Atmos. Chem. Phys. Discuss., 14, C10987–C10991, 2015
www.atmos-chem-phys-discuss.net/14/C10987/2015/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulation of the isotopic composition of stratospheric water vapour – Part 2: Investigation of HDO/H_2O variations" *by* R. Eichinger et al.

Anonymous Referee #2

Received and published: 9 January 2015

General comment:

This paper uses the EMAC model with a new submodel for calculating the water isotopologue HDO to investigate those processes determining the stratospheric water isotope composition and the water vapor budget. Particular emphasis is laid on understanding the tape recorder in δD. The authors present an interesting analysis of the effects of methane oxidation, which are shown to damp the δD-tape recorder above about 20 km. Further, they relate the summertime maximum of δD in the tropical lower stratosphere to transport processes in the Asian monsoon (ASM).

Overall, this well written paper presents interesting results useful for deepening our understanding of stratospheric water vapor and I recommend publication. However, I have one major comment (specific comment 1) and a few minor comments which the authors need to address.

Specific comments:

1) Asian monsoon effect - convection or in-mixing:

My major comment concerns the interpretation of the summertime maximum of δD in the tropical lower stratosphere and the proposed relation to the Asian monsoon, as formulated e.g. in the conclusions P29477/L14ff (and similarly in other parts of the paper):

"The origin of enhanced δD(H2O) in the lower stratosphere during NH summer in the EMAC model simulation was traced back to the Asian Summer Monsoon (ASM). Here, strong convection over the Tibetan Plateau lofts ice crystals into the upper troposphere, where these, when resublimating, isotopically enrich the water vapour. This water vapour crosses the tropopause over the Western Pacific and furthermore, follows the monsoonal anticyclone into the tropics. This process was shown to significantly contribute to the δD(H2O) tape recorder signal in the EMAC simulation."

I have serious doubts concerning this interpretation which is, in addition, in contradiction to the interpretation given by Randel et al. (2012), that convective ice lofting in the American monsoon causes the summertime δD maximum. If convection over Tibet enhances δD, why is δD above Tibet in the 380-400K layer particularly low (see Fig. 7)? The authors show that δD above Tibet is enhanced at 14 km, but how is this air of elevated δD transported upward, if not in the Asian monsoon?

Remarkably, the tongue of enhanced δD air above the West Pacific (Fig. 7), which the authors relate to convection over Tibet, is also evident in distributions of stratospheric tracers like ozone, and has recently been linked to in-mixing of aged stratospheric air from the extratropics into the TTL (Konopka et al., 2010). This in-mixing causes the summertime maximum of ozone in the TTL. Ploeger et al. (2012) further discussed how a tape recorder signal can emerge from this in-mixing. In my opinion, Fig. 7 suggests a similar mechanism for creating the elevated δD values in the tropics around 18 km/15°N (Fig. 6). From this point of view, in-mixing of aged stratospheric air from the extratropics around the Asian monsoon anticyclone enhances the δD in the tropics, in EMAC. In addition, convective ice lofting likely plays a role in the American monsoon (NAM), as proposed by Randel et al. (2012). The authors state on P29476/L21 that these effects of deep convection are likely underrepresented in EMAC. Hence, it seems not clear to me which effect (NAM convection or ASM in-mixing) dominates in the atmosphere.

I think a correct interpretation and clear description of these effects is a key point of the paper and therefore additional analysis is needed to either (i) confirm the proposed effect (convection over Tibet) and present a counter-proof for my contrasting interpretation (ASM in-mixing), or (ii) correct the interpretation, or at least (iii) discuss the potentially involved mechanisms adequately. Perhaps an investigation of the correlation between δD and a stratospheric tracer like ozone (which is probably included in the simulation) in the Western Pacific region (co-located with the tongue of enhanced δD) and in the NAM could elucidate these points.

2) Method description:

Although this is a follow-up paper of a Part 1, I think it would be helpful for the reader to briefly summarize the main processes affecting HDO and their representation in the model (e.g., in section 2).

P29464, L20:

Can you discuss possible reasons for the dry bias of EMAC water vapor?

P29469, L14:

"This air originates from the westerly wind regime at around 40N over the Asian continent, because a high potential vorticity gradient (not shown) north of this region prevents meridional air mass exchange (see e.g. Plumb, 2002)."

Horizontal PV-gradients above the North-Western Pacific around 130E are much weaker and transport (in-mixing) feasible. (This is related to my major comment.)

P29470, L23ff:

"The patch with negative values between 30 and 50N and 15 and 17 km suggests that the lack of the southward wind component in the American region leaves more isotopically enriched water vapour at the higher altitudes of the American extratropics."

I would think this region of negative anomaly in Fig. 9 simply reflects the low δD within the ASM core (compare to Fig. 7).

P29471, L5ff:

"For that, the anomalies w.r.t. the 21 year average of the δD(H2O) values between the 370 and the 390 K isentropes in the subtropical Western Pacific (15 to 40N and 120E to 140W) region and in the subtropical American and Western Atlantic region (15 to 40N and 120 to 20W)..."

The defined ASM region includes not the monsoon core, only the downstream region (where in-mixing occurs), while the defined NAM region includes both (NAM core and related in-mixing). This seems to be not a proper comparison of related effects. Consequently, the interpretation in P29472/L4ff that *"This correlation analysis confirms the connection between the strength of the Monsoon systems..."* mixes the different effects of convective upward transport in the monsoons and in-mixing around the anticyclones.

P29476, L10ff:

"However, Randel et al. (2012) present a different behaviour of δD(H2O) in the UTLS by analysing ACE-FTS satellite data. In this retrieval, enriched δD(H2O) at 16.5 km altitude can be found only over America and the patch of high δD(H2O) associated

with the ASM, as seen in the EMAC data is entirely lacking."

This is not true! Figure 10c in Randel et al. (2012) shows this patch as well (around 100E/15N), however weaker than in EMAC (and note that the ACE-FTS sampling density in this region is very low).

Technical corrections:

P29460, L13: *"...have a damping..."*

P29461, L5: *"...leads..."*

P29461, L24: *"...Eichinger et al. (2014)..."*

P29462, L5: A minus one is missing in the definition of δD.

P29465, L5: *"...processes are dominating..."*

P29476, L5: *"Later on,..".*

Figs. 3/4: ...could be merged into a single figure.

References:

Konopka et al. (2010), Atmos. Chem. Phys., 10, 121-132.

Ploeger et al. (2012), J. Geophys. Res., 110, 09303.

Randel et al. (2012), J. Geophys. Res., 117, D06303.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29459, 2014.