

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

Anonymous Referee #3

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The paper reports new results about airborne microwave measurements of water vapour at lower mesospheric altitudes. The authors try to analyse the difference in water vapour concentrations as observed during the outward and return flight from Switzerland to Australia. The analysis tool consists of a trajectory analysis on isentropic surfaces using ECMWF wind fields.

I read the previous comments to this paper by A. Feofilov , and from my point of view I agree to all his comments.

In addition, I have one major comment and several minor comments, so that I would recommend a final publication only after a major revision.

Major comment:

I do not agree with the conclusions which have been discussed in section 4 (Results and discussion) because I see a significant inconsistency.

Two local regions as part of the flight corridor have been investigated. The so called 'Mediterranean' area and the 'Arabic Sea and India' area, respectively, show different amounts of water vapour in the lower mesosphere during outward (Nov 8) and return flight (Nov 15). First, the case of the 'Arabic Sea' is discussed. At page 13780 (starting at line 15) the time range for the backward trajectories is defined as 4.5 days. Then the isentropic surface is defined as 2700 K. The authors say that this temperature corresponds to a approximate mean altitude of 60 km (line 19). Figure 8 shows the resulting backward trajectories computed on this isentropic surface by ECMWF data (line 11).

Now a first inconsistency occurs because Fig 10,11 show the corresponding AURA MLS water vapour at 50 km. This is not the correct altitude, because the authors talk about 60 km. Indeed, the authors should use the same altitude of transport (ECMWF isentropic 2700 K level)and water vapour (MLS at 60 km) in order to deduce unique conclusions. A fact, which the authors claimed in the text, but which is not shown in Fig 10 and 11.

However, and surprisingly, such water vapour plots valid for 60 km plots are shown in the discussion of the 'Mediterranean' case (Fig. 14, Nov 1 and Fig. 15, Nov 14).

The discussion using the 'Mediterranean' case starts at page 13781 (line 5). Now backward trajectories are again computed on the isentropic surface of 2600 K with a time of 3.0 days. Why do the authors change the isentropic surface from 2700 K to 2600 K (60 km to 58 km)? Why do you shorten the time duration of backward trajectories

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from 4.5 days to 3.0 days? However, now the authors use in their discussion the MLS water vapour plots from 60 km, a jump from 50 to 60 km, although they even decrease their isentropic height from 60 to 58 km.

So, to summarize, this is not a reasonable and fair analysis, using different altitude plots of MLS water vapour for the same trajectory study. The problem is that both type of water vapour altitude plots, e.g. Fig. 10 (50 km) versus Fig. 14 (60 km) show significant different water vapour structures. Fig 10 (50 km) shows strong water vapour amounts both in the northern (winter) and in the southern (summer) high latitudes. This is commonly observed because we see here a dominating chemical production of H₂O by the oxidation of methane by O single D (reaction: O (1D) + CH₄). On, the other hand, if you switch to an altitude of 60 km (lower mesosphere), dynamical transport effects will play a major role, especially the downward vertical wind over the winter pole transports dry air from the upper mesosphere downward. This effect can be identified in Fig. 14 as the blue color region in the polar winter region indicating low water vapor values. Obviously, the authors picked up for their analysis water plots of different altitudes in a non-unique way which simply allows an arbitrary fitting more or less to their observations.

Therefore, I suggest that this main part of the paper should be carefully rewritten.

Minor comments:

1) page 13777 (line 10-15): Delete the lines dealing with the general circulation of zonal winds, also delete Fig. 2 (CIRA wind). This is all too general, every reader is familiar with this, and even some of your sentences are not fully correct. Instead of, it would really impressive to show the horizontal plots of zonal and meridional wind fields of ECMWF which are used in the trajectory calculations. These should be discussed in section 4.

2) page 13777 (line 22): Solar lyman alpha photolyses in the upper mesosphere. In the lower mesosphere and partly in the stratosphere it are the deeper Schumann-Runge

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bands.

3) page 13777 (line 24): The reason of enhanced water vapour at high latitudes during summer in the 60-80-region is an upward vertical wind, thus, the gradient of H₂O from the equator to the pole in the mesosphere is due to vertical transport, and not due to photolysis.

4) page 13778 (line 18): Is the vertical resolution of your instrument really 10-15 km? (typing error?)

5) page 13779 (line 19): I miss the definition of the 'Mediterranean' area in lat/long intervals.

6) page 13780 (line 12): Delete Figure 7, say something about your numerical trajectory model, please delete the acronym TomTOM. Obviously, T. Flury has written this code, but do you really think that using Matlab and some visualisation software is so important to mention this several times?

7) page 13780 (line 22): Here I would insert a more detailed discussion of the wind fields (ECMWF) in combination with corresponding plots (see minor point 1).

8) Caption of fig. 11: In the text you write Nov 11, in the caption it is Nov 8.

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