

***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**

Anonymous Referee #1

Received and published: 2 January 2005

The main question addressed in the paper is how well the hydrological cycle is represented by the MATCH model. Apart from that, the GOME WV record is used as an independent data source to evaluate the model.

The paper contains a wealth of interesting material. However, I found the paper difficult to read. The authors should consider a significant reduction of the length of this paper since in its current form, the main conclusions are somewhat blurred by all kinds of (technical) details which are not really needed to understand the issue addressed in

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the paper. As an example, the technical description of the SSM/I retrieval is interesting information, but does not play a role in the remainder of the paper. Also, the comparison with FRESCO and the (O₂)₂ cloud product could be left out in my opinion. One or two sentences could be used to summarize the outcome of this comparison exercise. Another factor that makes the paper difficult to read is the length of many sentences. Examples are: page 7926, line 19-24; Page 7950 line 6-12. Some extra full stops would help the reader a lot. Finally, there seems to be some unnecessary duplication of information. For instance, the fact that SSM/I and TOVS WVC are routinely assimilated in NCEP appears many times throughout the paper.

The scientific contents of the paper is large and within the scope of ACP. This work surely took a lot of data collection and comparison. In that respect, the authors did a great job. I have only a few minor remarks here. First, the authors mention many times that the GOME data provide an independent data source. However, they should realize that assimilation of satellite data (TOVS and SSM/I) in an operation model is not an operation that makes these data useless. During the assimilation process the model is adjusted towards the measurements. These forcings in itself provide interesting information about the biases in the model (or data). Of course, these forcings are not available to the authors, but a similar kind of research as described in this paper could be performed with the assimilation system itself. Furthermore I am curious about the authors opinion to assimilate GOME WV data in (operational) weather forecasts models. Probably the answer will be that the estimated accuracy of for instance SSM/I (0.12 cm, page 7928) is much higher than the GOME estimate (sigma about 0.5 cm in figure 4/5). However, some kind of statement about this issue would be nice, mainly because the GOME product is sensitive to lower tropospheric water vapor over land (as opposed to TOVS). In general, the comparisons between MATCH results and observations are relatively qualitative and hard to interpret for readers (like me) who do not have a feeling for what a 'bad model' would produce. A bad model is defined as a model with oversimplified parameterisations of WV removal (e.g. whenever relative humidity > 100% water vapor is removed as precipitation). Can the authors provide

such 'bad model' results?

Finally, I like the part of the paper in which the formation of convective precipitation in MATCH is identified as a probable candidate for improvements. In my opinion the scientific reasoning towards this conclusion should be the heart of the paper. Leaving out information that is not strictly necessary for this reasoning should greatly improve its readability. Another weak point is the effects of multiple scattering on GOME WV retrievals. Now multiple scattering is used several times as an hand waving argument to explain discrepancies (e.g. page 7938, line 111, page 7944, l2, page 7945, l10). This calls for a more quantitative analysis of multiple scattering errors on GOME WV retrievals (error as function of aerosol optical depth and solar zenith angle).

Technical:

The title: some confusion with 'field measurements' could occur.

Introduction: Page 7920, l3: does this list belong in an introduction? Will be repeated later! In general, the detailed MATCH description seems misplaced here. Page 7921: l3, preferably from independent sources. As mentioned above, biases can be evaluated in the assimilation system itself. Page 7922: line 16-20: another example of redundancy. Also, line 27-29: this issue appears already for the third time here!

Page 7924, line 22-23, see above.

Section 2.2. My suggestion would be to shorten this part considerably. Section 2.3. To much irrelevant details Section 2.4. line 16: according to their strengths and limitations. I guess the strong points of different data sources are combined (and not the limitations).

Page 7930, line 1,3 redundant

Section 3: page 7930, line 8: "?? Satellite infrared histograms???" and also a long sentence.

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Section 4: Page 7931, line 11: I suggest to rephrase: “the analysis can be heavily influenced by the model” Line 17-25: too much detail.

Section 5: line 23: It sounds as if for the experiments done here, gases and aerosols have also been calculated. If not, these issues are irrelevant for this water vapor study. From line 4 page 7934 I understand that only water vapor (diagnosed mode?) is calculated. Line 26 suggests that wet scavenging of gases is calculated.

Section 6: Page 7936: why is figure 5 based on 1996-2000 while figure 6 on 1996-1998?

Page 7938, l11: Without a more detailed error analysis suppress this strong conclusion: The latter is probably/possibly due to $\tilde{\epsilon}$.

Page 7939: I could not grasp the ‘common cloud problem’. What I understand is that MATCH may calculate clouds on the wrong locations. Thus the GOME cloud mask (i) leads in general to a dryer model because humid grid boxes are left out in the analysis (ii) sometimes does not align with the model blanking out dry model pixels and leaving in moist model pixels. In my opinion this calls for a ‘common’ cloud mask in which the model cloud mask is aligned with the observed cloud mask. In general the ‘common-cloud-problem’ should be better explained.

Page 7939, l28, leave out ‘,’ after show.

In the comparison between MATCH and observations the role of the limited model resolution is hardly touched upon. For instance, many times too broad features are observed in the MATCH model (page 7940 l8). In what respect these features can be explained by model resolution?

Page 7941: line 5: ‘compares very well’. The question here is of course how wrong you can get in a zonally averaged sense given the input from NRA in terms of evaporative fluxes, temperature, and winds.

Line 19, see resolution argument above.

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Page 7944, line 17: Ridged??? maybe mean: rigid

Line 26-29: GOME Æ. Scene. Sentence does not run properly.

Page 7945: Line 10. Hand waving aerosol argument. The authors assume here that arctic sea-salt haze shortens the length of the light path and hence the retrieved WV columns. I do not understand how 'low relative humidity conditions' make the aerosols grow Æ

Page 7946: line 20: unclear: 'are therefore expected to be significantly influenced by NRA'. Please use other words to explain. In what sense does the MATCH hydrological cycle differ from NRA?

Page 7948, line 11, remove ';'. Line 25: moisture instead of moister. The same conclusion re-appears several times on this page and page 7947/7949/

Page 7949, line 14: 'strong regional close relation' Æ do you mean close/strong relation on a regional scale?

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

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