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Interactive Comment

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

S. Kremser et al.

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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.

Reply to Referee #1 S. Fueglistaler

We thank the referee for valuable comments, which helped to improve the manuscript. A major comment by the referee is about the use of the term 'validation' in the paper and its title. We fully agree that using this term was misleading in our original manuscript. 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

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Interactive Discussion

Discussion Paper

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. We do not examine the causes for the differences in the transport schemata between the CCMs which would be beyond the scope of this paper which is quite long already now.

Specific comments:

Abstract: *P11000/L18: Replace 'reproduced' with something like 'similar'.*

We replaced 'reproduced' with 'fairly similar to ERA-40'.

P11000/L22: Replace 'satisfactory'

Done

P11000/L25: I cannot see evidence in the manuscript for 'excessive mass flux'. Probably you want to say something else?

We changed 'excessive mass flux' to 'excessive water vapour flux through warm regions e.g. Africa in the NH winter and summer' to avoid any misunderstandings.

P11001/L1: Replace 'underestimated' with 'lower'.

Done

Introduction:

P11002/L1-2: Awkward. Give credits for stratospheric circulation to Brewer and to

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Holton et al. 1995 (which is missing altogether in the bibliography!). In the context of stratospheric water vapour, Fueglistaler et al. 2005 can then be cited for showing that stratospheric water is indeed in agreement with what one expects from a large-scale transport perspective.

We changed this part to: 'Brewer (1949) suggested that air enters the stratosphere primarily in the tropics, because only there the temperatures are cold enough to dehydrate the moist tropospheric air to the observed stratospheric values. Air then moves up and poleward before descending back into the troposphere. This concept of the Brewer-Dobson Circulation (BDC) was confirmed by several studies (e.g. Holton et al. (1995)). In the tropics the transition region from the humid, turbulent troposphere to the dry, stable stratosphere is called the Tropical Tropopause Layer (TTL)'.

We added Holton et al. 1995 to the bibliography.

P11002/L8: No need to cite Gettelman and Forster here, this is what the TTL is by definition.

We removed the citation Gettelman and Forster.

P11002/L13: The reference here should be Holton and Gettelman, not Gettelman and Holton. Moreover, this is a pure modelling study and certainly cannot be used to back the claim you make here; hence remove it.

We replaced the reference Holton and Gettelman 2001 with Gettelman et al. 2002c. The study by Gettelman et al. (2002c) suggest that overshooting convection is not a common occurrence and that above 15-16km radiation becomes more important than convection for vertical motion. We added the reference Gettelman et al. (2002c) to our reference list.

P11002/L19: This is a pointless sentence! The cold point is by definition the coldest point; also, please note that it should be the 'final' or 'last' dehydration point, not just 'dehydration point'!

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We followed the recommendation and no write 'final dehydration point' instead of just 'dehydration point'. Nevertheless, we think a clear definition of what we mean with the cold point along a trajectory is very useful here to avoid confusion with the cold point in the eulerian vertical profile. This is key to our method to examine the water vapour transport.

P11002/L22: Replace 'behind' with 'after'.

Done.

P11005/L11: Again, credits here should go to Holton et al. 1995.

We added the citation Holton et al. 1995.

P11005/L15: This statement is wrong. Diabatic trajectories do not give inaccurate results because of convection. What you probably wanted to say is that the trajectories that use only radiative heating rates, give wrong results.

We rewrite the sentence to avoid misunderstandings:'... therefore the diabatic trajectories used here, which do not include latent heat release, give inaccurate results...'

P11006/L10: Please give the vertical resolution for the tropical, not the extra tropical region.

We included the vertical resolution for the tropical tropopause region (600m).

P11077/L22: Diabatic trajectories show less dispersion, which does not implicitly mean that they better represent reality! More importantly though is that the subsequent comparison is somewhat arbitrary - if you want to make a statement about differences between the GCMs and ERA40, then the trajectories should be computed with the same method. At least, you should also show the results from the kinematic ERA40 trajectories (which should be easy, since you say that you have calculated them).

Wohltmann and Rex (2007) showed in detail that the diabatic trajectories better represent the real atmospheric conditions (in terms of net vertical transport AND vertical

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diffusion). The advection schemes that are used in the CCMs are based on vertical winds. Therefore our lagrangian calculations for the CCMs, which are designed to diagnose the transport in the CCMs, are also based on vertical winds. Since we want to compare the transport characteristics in CCMs with the real atmosphere, the best reference are those calculations, which are closest to the real atmosphere, i.e. the kinematic ERA-40 trajectories. Any issues that are introduced by using vertical winds are indeed present in the CCMs, because their advection scheme is based on vertical winds. Hence, the issues introduced by using vertical winds rather than the more realistic diabatic vertical motions need to be part of the comparisons presented here. However, to allow the reader better insights into the origin of the differences we present, we have included results from kinematic ERA-40 trajectories in Figures 14 and 15 and discuss that they look fairly similar to the diabatic calculations in the other figures.

P11008/L7: Theta=365K is actually very close to the tropopause, I'd remove the bracketed remark (upper troposphere). Have you thought about the problem arising from the differing temperature biases in the models and ERA40 when using a fixed potential temperature level? Would it not be better to use some difference in pot. temperature relative to the pot. temperature of the cold point? (The same also applies for the residence time calculation, mentioned on page 11009//L6.)

This comment was very helpful. We carried out significant new calculations and analysis. The temperature bias in the CCMs indeed leads to differences in the level of the cold point such that the cold point lies above 400K in many cases. We have analysed the pdf of the cold point height and adapted the vertical range of our calculations accordingly. The scatter of cold point altitude (in theta) is larger in the CCMs, which requires to cover a broader vertical range in the calculations and hence longer trajectories. We now started the diabatic trajectories (based on CCM data) on the 470K surface instead of the 400K surface in our prior calculations. Due to the higher initialisation point the diabatic trajectories were calculated for 178 days instead of 89 days to be sure that the trajectories reach the troposphere during the prescribed period. These

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new calculations changed the temperature bias and the warm bias in the water vapour concentrations in both CCMs but the pattern of water vapour transport (the geographical distribution of dehydration points) remain similar. Thus the main conclusions of this study did not alter.

P110010/L5-8. This 'fractional water' is not useful, please just show the distribution of the LCP. (It is pointless because it is misleading - an area where it is very cold will show up as an apparently 'unimportant' area, even though it may be the area where most water is removed (this problem is generic to all studies of a tracer budget: do you want to emphasize where most of the tracer comes from, or do you want to emphasize where most of the tracer gets removed. In any case, it does not help your discussion of ENSO later in the paper.)

For the purpose of this study we think that the 'fractional water' is very useful for the reader of this paper. It shows where the trajectories had the last contact with the ice phase. The water content of the trajectories at this point will reach the stratosphere. These figures give an impression which tropical regions are important in the transport processes of water vapour.

P11012/L10ff: The effect of ENSO on entry mixing ratios and the distribution of LCP is extensively discussed and explained in Fueglistaler and Haynes (2005) and should be referenced here. (In particular, see their Figure 2c and 2d; the latter shows the effect of ENSO very clear.)

We added the recommended reference at this point and to our reference list.

Conclusions: Again, in your discussion please remember that you compare with results based on ERA40, but you do not show observations, and consequently absolute statements like 'Overall the distribution is much too zonal and water vapour contributions from Africa are too high'.(P11018/L13) should be avoided.

We agree to the reviewer comment and we wrote a conclusion which takes the aspect

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noted by the referee into account.

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

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