

# ***Interactive comment on* “The SPARC water vapour assessment II: Profile-to-profile and climatological comparisons of stratospheric $\delta D(H_2O)$ observations from satellite” by Charlotta Högberg et al.**

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Summary:

1) Does the paper address relevant scientific questions within the scope of ACP? Yes.  
2) Does the paper present novel concepts, ideas, tools, or data? Yes (new comparisons of available data sets).  
3) Are substantial conclusions reached? Qualitative; more quantitative conclusions desired, see explanation and more detailed comments below.  
4) Are the scientific methods and assumptions valid and clearly outlined? Yes,

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for the most part, see detailed comments below. 5) Are the results sufficient to support the interpretations and conclusions? See response to 3). 6) Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes. 7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes. 8) Does the title clearly reflect the contents of the paper? Yes. 9) Does the abstract provide a concise and complete summary? Yes. 10) Is the overall presentation well structured and clear? Yes. 11) Is the language fluent and precise? Yes. 12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes. 13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See specific comments below for minor clarification requests. 14) Are the number and quality of references appropriate? Yes. 15) Is the amount and quality of supplementary material appropriate? Yes.

General comments and overall impression: The authors have summarized a large body of work in comparing profiles of HDO, H<sub>2</sub>O, and the ratio made by 3 instruments (SMR, MIPAS, and ACE-FTS), with two retrieval versions each for MIPAS and ACE-FTS. I feel strongly that this work is worth publishing, but in my opinion (and others may reasonably disagree) it is also incomplete. While I provided many comments below, my intent is not to be overly critical, but to suggest additional content so as to increase the return from the effort that the authors have already put into this manuscript.

I believe that the primary reason that one compares measurements made by multiple instruments is to validate a priori estimates of measurement accuracy. This validation is imperfect, and the comparison cannot be used to estimate accuracy since the difference between measured values does not tell you if any one value is correct. However, although I may have missed it, I did not see a discussion of the retrieval accuracy for any of the three instruments, only precision, so I do not know if the differences that were discussed were consistent with estimates of individual biases. And in the end, there were so many reasons for differences in the profiles (differences in spectral databases,

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calibration errors, time/space coverage, and so on), that that I am not sure that I have learned anything about the absolute accuracy of the various measurements of H<sub>2</sub>O, HDO, and delta D.

I would ask the authors to: 1) Provide a quantitative discussion of sources of systematic error for HDO, H<sub>2</sub>O, and delta D (some errors that affect individual profiles will cancel in the ratio, others will not) for each instrument. Perhaps provide an error budget in a table for each molecule and instrument. Or, if the paper is already struggling with length, maybe put it into the supplement and at least include a rolled up estimate that can be compared to the observed differences between instruments. 2) Try to say something conclusive about what was learned from the comparisons about the quality of the data. Plausible qualitative explanations are provided for differences observed in various regions (at the end of the Conclusion, for example), but I still don't know which profile to believe.

An additional personal preference: I feel that the comparisons are further complicated by showing two retrieval versions each for MIPAS and ACE-FTS. I would prefer sticking with the latest release (not beta or test) version. A simple comparison between versions for each instrument might be called for if there is an extensive publication record for the older version and it is necessary to show the difference, but it would simplify things to show only 3 comparisons (3 pairs selected from a set of 3 data sets) instead of 10 (10 pairs selected from a set of 5 data sets).

Specific comments: page 11, line 5, and page 34, line 15: Although this information is probably in the references (and citations in those references), it would be useful to include some discussion of the specific differences in the spectral databases used for MIPAS and ACE-FTS (and SMR, for that matter). Specifically, whose line parameters (strengths, positions, and linewidths) are used for H<sub>2</sub>O and HDO? What are the uncertainties in the parameters for the lines used in the retrievals, and how does that affect the profiles?

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page 16, line 9-10: This would probably be obvious to most people, but it would have been helpful to me to clarify to me here that by "all available data" you meant (I assume) the full data sets for each instrument as described in section 2. As distinct from the subsets used for profile comparisons as listed in table 2.

page 30, lines 13-31: This was a section that I thought needed to be more quantitative. Sideband leakage is specified, but the bias this may cause in H<sub>2</sub>O is not quantified. Likewise, bias due to spectroscopic parameters is mentioned, but the parameters are not identified, the uncertainty in the parameters is not specified, and the effect on retrievals is not quantified. Having this additional information is very useful when trying to make sense of the difference between SMR and other sensors.

page 32, line 18: when the authors specified "homogeneous coverage in latitude and time", I was confused. A sun-sync orbit covers all latitudes but just 2 times (ascending and descending) at each latitude. Does time mean season, not time of day?

Figure 1, lower left panel (H<sub>2</sub>O bias): This figure confused me. Looking at around 30 hPa, we get SMR-MIPAS $\sim$ -1.4 ppmV, and SMR-ACE $\sim$ -0.7 ppmV. That would suggest that MIPAS-ACE $\sim$ +0.7 ppmV. But the figure shows more like  $\pm$ 0.2 ppmV (depending on the exact algorithm pair). However, Figure 7 shows MIPAS-ACE much closer to +0.7 ppmV, even though this is not a direct profile-profile comparison. Does this suggest that, for H<sub>2</sub>O anyway, the direct comparison of ACE and MIPAS is invalid due to insufficient data and poor coincidence?

Grammatical: page 6 of 55, line 9: should this read "climatological comparisons"?

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