

Interactive comment on “A reassessment of the discrepancies in the annual variation of $\delta\text{D-H}_2\text{O}$ in the tropical lower stratosphere between the MIPAS and ACE-FTS satellite data sets” by Stefan Lossow et al.

Anonymous Referee #1

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This very well done paper systematically explores the reasons behind the observed differences in δD variation in the lower stratosphere. All of the major contenders are considered including sampling, vertical resolution between instruments and between data products, and the previously known “start altitude effect” in MIPAS. The analysis is convincing and the treatment is thorough, using appropriate averaging kernels and an understanding of the retrievals without becoming overly technical (this is well balanced with supplementary figures in the appendix). It is a fine example of how careful handling of such satellite observations is extremely critical for interpretation. I do

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agree with the other reviewer that a rephrasing of the last sentence of the abstract is required. Aside from some very minor suggestions for the authors to consider below, I recommend this paper be published in AMT.

P1, line 15: annual variation in the MIPAS data up to an altitude of 40 hPa is substantially impacted

P4, line 6: A focal point of the discussion is the MIPAS

P8, line 5: ACE-FTS data set systematically indicates an earlier occurrence

Is there qualitative rationale for requiring 20 observations per bin for MIPAS & SMR, and 5 per bin for ACE-FTS?

Consider showing the time series in Fig 1 on the same time axis. As it is, it is hard to interpret even though it is pointed out in the text.

Figure 6: would it be better to show these results as a function of month at a few pressure levels, rather than as profiles? It might be easier to link to the discussion in the text.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-309, 2019.

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