**Atmospheric
Chemistry
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Discussions**

Interactive comment on "Daytime SABER/TIMED observations
of water vapor in the mesosphere:
retrieval approach and first results" *by*
A. G. Feofilov et al.

Anonymous Referee #2

Received and published: 4 August 2009

This manuscript provides the first quantitative discussion of mesospheric water vapor retrievals from the SABER instrument launched on the NASA/TIMED satellite in 2001. The scientific community has waited a long time for this potentially valuable dataset, and the authors provide a highly relevant first look at the retrieval algorithm with some early results. The methodology for the retrievals is complex, and consideration of non-local thermodynamic equilibrium (non-LTE) processes is critical. The authors are well aware of this and not only identify the key reactions that affect the $H_2O(v_2)$ populations but find the best reaction rates that deliver the most robust solution. They therefore

provide not only early results from H2O retrievals but also set a standard for future laboratory studies. In addition, the manuscript is structured well and is well written with a few minor problems. The reviewer therefore finds this work to be of substantial value, even though only a small subset of the data is presented. Ultimately, the large quantity of data, the high vertical resolution and the observations over nearly an entire solar cycle will all be of great value to the community.

The reviewer nonetheless has a few major concerns which center primarily on the clarity of the presentation rather than the scientific methods, which appear sound. The reviewer also has some more technical minor concerns listed subsequently.

Major comments:

1) Section 5.2. There is a discussion here on sensitivity of the retrieval to local temperature, which appears to be an important effect. The authors introduce two artificial adjustments to a temperature profile to assess the impact. The reviewer suggests a figure showing the temperature profile for one case and the two test profiles overplotted. This figure could be "Figure 7a" and should be accompanied by one or two sentences describing why the profile was adjusted in this way. Figure 7b could be a comparison between the ACE-FTS temperature and SABER temperature measurements with the adjustment discussed on p. 13962. This figure is just conceptual, but the reviewer strongly suggests a figure of temperature profiles added to support the temperature sensitivity discussion.

2) Table 5. It is difficult for the reviewer to study this table and decide on the quality of the agreement between datasets. The vertical resolution of the ground-based H2O retrievals is much larger than that of SABER, so a direct comparison between the columns in this table may be misleading. The authors either need to convolve their data with the appropriate averaging kernels and include that result as another column or else include a discussion on the difference in vertical resolution between the datasets and how that may affect the comparison.

3) Figure 8. This is an important figure and very hard to read. It must be larger. It also might only be necessary to show every ppmv interval (or 0.5 ppmv) rather than every 0.25 ppmv. In addition, the contours need to be thicker for clarity.

4) Section 7, p. 13971, Fig. 9. Rather than show a ratio of radiances in Fig. 9c, the reader would like to know the change in H_2O mixing ratio. Can the authors include a panel which shows this? Similarly, it would be far more instructive in Section 8, line 21 to indicate the variation in mixing ratio rather than the difference in simulated radiance.

5) Section 7. At the end of this section there is a statement indicating that the "new" SABER retrievals will be better in the "next release" of SABER data. To the reviewer's knowledge, there is not presently a publicly released version of the SABER H_2O data, which leaves the reviewer confused. If there is a publicly released version of SABER H_2O the authors should say so and tell the reader where to get it. Whether there is a publicly released version or not, this manuscript has addressed several different issues that impact the H_2O retrievals using multiple non-LTE models. It would be very useful at the end of this section to 1) identify the version of SABER H_2O released or to be released, 2) state the non-LTE code used in the retrieval (presumably SOPC) and how well it agrees with other non-LTE models, 3) indicate explicitly what reaction rates were fixed in the SABER H_2O retrievals, 4) indicate whether SABER temperatures were adjusted in this version of the H_2O retrievals and whether these adjusted temperatures will or will not be in the database and 5) indicate to the reader what improvements will be made in future releases of the SABER H_2O data. Any other relevant information that the authors can add for the interested researcher would be much appreciated. All this in a summary paragraph at the end of this section would improve the traceability of the results. Thank you.

6) Section 8, p. 13972, lines 5-6. Did the authors only have to vary the rate coefficients to achieve good agreement with other instruments and models (see comment #1)? Section 6.1, p. 13966, lines 15-16 indicate that the temperature profiles were modified as well. Please be explicit. Thank you.

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Minor comments:

1) Abstract, p. 13945, line 9. "We analyze" and "suggest" (line 11) is more accurate than "The paper analyzes" and "suggests".

2) Introduction, p. 13946, line 7. The Zasetsky et al. reference addresses homogeneous nucleation, which is controversial and only potentially relevant for extremely cold conditions in the polar summer. Perhaps a more general reference discussing possible nucleation processes in the polar summer mesosphere may be more appropriate [e.g. Rapp and Thomas, JASTP, 68, 715, 2006]?

3) Introduction, p. 13946, line 12. The idea that NLC are indicators of climate change is interesting but is not shared by all [e.g. von Zahn, EOS, 84, 261, 2003]. There is furthermore no direct evidence making this climate change connection. The reviewer suggests the word "possible" before "indicators".

4) Introduction,. p. 13946, line 15. The first study to make the connection between space shuttle exhaust and NLCs was Stevens et al. [GRL, 30, doi:10.1029/2003GL017249, 2003].

5) Introduction, p. 13947, line 6. Missing the word "the" after "providing".

6) Section 3, p. 13949, line 2. Comma missing after "gas phase".

7) Section 3, p. 13949, lines 8-11 and Fig. 1. It would be useful to the reader to somehow highlight the 6.6 micron transition in Fig. 1.

8) Section 3.2, p. 13950, lines 20-21. This sentence is not clear to the reviewer, if the daytime model includes the nighttime model, why do the authors not present nighttime data? Please reword for clarity.

9) Table 1 is difficult to read. Please enlarge. Thank you.

10) Section 5.6, p. 13966, line 2. Missing the word "the" before H2O.

11) Section 6.1, p. 13967, lines 23-27. The reviewer is confused. The authors advertise better agreement with a better SABER temperature retrieval, but weren't the profiles already modified assuming a SABER temperature bias (p. 13966, lines 15-16)? Please clarify. Thank you.

12) Section 7, p. 13970, line 23. Based on the approach outlined by the authors in this manuscript, the reviewer believes that "inferred from" is more accurate than "found in".

13) Section 8, p. 13971, line 17. Missing "the" before "H2O".

14) Section 8, p. 13971, line 23. The reviewer suggests "inferred" instead of "identified" (see comment #12).

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 13943, 2009.