

Interactive comment on “Trend in ice moistening the stratosphere – constraints from isotope data of water and methane” by J. Notholt et al.

Anonymous Referee #3

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General comments

The authors use an interesting set of balloon MkIV FTIR solar occultation data to derive trends in isotopic ratio values (δD values) in water when entering the stratosphere, and from these trends in the transport of particulate water through the tropopause, for the period 1991-2007. The objective is to verify whether trends in the partitioning between water vapour and ice entering the stratosphere can explain (part of) the stratospheric water trends.

This is an interesting paper, in which the balloon data set is exploited in an original way. The paper is well written in general but a little careless as to figures and details (missing elements in the captions / legends, etc; see specific comments below). Also the reasonings are sometimes hard to follow since they are given only very briefly.

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Specific comments

- pg 16976, line 5 : the delta notation should be defined here
- pg 16976, line 21: delete 'at an altitude'
- pg 16977 line 20-21: have the micro-windows been fitted simultaneously or not ? it is not clear since the authors say 'individual results have been averaged'. Please clarify.
- Pg. 16978 eqs (3) and (4): the superscript 'tr' has not been defined. Then the question rises what the VMRs without superscript stand for exactly.
- Pg. 16979 line 11: add 'in' before '(Rahn et al., 2003)'
- Pg. 16981, line 4: the authors argue that the lack of detection in the ATMOS profiles may be due to their different vertical resolutions and accuracies: how do these characteristics compare between ATMOS and the presently used balloon data ? On the other hand, they mention clear evidence in MIPAS data which – I suspect – have about the same vertical resolution as ATMOS data. What about the characteristics of MIPAS versus ATMOS as to vertical resolution and accuracy? Are these parameters really the reason for the differences in detection?
- Section 4.1: I have a problem with the fact that this Section considers the results of Fig. 2 as 'short-term variations'. The Table in the supplement clearly shows that the data set spans several seasons but spread over different years. The authors used only 23 balloon profiles covering 17 years, so on average they do not even have 2 profiles per year. How then can you disentangle short- from long-term variations ? So don't we see a mixture of short- and long-term variations in Fig. 2?
- Pg. 16981, line 13: why would the contribution from isotopically heavy ice increase with increasing H₂O ?
- Pg. 16981, line 23: should read 'partitioning between vapour and ice' instead of 'partitioning between water and ice' ?

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- Pg. 16981, line 25: I guess that delta 'D = -300' should be 'delta D for ice= - 300' ? The units are missing.
- Pg. 16982 lines 3 and following: I don't understand how the conclusions in the summary follow from the paragraph just above it. The authors should be more explicit in their explanations. In fact, it is not clear how the last paragraph on pg. 16981 contributes to the findings in the summary at the start of pg. 16982.
- Pg. 16982, line 22: the delta value trend of 11.1 plus/min 12.3 per mille per decade is not statistically significant, and neither is the trend calculated in Fig. 3 (black line) so can we conclude anything?
- Pg. 16984, line 21: Should read 'The changes in the calculated amount of particulate water are within the uncertainty. ...'
- Fig. 2: vertical axis of middle plot not fully readable; legend not explained in the caption and not fully comprehensive
- Fig. 3: I don't see the black symbols in plot (c). Open and closed circles in plots (a) and (b) are not identified. The first sentence in the caption is not complete ([HDO^{entry}] is missing)
- Vapor' and 'vapour' are both appearing in the manuscript: English or US spelling should not be mixed.

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