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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.

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*Reply to referee #2

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

  Thank you very much for your interesting and helpful comments. Please find our answers to all your comments below.

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.

1.1. This is an interesting and important point. The discrepancy between the conclusions (NAM vs. ASM) of Randel et al. (2012) and the present study arises from our results. We do discuss this discrepancy, which leads us to the argument, that the convection scheme might be in error. We will discuss this point much more thouroughly in the revised manusctipt, please see below.

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?

1.2. But Fig. 8 shows that in the region where enriched tropospheric δD can be found, also the tropopause and the isentropes are elevated. The upward transport of the enriched air is indeed caused by the ASM, in the outflow. Isentropic transport in combination with the westerlies here can account for the advection to the region over the West Pacific, where the tropopause is low. Also for the NAM this can be seen, but weaker. However, for the West Pacific, this can be complemented by inflow from old stratospheric air from the extratropics, as you state below. We will discuss this point, please see below.

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.

1.3. Thank you for this important information. Indeed we did not consider this process sufficiently. We will discuss it in more detail in the revised manuscript. However, we are not convinced that a comparison with ozone would hold for this, since this in-mixing process is largely dependent on the species itself, more specifically on its meridional gradient. Ploeger et al., (2012) also state that this process plays a role for the annual ozone variation in the tropics, but not for carbon monoxide and water vapour. Moreover, as stated above, these processes do not exclude each other. The question will then be how large the respective contributions are for δD(H_2O).
We will perform an additional sensitivity study without the influence of cloud/ convection on δD(H_2O) in order to resolve this issue, and replace the correlation analysis in Sect. 5.2. In fact, this leads to a restructuring of Sects. 5 and 6, please see below and also our answer to referee #1.

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.

1.4. This is indeed one of the main questions remaining from this study. We suggest this to be investigated by implementing and using other convection schemes in future studies.

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.*

1.5. Thank you very much. We agree that our analysis was a bit weak in this respect and that the conclusions might go too far. Therefore we will restructure Sects. 5 and 6.
As also stated in the reply to referee #1, we will remove large parts of Sect. 5 (including Fig. 9) and subsection 5.2. Instead, we will investigate more deeply the role of cloud/convective effects and in-mixing of old stratospheric air from the extratropics for the tape recorder. For this we will conduct additional sensitivity simulations without the effect of clouds/convection on δD(H_2O).
The individual influence of the two different monsoon systems, however, will still be addressed in the analysis and the revised discussion.
Due to the interhemispheric differences of lofted ice and its influence on δD(H_2O) in stratospheric water vapour, the ice lofting analysis (Sect. 6) will be used as motivation at the beginning of the section.

- *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).

  2. We will include a couple of summarizing sentences with reference to Part 1 in the introduction.

- *P29464, L20: Can you discuss possible reasons for the dry bias of EMAC water vapor?*

  3. This is a well known issue in many models. The main reasons for this are a too cold tropopause and possibly a too coarse model grid. We will include this in the manuscript.

- *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.) P294

  4. Thank you very much for this comment. We will change the manuscript accordingly and show a much deeper analysis of this effect. Please see point 1.5 for details.

- *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).*

  5. Thank you, you are probably right. However, due to the methodological issue (reply to referee #1 point 11) and the discussion above, we will remove this figure.

- *“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.

6. Thank you, among other reasons this comment leads us to remove the entire subsection and change the focus of this part of the manuscript (please see point 1.5).

- *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).

  7. Thank you for indicating that, we will change this sentence accordingly.

TECHNICAL CORRECTIONS

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

  1. Thanks, will be corrected.

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

  2. Thanks, will be corrected.

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

  3. Thanks, will be corrected.

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

  4. Absolutely, thank you very much for reading carefully.

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

  5. Thanks, sounds better indeed.

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

  6. Thanks, will be corrected.

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

  7. Thanks, good idea.

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