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Interactive Comment

Interactive comment on "Evaluation of the hydrological cycle of MATCH driven by NCEP reanalysis data: comparison with GOME water vapor field measurements" *by* R. Lang and M. G. Lawrence

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First of all we would like to thank the three anonymous referees for taking their time in critically refereeing our manuscript contribution to ACP. The comments, critiques and suggestions have helped us to improve the readability of the paper significantly, and to clarify a number of important issues the paper wants to raise, some of which might indeed have been covered under too many technical aspects of our model evaluation studies. In order to address the main issues raised by the referees, we have significantly restructured and shortened the paper especially with respect to instrumental details. We furthermore focussed our description of the results and their discussion on the main conclusions to be drawn from the comparisons. The main conclusions are that a generally too dry MATCH model, with on average too high precipitation rates, is related to an model underestimation of the observed WV residence time. In addition, we concluded that the strong correlations between the residuals in WV and precipitation, may be either due to problems in the rate of surface evaporation, or to an unrealistic transport of moisture. As has been pointed out by referee # 2, the latter has not been clearly stated in the ACPD version of the paper as being a possible cause of the observed correlations. We agree with the referee's conclusion that both the overestimation of surface evaporation, as well as differences between the modelled and the real influx of water vapor into a specific region, may lead to the observed residuals in water vapor and precipitation rates. Even though we made this statement at an early point of the ACPD paper it got lost along the way to our main conclusions. In the revised version of the paper this has been clarified.

We restructured the paper in the following way:

> The description of the GOME instrument and retrieval method has been reduced to the most important points. The evaluation of the cloud mask retrieval is now briefly summarized and we refer to the detailed description in the ACPD version of the paper.

> The description of the SSMI instrument has been shortened and the description and use of the NVAP database has been dropped completely. As a consequence, the evaluation of the retrieved total WVC from GOME in section 6 of the paper is now solely focussing on comparisons between GOME and SSMI and the radiosondes. We again refer here to the additional evaluations provided in the ACPD paper.

> The description of the NCEP reanalysis data has been reduced to the most important points relevant for the analysis of the results.

We restructured the results and discussion part in order to avoid repetition and in order to focus on the most important findings. Where necessary we refer to the ACPD version of the paper. Generally the results and discussion sections have been shortened significantly in order to increase the readability of the paper. Added subsection titles will guide the reader through the various observed features in order to make it easier for him or her to focus on specific aspects of the comparisons.

We area-weighted all global mean values (Fig. 7 and Table 1 and 2) in order to make them comparable to other studies. This affects the absolute numbers somewhat including the residence time values. However, the conclusions of the paper are not affected since area-weighting did not significantly change the relative values between models and observations.

1. Specific replies to the referees and changes adapted

1.1. Referee #1: Assimilation of GOME data

We do not want to suggest that assimilation of TOVS and SSM/I water vapor information in reanalysis models makes these observational data sets useless. Actually, the fact that the data can be assimilated in an useful manner indeed shows that the data is quite useful, as has been pointed out by referee #1. However, we did want to point out, that by assimilating this data in the reanalysis sets, a model evaluation employing SSM/I or TOVS data for comparison can not be seen as a completely independent evaluation in case the model is driven by reanalysis data. We refer to this as an advantage of GOME data, which has not yet been assimilated in the reanalysis model systems. However an assimilation of GOME WV columns over remote land regions should be considered by the weather centers. Here, the accuracy of the GOME derived column is an issue for assimilation as has been suggested by referee #1. But, as has been pointed out in the paper, our error estimates are based on sensitivity studies providing upper boundary systematic biases, which can be significantly reduced overall by selecting only the appropriate regions for assimilation, similar to what has been done for TOVS and SSMI data. For a large portion of remote land regions, e.g. in the subsidence regions, the biases are significantly below 0.1 cm.

1.2. Referee #1: "Bad model results"

A study of the detailed model processes responsible for the identified differences can indeed provide such "bad model results", which are commonly caused by deficiencies in model parameterizations or the supplied reanalysis data. These kinds of sensitivities studies would, however, heavily increase the amount of scientific material that had to be incorporated in the already long paper. We therefore decided to make them the subject of follow-up studies, which are currently being performed at the MPI for Chemistry.

1.3. Referee #1: Impact of multiple scattering and aerosols on the GOME retrievals

A study on the impact of multiple scattering and aerosols on the retrieved GOME WV column has actually been performed in detail in a previous publication by Lang et al., (2003). The results of this study are reported in the ACPD paper and an appropriate reference has been given. We will make this point clearer in the revised version of the paper.

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1.4. Referee #1: The common-cloud-problem

A common-cloud-mask strategy accounting for modelled and observed clouds has been applied exactly as the referee suggests in the ACPD paper, in order to evaluate the impact of the different possible application of the GOME cloud-mask to the comparisons. We rephrased parts of the explanation of the effect of cloud masking in the revised paper in order to clarify the results. The common-cloud-problem is an important statistical effect, which has to be taking into account for model output comparisons with tropospheric retrieval results not only in the case of water vapor but also for other retrievals employing measurements affected by clouds.

1.5. Referee #1 and #2: Model resolution and water vapor transport

Referee #2 correctly points out that the influx of water vapor into the observed regions like Europe and Asia, if not accurately modelled, may contribute significantly to the observed differences in both precipitation and water vapor. At the present stage of our studies, we cannot rule out either surface evaporation or transport as being possible candidates for the observed differences. In the new version of the paper this has been pointed out more clearly and we mention this problem when providing the averaged numbers for Europe and Asia. Both parameters, the dynamics and surface evaporation, are shared by both models, NRA and MATCH, which causes similar residuals in the comparisons. Differences between MATCH and NRA comparisons can therefore very likely be attributed to differences in the employed convective scheme, the model formation of clouds, as well as the production and evaporation of precipitate. For the latter processes the employed model resolution plays an important role as has been suggested by referee #1. Future sensitivity studies have to reveal how different model resolutions improve or worsen the situation. Preliminary studies showed that model resolutions below the T63 resolution employed here represent the convection less ad-

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equately. However, the situation does not improve significantly going to higher model resolution. The impact of the reanalysis data employed on the shape and strength of the ITCZ in water vapor content seems to be stronger than the influence of increased model resolution based on the first sensitivity study results.

1.6. Referee #2: Convective closure

The referee touches on a fundamental topic, convective closure schemes, which is the subject of heated debate in the field (see, e.g., Emanuel et al., QJRMS, 1994, and the ensuing comments and replies). In NWP models such as NCEP, as indicated by the referee, it has been traditionally been very popular to close on moisture convergence (the Kuo scheme and its derivatives). On the other hand, climate models have gravitated more towards closing on the CAPE (or on the closely-related "cloud work function"). This can indeed lead to substantial differences in the computed hydrological cycles. In our case, however, we do not expect this to be the main cause of the differences, since for this study we use the NCEP reanalysis data. Kalnay et al. (1996) indicates that the convection scheme used in the reanalysis runs was the new Pan and Wu (1994) scheme, which replaced the previous Kuo-based scheme. The Pan and Wu (1994) scheme is a bulk mass flux formulation based in principle on the Arakawa and Schubert (1974) scheme, with a closure on the cloud work function; the same principles are used in the Zhang and McFarlane (1995) scheme employed in MATCH. There are certainly differences between the Zhang and McFarlane (1995) and the Pan and Wu (1994) schemes, e.g., in the assumptions about the distribution of the base mass flux as a function of the plume entrainment rate, but we expect this should result in smaller differences than if completely different closure formulations were being used, as the referee suspected might be the case. Nevertheless, in response to other comments, we have generally tried to open up the discussion to make clear the possibility of various causes for the differences with NRA and with the observations, including

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the transport schemes, which need to be tested on an individual basis in the future. Also, in response to referee 3, we now do point out (in section 5) that both the Zhang and McFarlane (1995) and the Pan and Wu (1994) schemes are based on the same principles (Arakawa and Schubert (1974)).

1.7. Referee #3: Differences between NRA and MATCH

The major differences between a transport model and a reanalysis system is, of course, the assimilation of observations in the diagnostic part of NRA. As we pointed out in the ACPD paper, not all NRA output is, however, equally supported by observations and some parameters are simply prognostic model output even in diagnostic mode. For example, precipitation is a completely prognostic parameter not directly supported by any observation. It therefore depends significantly on the employed convective scheme. We added some additional information on the differences between NRA and MATCH to the description of the model. In the new version of the paper we now employ consistently the NRA data on a T62 resolution 28 layers as is publicly available on the web. Before, we used partly the input NRA fields for MATCH which are provided on a T63 resolution, 28 layers and partly results from the NRA T62 version. The new NRA results are therefore more consistent and come closer to what has been modelled by MATCH. These changes do not affect our main conclusions. However, NRA data provided here is different to what has been used as input for MATCH. This is why there can be differences, for example, also in PR on a global average for both models. We now have added a few sentences pointing out the specific similarities and differences, though we felt this was better placed in section 5 (MATCH description, after the NRA description).

1.8. Referee #3: ENSO

The two comparisons, 1998 and three years averages 1996 to 1998, have been chosen in order to support the significance of the result with respect to year to year changes like, for example, ENSO. Even though some differences are reduced for the longer time span as expected due to better statistics, the basic conclusions drawn in the paper are supported by both comparisons. The discussed differences can therefore not be attributed to ENSO, because 1996 to 1998 includes ENSO as well as non-ENSO periods.

1.9. Referee #3: GOME instrument issues

We added information about the period of available GOME WV retrievals to the revised version of the paper. SCIAMACHY is now mentioned as an successor of GOME. However, the recent OMI instrument covers only a very weak WV absorption band around 550 nm, which has not yet been studied for its potential of delivering accurate columns. It is the subject of ongoing discussion if new versions of the instrument (OMI 2) should extend their spectral coverage towards the near IR in order to include the important WV bands in this wavelength region. We support these ideas especially with respect to the fact that the instrument will significantly improve the spatial resolution with respect to GOME. We further removed the references to the additional GOME products because they are not subject of the current study. The influence of insufficient characterization of the degree of polarization of the earthshine on the retrieval of water vapor in the optically thin 590 nm band is negligible. Significant influence can only be expected for strong absorptions like the oxygen A-band or the water vapor absorption around 720 nm. There, the low spectral resolution of the GOME PMD measurements may affect the retrieval accuracy (c.f., Aben et al., GRL 1999). The degradation of the PMD measurements in channel 3 and 4 is also very small and does not significantly effect the

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retrieval quality.

2. Technical comments

We applied all the technical and formatting corrections and suggestions made by the referees in so far as they still apply to the revised version of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7917, 2004.

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