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Comment

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

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Received and published: 29 September 2009

We would like to thank Reviewer#1 for his/her comments on the paper. The responses to reviewer's comments are given below. We marked the reviewer's and the author's comments by “RC:” and “AC:”, respectively.

1) **RC:** “The abstract should contain the following information: quantitative errors, and the reason for concentrating on daytime observations.”

AC: The quantitative error values have been added to the abstract. The reason for

concentrating on daytime retrievals has been mentioned (see also the response to the minor comment #8 by Reviewer#2).

2) **RC:** "Because the rate coefficients are 'tuned' to allow best agreement with ACE data, there is the nagging question as to whether possible temperature dependence of the three coefficients is relevant, and whether the three values obtained by the chi-squared analysis are indeed unique. . . . Presumably the temperature dependence of the reactions is buried in the overall numbers because the comparisons were made under a number of different thermal conditions."

AC: The question about uniqueness of the solution is the correct one. However, the answer to it is implicitly embedded to Section 5.4 "SABER H₂O Validation". The confidence region introduced in lines 2-4, page 22 implies that the values obtained in chi-square plot analysis do have uncertainties. These uncertainties are assigned to the corresponding rates coefficients that makes the set non-unique. However, Fig. 7 shows that taking the rate values that are beyond the limits given by formulas (1)-(3) on page 22 makes the retrieval to disagree with ACE-FTS measurements. In principle, one may try to verify the rate coefficient temperature dependencies using the methodology described in the paper if the number of nearly coincident common volume measurements is sufficient to build a good statistics on temperature values at various altitudes. Since each rate can have its own temperature dependence that will add another 3 dimensions to the already complicated chi-square plot and will hinder the analysis. Additionally, the temperature dependence of the reaction rates is usually weak, see Table 1. Summarizing, we do not consider the rate temperature dependence analysis possible for the number of overlapping events available at the moment.

3) **RC:** "During summer solstice at high latitude, I would have expected a hydration due to PMC sublimation near 80 km. I do not see this well-established feature in the plots, which should show up at the advertised resolution. However the plots are so small in my version of the paper, that it could easily have escaped my attention. It would be desirable for the journal to blow these plots up to something readable."

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AC: This comment is absolutely correct. The effects of freeze drying in the PMC formation area and hydration due to PMC particles sublimation below the cloud are expected and observed by the instruments that have sufficient vertical and temporal resolution. We have observed these effects with SABER and reported it recently at IAGA 2009 in Sopron, Hungary (202-THU-O1100-0939. A.G. Feofilov, S.V. Petelina, A.A. Kutepov, W.D. Pesnell and R.A. Goldberg, "Water vapor, temperature, and ice particles in polar mesosphere as measured by SABER/TIMED and OSIRIS/ODIN instruments"). The analysis made in this work shows that the freeze drying and hydration features change rapidly and, therefore, they are not always present on the atmospheric "snapshots" like that shown in Fig. 8. The feature that resembles the hydration can be seen at 80N, ~78 km altitude in Fig. 8b. Detailed investigation of water vapor behavior in summer polar mesosphere will be made elsewhere. Figure 8 has been modified for better readability in accordance with the reviewer's suggestions.

4) **RC:** "The reference by Zasetsky on ice particle nucleation is not appropriate, since that reference proposes a speculative mechanism that may or may not be operating at low temperature, and even if it is legitimate, probably only operates in the coldest regions of the summertime mesopause. A better reference to the classical (heterogeneous) nucleation is Keese, R. G., 1989, Nucleation and particle formation in the upper atmosphere, *J. Geophys. Res.*, 94, 14,63–14,692."

AC: The reference was changed to newer paper by "Rapp, M. and Thomas, G.: Modeling the microphysics of mesospheric ice particles: Assessment of current capabilities and basic sensitivities, *J. Atmos. Sol.-Terr. Phys.*, 68(7), 715–744, 2006" that also includes the references to the paper of Keese, 1989 suggested by Reviewer#1.

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

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