

Interactive comment on “Simulation of the isotopic composition of stratospheric water vapour – Part 1: Description and evaluation of the EMAC model” by R. Eichinger et al.

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Dear referee,

thank you very much for your valuable comments and suggestions. Please find below our reply to all your points.

1. Minor comments:

- Point 1: Thank you, further comma mistakes will be revised during the type-setting process.

C8646

- Point 2: MESSy: Modular Earth Submodel System; we will add this in the manuscript.
- Point 3: To our knowledge Schmidt et al. (2005) is the only publication, analysing $\delta D(H_2O)$ in the TTL and above in an AGCM modelling study. We will include this to the introduction and mention the crucial weaknesses of the applied model in that study (vertical resolution and prescribed chemical HDO tendency).
- All other points will be changed accordingly, thanks for reading carefully.

2. General suggestions

- Point 1: This is a very true point. However, for the start, we decided not to carry out this task for mainly two reasons:
 - (a) In order to make an exact one to one comparison between model and satellite data, the model output firstly has to be sampled along the satellite orbits at the exact time and place of the satellite overpass. Then, the cloud filtering and the averaging kernel will be applied. The cloud filtering also involves statistically complex measures. Firstly, the question arises if the observed or the simulated clouds have to be taken for filtering. Taking the observed only, may induce effects in the lower stratosphere through the cloud events, which may take place at other times in the model. Filtering the results by both modeled and observed clouds requires a statistical evaluation of the number of the remaining co-localised profiles.
This elaborate evaluation, in fact, does go beyond the scope of this initial study, but is planned for future projects.
 - (b) As is, the comparison 'simply' shows the best estimate of the model and of the satellite retrievals to describe reality. The meaning of this

C8647

way of comparing the two approaches, lies in drawing conclusions from the existence or nonexistence, respectively, of the tape recorder signal.

- Point 2: These questions are also very important. Yet, their basis is the more detailed evaluation towards particular satellite retrievals from Point 1. Therefore, we will have to leave them open for now.

The focus of this article (apart from the description and evaluation of the model) is the diverging results from the different satellite retrievals w.r.t. the $\delta D(H_2O)$ tape recorder. Since this is the first study with a global climate chemistry model, which addresses this question, we consider these details as secondary for now. The general question rather is, if such a signal is to be expected or not. Please also note that the addressed science question (the reason for the formation of the $\delta D(H_2O)$ tape recorder and its sensitivity) is investigated in Part 2 of the article.

Please comment in case you consider certain parts of these explanations as indispensable for the manuscript, or have any issues with them.

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