Atmos. Chem. Phys. Discuss., 8, S6411–S6414, 2008
www.atmos-chem-phys-discuss.net/8/S6411/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Water vapor transport in the lower mesosphere of the subtropics: a trajectory analysis" *by* T. Flury et al.

A. Feofilov (Referee)

artf@usm.lmu.de

Received and published: 27 August 2008

General comments

Water vapor measurements in the middle atmosphere have been performed since the 1970'-s utilizing ground- and aircraft-based microwave measurements. Spaceborne measurements of the water vapor altitude distributions started in 1978 with the launch of the Nimbus-7 spacecraft observatory that utilized the SAMS and the LIMS instruments for water vapor observations. Since then water vapor has been measured by a number of space experiments: ATMOS, HALOE, ISAMS, CRISTA-1,2, and others. Currently, there are six satellite-borne instruments that are performing water vapor measurements in the atmosphere: ACE FTS/Scisat-1 and SOFIE/AIM occultation instruments and SMR/Odin, MLS/Aura, MIPAS/Envisat, and SABER/TIMED limb-emission

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

instruments. These instruments provide comprehensive latitudinal and seasonal coverage and vertical resolution of about 2 km. It looks like having another measurement that is limited in space and time cannot provide new information about water vapor distribution in the middle atmosphere.

However, the paper demonstrates that combining the measurement with the model can reveal the inner mechanisms of atmospheric dynamics that makes the paper interesting from the methodological point of view. Using the trajectory model the authors managed to trace the origin of water vapor measured in the course of the mission and to explain the 20% difference of water vapor VMR observed in the same area within 10 days period. This approach has considerable promise if used in combination with the data obtained with the above mentioned instruments that provide excellent temporal and spatial coverage. This can be named the strongest point of the paper.

I have the following general recommendations aimed at improving the quality of the paper and future work.

a) The instrument was operated once a year in 1998-2006. However, the extended analysis is performed only for 2005. It would be interesting to perform a more systematic analysis and to compare the atmospheric dynamics behavior in 2005 with that in other years using the same trajectory tracing approach.

b) Since the measurements are limited in time and space they cannot be used in long-term trend analysis. However, this kind of measurements is invaluable for the validation of other instruments. If properly coordinated with space-borne missions, these measurements can serve as an independent source of information for validating the models used for radiation inversion.

Summarizing, I think that the paper can be published in ACPD if the authors take into account the general and specific comments given in the review.

Specific comments

ACPD

8, S6411–S6414, 2008

Page 13776, lines 25, 26: comparing the Volume Mixing Ratios is not representative in respect to the text given in the line 1 on page 13777. The feasibility of measurements of this kind is linked with real concentrations and optical thicknesses along the line-of-sight. In some cases the emitters can be blanketed by the optically thick layer.

Page 13777, line 5: "typical" profile mentioned here and presented on Fig. 1 lacks the parameters of the scan, namely latitude, longitude, season, date, and time. These are required due to strong seasonal variability of water vapor VMR, especially in the polar regions.

Page 13378, lines 16–18 and page 13779, line 11: the description of a water vapor VMR profile obtaining is not clear. The instrument flies and measures continuously with a fixed line of sight perpendicular to the flight direction. The vertical and horizontal fields of view are also given. However, the integration time and, as a result, the spatial and temporal averaging parameters cannot be estimated from the text. I would also suggest moving all instrumental parameters description to the "Instrument" section.

Page 13779, line 17: the latitudinal gradient described here needs better explaining. Lines 14 and 15 refer to the mesospheric gradient above 65 km. Aura MLS water vapor data presented in Fig. 10, 11, 14, and 15 do not show water vapor distribution at 70 km. However, even 60 km distribution shows the features typical for this season. H2O VMR decreases from the summer to winter hemispheres at the mesospheric altitudes. Partially this is explained by the strong pressure decrease at high latitudes in the winter hemisphere that is linked with the lower temperatures below ~70 km altitude. The second reason for the summer to winter H2O decrease is the behavior of the vertical wind in different seasons (Körner and Sonnemann, JGR 106(9), 2001). As their Fig. 5a shows, the upward transport in the summertime changes to downward transport in the wintertime. However, Fig. 4 of the reviewed paper demonstrates the opposite behavior. Taking into account the above mentioned facts the explanation given in lines 16-18 looks speculative.

ACPD

8, S6411–S6414, 2008

Interactive Comment

Page 13779, lines 12-21 and references to Fig. 4, page 13788: Fig. 4 shows that there are two parameters that were varied during the flight, namely, latitude and longitude. One can see that these parameters were in counter phase with each other that complicates the "single-parameter" conclusions. Water vapor distributions measured by Aura MLS shown in the paper demonstrate the complexity of the atmospheric dynamics. The only way to understand the instant measurement in this case is to use an additional source of information like trajectories calculations and models. The paper shows that it's really doable and I would put more stress to it from the beginning.

Page 13780, lines 7-14: the model used for trajectories calculations is not described either in the text or elsewhere (reference). It is not clear from the text how accurately the model predicts the trajectories and what are its limitations. The ECMWF acronym is not defined in the text.

Technical corrections:

Page 13788, Fig. 4: the figure requires modifying. The non-linearity of the horizontal axis leads to puzzling the reader. I would suggest latitude binning instead of profile number binning. Using more natural geographic grid would also help to avoid using different rectangle sizes in the upper half of the picture.

Page 13789, Fig. 5 and page 13790, Fig. 6: the grids used here are very coarse. Fig. 4 shows that the changes were gradual and one could use a smaller bin size. There is also an inconsistency in figure titles. Fig. 5 shows the same relative difference in H2O VMR as Fig. 6 but has a different title.

Page 13779, line 5: "focusses" -> "focuses"

Page 13782, line 16: "prooven" -> "proven"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 13775, 2008.

ACPD

8, S6411–S6414, 2008

Interactive Comment