Atmos. Chem. Phys. Discuss., 3, S1495–S1497, 2003
www.atmos-chem-phys.org/acpd/3/S1495/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The isotope composition of water vapour: A powerful tool to study transport and chemistry of middle atmospheric water vapour" *by* Ch. Bechtel and A. Zahn

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Received and published: 10 September 2003

Overall quality:

This paper presents a detailed photochemical modeling study of the isotopic composition of stratospheric water vapor. It expands on earlier work (particularly the work of Kaye et al.) by treating simultaneously HDO and the heavy oxygen isotopomers, as well as more recent measurements of smaller ^{50}O3 enrichments. This is a good time to revisit this issue due to improved measurements of reaction rates in the lab and mixing ratios in the stratosphere, as well as interest in the isotopic composition of other species such as CO_2 and N_2O. My only real complaint is that I would have liked to see more comparison with the results published in two of my own papers, as described

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below. I will leave it to the authors to decide whether or not the content of those papers merits further comparison and discussion in the present work. Overall I feel that this paper is a valuable contribution to the literature and should serve as a useful reference for future work.

Specific scientific issues:

1) At the end of the Introduction the author mentions 3 previous studies, but does not discuss the photochemical results in Johnson et al., (Isotopic composition of stratospheric water vapor: Measurements and photochemistry; hereafter referred to as 2001a) even though this reference is cited elsewhere in the text. Although the model presented by Johnson et al. is much simpler than that presented here the results are relevant because they compare well with the measurements presented in the same paper.

2) Section 3, second paragraph: The author assumes that the vapor pressure isotope effect will produce mass dependent isotope fractionation in H2O, so that the depletion in ^{17}O will be just 52% of the depletion in ^{18}O. However, Jancso and Van Hook (1974) observed a ratio of 0.564 between 313 and 363 K. This agrees well with the data of Johnson et al. (2001a) above 21 km, although the measurement precision is not sufficient to distinguish between a slope of 0.52 and 0.564.

3) Section 4.5, second paragraph: Johnson et al. (Isotopic composition of stratospheric ozone, JGR 105, 9025-9031, 2000) was the first to point out that stratospheric ozone isotope measurements were in agreement with reaction rates as measured in the laboratory, with the exception of a few early measurements that were then discounted by Mauersberger et al. (2001). Johnson et al. also confirm that the asymmetric form of ^{50}O3 is enriched more than the symmetric form.

4) Section 5.2, second paragraph: The only data shown in Figure 3 are the old measurements published by Rinsland et al. The data presented by Johnson et al (2001a) have systematic errors of less than 40 per mil, and the binned data have precision

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considerable higher than that. Rather than compare measured and modeled altitude profiles it may be more meaningful to compare correlation plots such as δ_{18}(O) vs H2O, as done by Johnson et al., since this can correct (to first order, at least) for the effects of transport. In doing so, it also becomes possible to compare the model results presented here with the simple analytical expressions derived by Johnson et al. (2001a).

5) Section 6.2, first paragraph: The author should point out that the averages presented here are mass-weighted averages. While this is clear from the text, a causal reader may still interpret these numbers as the average value for a vertical profile, which is the typical meaning of "average value" in the remote sensing community.

6) Figure 2 caption: The solid circles are included in the figure legend, but not mentioned in the caption.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3991, 2003.