA40.

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 reproduced by the two CCMs chosen for this study".

Minor

p. 11010: "ppvm" should be "ppmv" (typo x 2). p. 11010: do not "exceed" 5 ppmv ("exceed" is missing).

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